Assyria and the West

A Fresh Look at the Unshakeable Pillars of Late Bronze and Iron Age Chronology in the Eastern Mediterranean World

Edited by
Pieter van der Veen
Ronald Wallenfels
Peter James†

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Dedicated to the memory of

Oscar White Muscarella (1931–2022)

archaeologist, rebel, sceptic, critic and low dater

R.I.P. Oscar!

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Preface and Acknowledgements

We are happy to present to you the conference Proceedings of the Fourth 'Bronze to Iron Age Chronology of the Ancient Near East' (BICANE) colloquium, held in 2021 as a Webinar, some ten years after our Third BICANE colloquium (held at Sidney Sussex College in Cambridge). Unfortunately, the publication of this volume has been delayed by other obligations and extended illnesses on our part.

Already the realisation of the colloquium itself was plagued by several challenges, foremost among them the Covid-19 pandemic. While we had originally planned to have the meeting at the new premises of the Palestine Exploration Fund (PEF) by the Thames in Greenwich (London), we were finally forced to turn the event into a Zoom-Seminar. This despite many previous months of organisation, mainly by Dr David Ellis of Cambridge Science and Archaeology Forum and his team (CSAF) and Felicity Cobbing (executive secretary of the PEF), for which and to whom we remain grateful. Further, we are particularly thankful that David Ellis and his team (especially Adrian Umpleby) provided their impressive technical expertise when the Webinar proved to be the only viable option short of indefinite postponement. We are also grateful for the financial support we received from the boards of CSAF and Wort und Wissen during the editing process. Without their support this volume would not have seen the light of day. We also wish to thank the authors for taking the time to research and to write up their articles and, most especially, for their patience with the editors. A warm thank you also goes to those lecturers, who did not submit an article, but participated in the discussions at the conference and shared their views on related subjects.[1] We also thank Archaeopress (Oxford) for kindly accepting the proceedings – especially so David Davison and Mike Schurer who saw us through the process – and Friedrun van der Veen and Johannes Weiss for assisting with the editing, layout, and typesetting of the volume.

With the help of all these and other people (not mentioned here by name) we now can present the reader with a solid contribution to the topic of the colloquium 'Assyria and the West' (as related to the 'Dark Age' around the turn of the second to first millennium BC) that has plagued the archaeology of the Eastern Mediterranean and beyond for so many years. Since the publication of *Centuries of Darkness* in 1991 this subject has continued to intrigue many scholars. The articles in this book bear clear witness to that. We hope that this book will contribute meaningfully to the ongoing discussions in the field and will stir further interest in the subject matter, hoping that it will solve some of the remaining enigmas in Ancient World Chronology. Even so, the alternative scenarios outlined in this volume will remain mere working hypotheses.

Peter James†, Pieter van der Veen,
and Ronald Wallenfels
(the editors) Summer 2024

[1] Dr David Ellis (CSAF), 'Towards Consilience in the Chronology of the Ancient Near East'; Dr Alexander Fantalkin (Tel Aviv University), 'Tell Qudadi and Iron Age Chronology of the Southern Levant'; Dr Norma Franklin (Haifa University), 'Construction, Correlation and Chronology. Megiddo: from an Omride city to an Assyrian regional capital'; Dr Takayoshi Oshima (Leipzig University), 'The Cuneiform Scribes in the Southern Levant in the Sargonid Period'; Dr Johanna Regev (Israel Antiquities Authority), 'The Microarchaeological Radiocarbon Approach to Building the Iron Age Chronology in Jerusalem'; Dr Willemijn Waal (Leiden University and NINO), '"The Dark Age" and the Greek Alphabet'; Henry Zemel (CAENO Foundation), 'Astronomy is the Arbiter of Chronology'. The abstracts of their lectures are found in the colloquium booklet, available on https://uni-mainz.academia.edu/PetervanderVeen/Drafts.

Reader's Note

A completely consistent rendering of the names of people and places from among the widely different ethno-linguistic realms across the ancient Near East whose languages and scripts are only imperfectly understood by modern scholars – themselves of widely differing linguistic backgrounds – in a modern Roman-based script without resorting to specialized individual characters or potentially ambiguous digraphs is difficult. However, in a synthetic work such as this, some measure of consistency among the presentations seems a worthy goal. To that end, the editors have suggested to the contributors conventions for normalizing given foreign names guided by, e.g., Brinkman (1977) for Assyrian and Babylonian names, Kitchen (2009) for Egyptian names, Bryce (2005) for Hittite and Luwian names, Potts (2016) for Elamite names, and RSV for biblical names. These have been variously adopted by the contributors as suited their individual needs and preferences.

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A General Introduction: 'Assyria and the West'

Peter James†, Pieter van der Veen and Ronald Wallenfels (eds)

Some seventy years ago George Hanfmann (a true 'great' in the fields of both classical and Near Eastern archaeology) wrote that:

> … a word of caution must be added regarding the so-called 'absolute' dates. Because the Near East is so vital for the chronologies of the European, Central Asiatic, and possibly even Far Eastern areas, it is well to remind ourselves from time to time that the two great pillars of the chronology of the Bronze Age, the Egyptian and the Mesopotamian, are not two stout towers resting on immovable foundations. They may rather be likened to two buoys linked by a chain and anchored, to be sure, yet raised or lowered by the waves of the sea. Both chronologies include problems which cannot as yet be solved except by reasonable guesses – the specific years to which observed astronomic data should be assigned, the estimates for the lengths of obscure periods, and the evaluation of possible gaps, duplications, and exaggerations in Royal lists and building inscriptions.[1]

Somewhat overlooked, Hanfmann's caveats should have come into even greater focus in recent years. His first 'great pillar', the standard chronology of Egypt, has come under ever increasing scrutiny, particularly with respect to the 'Third Intermediate Period Egypt' (TIP) which separates the firm ground of Saite 26th Dynasty history (664–525 BC) from the New Kingdom.[2] Claims that secure dates for the Egyptian New Kingdom may be arrived at by 'dead reckoning'[3] backwards through the TIP are demonstrably fallacious.[4] There remains a disconcerting amount of reliance on the fragments of Manetho,[5] while the alleged methods of astronomically dating Egyptian history – both lunar and Sothic – have been realised to be far less reliable than once believed.[6]

Hence scholars now point increasingly to synchronisms with Mesopotamia as confirmation of the Sothic-derived Egyptian dates.[7] While Egypt and the southern Levant were at the core of our third BICANE colloquium (2011) and its subsequent proceedings (2015), the chronology of Assyria and the West is the focus of the fourth BICANE Proceedings, based on the conference held in 2021.

Assyria is clearly linked to other great cultures of the ancient Middle East, including the lands of the pharaohs, the Kassites of Babylonia and the Elamites, the Hittites and 'Neo-Hittites', as well as other local cultures of Syria, Cis- and Transjordan. They involve links between the 18th–19th Dynasty pharaohs and various Middle Assyrian and Kassite kings. The question must be asked, however, how valid those synchronisms really are. And how reliable is the Assyrian King List in general, which essentially provides Hanfmann's second 'pillar' for Bronze Age chronology? Does Mesopotamian history, based solely on native and contemporary evidence, provide confirmation of the standard Egyptian-based chronologies of the Ancient Near East or are we dealing with circular arguments here?

In many respects, Mesopotamian chronology seems antagonistic rather than complementary to the Egyptian. Throughout the Late Bronze Age to the late Iron there exist tensions between the two in regions all the way from Anatolia to the Negev of Israel. From the Amarna letters we can see that some Syro-Canaanite rulers were using an Assyrianising variety of Akkadian,[8] which seems premature: according to the

[1] Hanfmann (1951, 361). He was reviewing Claude Schaeffer's monumental *Stratigraphie comparée et Chronologie de l'Asie Occidentale (IIIe–IIe Millénaires), Syrie, Palestine, Asie Mineure, Chypre, Perse et Caucase* (Oxford University Press, 1948).

[2] See e.g. James *et al.* (1987; 1991a; 1991b; 1992; 1998); Morkot (2017); Morkot and James (2009); James and Morkot (2010); Morkot and James (2015); James (2017a, 2017b; 2021); Hagens (1996; 1999); Dodson (1993; 2012; 2013; 2015); Porter (2008); Thijs (2015); Wallenfels (2019).

[3] Kitchen (2007b, 166–167). Kitchen's reasoning is uncritically accepted by most Egyptologists. For a conspicuous example, see Shortland (2005).

[4] See James *et al.* (1998); Morkot and James (2015); Thijs (2015).

[5] James and Morkot (2013, esp. 221–222). 'Manetho' gives us an invaluable glimpse into Hellenistic chronography (Kokkinos 2015; Dillery 2015) and, crucially, the development of modern views of Aegean chronology (Kokkinos 2009, esp. 245–46) but should not be used for reconstructing pre-26th Dynasty history.

[6] Wells (1996); O'Mara (2003); Kitchen (2002, 11; 2007, 164).

[7] E.g. Spalinger (2002, 248); Depuydt (2005, 32; 2007, 60–69); Wiener (2006, 326–327); Krauss and Warburton (2006, 477); Kitchen (2007b); Dodson (2012, xi, 181–189); cf. James (2017a, 337–338); Dodson (2013); Aston (2013, 307–309).

[8] 'Certainly the most striking feature of the Jerusalem scribe's language, though so far it has not been recognized, is its large Assyrian component' (Moran 2003, 265). For details of the anachronistic use of Assyrian script in the west in general, see von Soden (1986).

standard model, Assyria *c.* 1350 BC being only a nascent international state, still to make any military or political impact on the Levant or even on western Mesopotamia. It is Egypt, again, that provides the dates for the empire of Hatti based at Hattusa (Boghazköy), while Assyria controls those of the 'Neo-Hittite' kingdoms which succeeded it at Carchemish, Malatya, Aleppo, Hamath and Unqi in northern Syria during the Iron Age. Here, Imperial Hittite styles (in nomenclature and orthography) come into direct conflict with indications from Assyrian-dated artistic sequences. Despite the recent re-excavation of Carchemish on the Euphrates, the seat of a cadet branch of the Hittite imperial family, the question is still being asked:

> Can we bridge the gap between Suppiluliuma II and the Suhi-Katuwa dynasty, a period of 200–300 years? ... the present picture given in various contributions that more or less eliminate the previously postulated gap of the Dark Ages is overly optimistic ...'[9]

In southern Anatolia the rock carvings of Hartapu at Kızıldağ-Karadağ have provoked much head-scratching since their formal publication in 1992:

> ... there proves to be a chronological gap of about 300 years between the paleographical and the art-historical datings of the Hartapu monuments, which to this day remains without a satisfactory explanation. As an attempt, it was suggested that, while the inscriptions date to the late 2nd millennium BC, the figure of the king has been added later.'[10]

The problem is acute (James 2021–2022). Further south, at Byblos on the Lebanese coast, links with Neo-Assyrian art have raised a long-standing mystery concerning its royal inscriptions. Whereas these would argue for the 9th–8th centuries BC, contemporary Egyptian finds push them back into the 10th century and earlier.[11] The problem bleeds over into the Aegean where the adoption of an '11th-century' Phoenician alphabet by the Greeks in inscriptions no earlier than the 8th century, still provokes controversy.[12] In the far south, a 'gap' is apparent in the Negev between locally dated pottery, ultimately reliant on Mesopotamian links, and earlier Iron Age wares dated by pharaonic inscriptions.[13]

Between these two regions, Israel remains the focal point of one of the most heated debates in archaeology. Based on her conclusions from the palace of Omri-Ahab at Samaria, British archaeologist Kathleen Kenyon dated the contemporary buildings of Megiddo Stratum IV (since referred to as VA/IVB) with the same pottery and architecture to the 9th century BC. This was against the American school of George E. Wright and William F. Albright, who held that they were from the 10th century BC.[14] Their argument was that the close similarity of the six-chambered gateways of Iron IIA Megiddo, Hazor and Gezer meant they were designed by the same architect, a fact allegedly echoed in the biblical verse stating that Solomon built at those three cities (1 Kings 9:15). A Kenyon-style low model has been famously promoted (without any due credit) by Israel Finkelstein in the so-called Tel Aviv 'Low Chronology'.[15] But a corollary of the highly controversial Finkelstein version is a lengthening of the time during which the Philistine Monochrome and Bichrome wares were current – and, if Iron IIA began with the Omride Dynasty, a stretching of the Iron I period from 200 years to an implausible 300–350.[16] With the end of the LBA set at 1200/1175 BC by Egyptian chronology and the late Iron Age set by epigraphic and Neo-Assyrian evidence, the appearance of a clear 'Dark Age' in Israel, to match those in Anatolia, the Aegean and the Near East, is a logical fallout. As a chronological artefact this should have always been there but was masked by the very belief that Solomon belonged to the Iron IIA period and that this began c. 1000 BC.[17]

Albright, the last great polymath in the field, saw the onset of such a long Dark Age and the interconnectedness of many of the problems summarised here – exemplified by the title of one of his vintage papers: 'Was the Age of Solomon without Monumental Art?'[18] Albright accused the then leading expert on ancient Near Eastern art, Henri Frankfort, of having unconsciously conducted 'what amounts to a systematic campaign to discredit the entire Solomonic building tradition by the simple expedient of denying the existence of art or architecture in Greater Syria between ca. 1200 and 850 B.C.' Frankfort had, for example, insisted on a 13th-century date for the 'Neo-Hittite' sculptures at Malatya

[9] Aro (2013, 246).

[10] Oreshko (2017, 48).

[11] Wallenfels (1983; 2019, 496–498); James *et al.* (1991a: 250–251, 276–277, 389, nn. 53–54); James (2008, 153–154); van der Veen (2015); more half-heartedly, Sass (2005).

[12] James *et al.* (1991a, 81–85; 1998, 29); Papadopoulos (2016); Waal (2018; 2019).

[13] Bimson and Tebes (2009).

[14] Kenyon (1970, 248–250, 268-271; but cf. 346–347). For a restatement of Kenyon's position at the last BICANE colloquium, see Chapman (2015).

[15] With the notable exception of Wightman (1990), James *et al.* (1991a; 1991b; 1992, 130) carried a lone flag for the Kenyonite position until Finkelstein began to resurrect the same basic arguments in 1996. For brief discussion, see James *et al.* (1998, 41, 'Postscript'); James (2008); van der Veen (2020, 65–66).

[16] Perhaps because he has felt the weight of this problem, Finkelstein, in an astonishing *volte face*, seems to have announced in 2018 that the Megiddo gateway *is* Solomonic after all!

[17] See Kokkinos (2009, 51, n. 50).

[18] Albright (1958).

– and, at the other end of the divide, the ('archaising') sculptures from Aramaean Gozan (Tell Halaf) to the mid-9th century BC. Albright was also concerned about 'the drastic lowering of the Iron Age chronology of Cyprus by Einar Gjerstad, which would logically carry with it similar lowering of dates in adjacent Syria, Palestine and Cilicia.'[19] Albright sought to solve the problem by nudging finds into the gap. Where Frankfort dated the sarcophagus of Ahiram at Byblos to before 1200 BC, Albright moved it into the tenth century BC, still too high for comparanda from Neo-Assyrian art but not the conventionally dated Egyptian evidence.[20] Eric Cline's recent claim, that this 'Dark Age' was rather 'a new age with new inventions and new opportunities, as well as failures, in an age of chaos and reconfiguration' cannot gloss over the fact that we are still dealing with troubling anachronisms, suggesting that we are in fact dealing with what appears to be a modern construct.[21]

Rather, as *idée fixe* the LBA/IA transition set at c. 1200 BC has also allowed a conceptual barrier to develop in our understanding of the origin and identity of various Levantine peoples. Several decades ago, it was thought that the Neo-Hittite states arose because of a migration of Luwian-speaking 'Hieroglyphic Hittites' who, pushed from Cilicia by the equally nebulous 'Sea Peoples', overran Syria to maintain a 'strange afterglow' of Imperial culture for several centuries.[22] Similarly old textbook assumptions that the Aramaeans, Philistines, Phoenicians and Israelites all somehow 'arrived' *c.* 1200 BC still influence our preconceptions. Earlier references to Aramaeans (cuneiform records), Philistines (Hebrew Bible), Tjekker and Sherden (Egyptian records), Phoenicians (Egyptian Fenkhu) and Israel, even Asher and other tribes (Egyptian records),[23] let alone Ḫabiru (cuneiform), are still brushed aside as awkward curiosities. Other notions that still infest the literature are the ideas that cremation burial came onto the scene at the same time, and that bronze manufacture was miraculously replaced by ironworking with the advent of the Iron Age. (In Near Eastern terms it is at least a convenient though nicely misleading ceramic term.[24]) Sometimes a single blow can smash through the conceptual barriers of what might be termed '1200ism'. The prevailing opinion that the Mycenaeans could not have been Greek was proved wrong, flatly, by the relative 'outsider' Michael Ventris when he deciphered Linear B in the early 1950s.

An option, now long discussed, is to simply lower the start of the Iron Age along with a revision of Egyptian Third Intermediate Period chronology – and there is much evidence from Mesopotamia that is in step. The Levant, the interface between the two areas is of key importance here due to its rich archaeology, and one strategy to resolve the complex issues involved is to work from the known to the unknown. Invasions of Mesopotamian conquerors such as Nabonidus, Nebuchadrezzar II, Sennacherib, Sargon II and Tiglath-pileser III (as well as Saite and Kushite pharaohs) have provided plausible links with the destruction levels of key cities such as Ekron, Lachish, Samaria and Megiddo, though some of these possibilities need closer re-examination.[25] Late Assyrian and post-Late Assyrian imports (glazed vessels, seals, fibulae, weights and other small finds) found *in situ* are of primary importance.[26] The same is of course true of Egyptian finds.[27] Imitations of ceramic styles such as 'Assyrian Palace Ware' and other Assyrianising styles in Cis- and Transjordan need careful consideration with respect to the time of their production in the Mesopotamian homeland and the adoption of styles in the distant peripheries of the empire.[28]

The stable chronologies of Assyria post 910 BC and Babylonia post 747 BC are also, of course, our best handle not only on the 'mysterious numbers' of the Hebrew kings but those of Tyre and other Phoenician, Philistine, Transjordanian and Syrian kingdoms.

The contributions in this volume deal with several of these important chronological aspects. We have also included a section on 'science-based' dating methods (radiocarbon dating, astronomical dating and DNA analyses). We are told by practitioners of these methods that they can confirm or refine the standard chronologies for the Bronze and Iron Ages. But have they really proved to be the *deus ex machina* that historians once dreamt of?[29]

[19] Kenyon (in Crowfoot, Crowfoot and Kenyon 1957, 196) invoked Gjerstad's chronology for the Black-on-Red pottery from Samaria to support her 9th-century Omride date for the construction of its palace; cf. Tappy (1992, 126–132); James *et al.* (1998, 31).

[20] See Wallenfels (2019, 496–498).

[21] Cline (2024, jacket cover).

[22] See the magisterial handbook of Gurney (1954, 39–40, 129–130); and that of Roux (1966, 245); earlier, see e.g. Bonfante and Gelb (1944, 171–172).

[23] For an apparent pre-Merenptah reference to Israel in an 18th or early 19th Dynasty toponym list, see Zwickel and van der Veen (2017). For other possible references to tribal names in Egyptian topographical lists, see van der Veen (2022).

[24] See Chapman (1989 and 1990) for discussion and a suggested reform of the terminology for the 'three age system' as used in the Levant.

[25] Bagg (2011); James (2004; 2005; 2006; 2008; 2015b); Franklin (2018a; 2018b); van der Veen (2020).

[26] Fantalkin and Tal (2015); van der Veen (2020).

[27] Knauf (2002); Fantalkin (2001); James (2008, 147, 156, 164); van der Veen (2020).

[28] Hausleiter and Reiche (1999); Na'aman and Thareani-Sussely (2006); Singer-Avitz (2007); van der Veen (2014); Hunt (2015); van der Veen (2020).

[29] For some critiques, see Keenan (2002); James (2012; 2013); Porter (2015); Zerbst and van der Veen (2015).

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Obituary

Peter J. James, 1952–2024

Peter James gained a BA Honours degree in Ancient History and Archaeology at Birmingham University, specialising in the archaeology of Mesopotamia, Anatolia and Syria/Palestine. He followed this with postgraduate research in Ancient History at London University. In 1976 he joined the core group of the Society for Interdisciplinary Studies (founded the previous year as the Interdisciplinary Studies Group) and was appointed History Editor for its journal, the *SIS Review*. The purpose of the SIS and its journal was to promote academic discussion of the theories of Immanuel Velikovsky in the areas of catastrophism and revised chronology. Peter also became a Senior Editor of the American publication *KRONOS*, another journal devoted to Velikovsky's theories. For both journals he produced many important articles in the area of ancient history, and also displayed an impressive knowledge of ancient astronomy and mythology.

Although it was chiefly through reading Velikovsky that Peter became convinced the chronology of the Ancient Near East was flawed, he was far from uncritical of Velikovsky's views. As the following example shows, he was diligent in checking the soundness of Velikovsky's arguments. In *Ages in Chaos, Volume 1: From the Exodus to King Akhnaton* (London: Sidgwick & Jackson, 1953) Velikovsky pointed out that the Septuagint version of 1 Kings 12 contains extra information on the time Jeroboam spent in Egypt at the court of King Shishak; it states that while there he was given Ano, sister of the king's wife, in marriage. Velikovsky tells us that the name Ano is known from only one other source, namely a canopic jar dated on stylistic grounds to the time of Thutmose III. This seemed like convincing confirmation of Velikovsky's 500-year revision, in which Thutmose III is the biblical Shishak. Peter wrote a typically courteous letter to The Metropolitan Museum of Art in New York, to whose Egyptian Antiquities collection the jar belonged. He requested more information, citing the catalogue number given by Velikovsky. He discovered that the jar is actually from the time of Amenhotep III, one of many belonging to that king's family and harim.

In 1978 a conference on Velikovsky's theories was organized by the Department of Extra-Mural Studies and Adult Education at Glasgow University. Eight of ten papers delivered at the conference were related to Velikovsky's revision of ancient chronology; the remaining two were concerned with his cosmological theories. Peter gave a paper on 'Chronological Problems in the Archaeology of the Hittites'. He described the paper as 'of necessity, brief and sketchy', but in fact the level of detail was impressive. The paper presented a wealth of evidence for 'a radical lowering of the chronology for the Hittite Empire..., enabling it to overlap with the so-called "Iron Age Neo-Hittite" civilisation'. To that extent Velikovsky's revised chronology seemed vindicated. But Peter also made it clear that stratigraphical evidence from Anatolia and northern Syria entirely ruled out Velikovsky's separation of Egypt's 18th, 19th and 20th Dynasties. (Other conference papers, including the present writer's, agreed that these dynasties could not be separated.)

Later that year Peter returned to this problem in an extended review of Velikovsky's latest publication, *Ramesses II and His Time* (London: Sidgwick and Jackson, 1978, published too late for discussion at the Glasgow conference). Peter focused on detailed evidence that there could be no gap between the 18th and 19th Dynasties ('A Critique of "Ramses II and His Time"', *SISR* III/2, 1978, 48–55). He ended his critique as follows: 'Some tentative beginnings of an alternative "Glasgow Chronology" are offered in this issue by Geoffrey Gammon and John Bimson. Many problems remain... but I would like to urge other workers in this field, and Dr Velikovsky himself, to consider this alternative possibility for the continuation of Ages in Chaos.' Further discussion of the putative 'Glasgow Chronology' appeared in *SISR* IV/2–3 (1979/80), with Peter replying

to criticisms from John Day (who wrote from a conventional standpoint) and Lynn Rose (a defender of Velikovsky). Velikovsky died in 1979 without – so far as I'm aware – responding to the challenges which emerged from the Glasgow conference.

So far Peter had not rejected Velikovsky's dating of the 18th Dynasty, though he had argued for some adjustments. However, when the *Proceedings* of the Glasgow conference were finally published in 1982 (*SISR* VI/1–3), Peter added a telling Postscript to his paper: 'The writer would like to add that he now feels somewhat higher dates than those experimented with in this paper are required by the evidence.' For those of us who knew the thinking behind this cryptic sentence, it marked Peter's departure from Velikovsky's revision, and also from the 'Glasgow Chronology'. I and others soon followed his lead.

But Peter remained convinced that something was seriously amiss with the chronology of the Ancient Near East and that the conventional Egyptian chronology was the root of the problem. The next stop on his journey became known as the James/Rohl chronology, as it emerged from his collaboration with David Rohl. Together with others they experimented with less drastic reductions for the dates of Egypt's New Kingdom, but eventually there was a parting of the ways. While David Rohl favoured a reduction of *c.* 350 years, Peter became convinced that a reduction of *c.* 250 years was to be preferred. In collaboration with I. J. (Nick) Thorpe, Nikos Kokkinos, Robert Morkot and John Frankish, he produced *Centuries of Darkness* (London: Jonathan Cape, 1991). David Rohl's revision (which became known as the New Chronology) appeared four years later (*A Test of Time: The Bible – From Myth to History*, London: Century, 1995).

Peter was the lead author of *Centuries of Darkness* and also designed its striking and symbolic cover – an incomplete jigsaw puzzle based on one of the battle scenes from Tutankhamun's painted wooden chest. The book was soon translated into Spanish (Barcelona: Critica, 1993) and later into Greek with an Introduction by Nikos Kokkinos (Athens: Aiolos, 2006). Unfortunately Peter's cover design wasn't preserved in either case.

The initial reaction of conventional scholarship to *Centuries of Darkness* was predictably negative; Peter and his co-authors were kept busy responding to volleys of criticism from defenders of orthodoxy. Peter was always gracious and entertaining in debate and for the most part seemed to thrive on it. He only became irritated when a critic didn't take the trouble to understand the issues before going on the offensive.

Peter held firm to the *Centuries of Darkness* chronology for the remainder of his life, believing *c.* 250 years to be the maximum, and indeed the optimal, possible reduction from conventional dates. He continued to develop aspects of it in numerous papers, many of them published in mainstream academic journals – a testimony to the recognition he earned through his meticulous scholarship. An impression of his prolific output can be gained from the *Centuries of Darkness* website (https://www.centuries.co.uk – especially under 'The Continuing Debate: Replies to Critics and Further Research', and 'Recent Developments'); and see also https://bham.academia.edu/PeterJames.

By 2004 Peter, Pieter van der Veen and I felt more should be done to further discussion of revised chronology. The idea that emerged was to organise occasional conferences where scholars could discuss the pros and cons in an open-minded spirit. The first conference was held in Berlin (2006) and the second in Cambridge (2009). It was at this time that the discussion group would call itself BICANE. But it was not until the third meeting in 2011 (also in Cambridge) that publication plans originated, culminating in the first proceedings in 2015, edited by Peter James† and Pieter van der Veen. The conferences and proceedings have engaged scholarly discussion as Peter had wished and have advanced the initial thesis proposed in *Centuries of Darkness*. The present volume contains the latest fruits of that process.

Peter always described himself as a generalist, and indeed his interests were wide-ranging. He had a long-term fascination with the origin of Plato's account of Atlantis, and his evolving view on the subject eventually gave rise to *The Sunken Kingdom: The Atlantis Mystery Solved* (London: Jonathan Cape, 1995). His ability to encapsulate prodigious research in a popular style is clearly on display here, and also in the two books he co-authored with Nick Thorpe: *Ancient Inventions* (London: Michael O'Mara Books, 1995) and *Ancient Mysteries* (New York: Ballantine Books, 1999).

Our frequent e-mail exchanges ranged over many topics, including cosmology, vegetarian cookery (another subject on which he had thought of writing a book), growing raspberries and Doctor Who – a TV show we had both followed since the first episode in 1963 (he was no fan of the most recent series).

But above all Peter simply loved research and writing. He once told me he would need at least three lifetimes to achieve everything he wanted to do. I'm sure all who knew him are wishing his one lifetime had been a great deal longer. We have lost a dear friend, and the academic world has lost an original and brilliant scholar. Instances of 'James in prep.' in this volume and elsewhere will be a lasting reminder of his unfinished task.

John J. Bimson, 1st October 2024

SESSION 1

THE PRESENT STATE OF MESOPOTAMIAN AND HITTITE CHRONOLOGY: UNSOLVED ISSUES AND FRESH APPROACHES

An Introduction to Session One: 'Does Anybody Really Know What Time It Is?'[1]

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The earlier, conventionalized, history of the Ancient Near East at large is epitomized by the Third Edition of The Cambridge Ancient History (CAH)*, volumes I and II (Edwards, Gadd and Hammond 1970, 1971; Edwards* et al. *1973, 1975). Significantly, its history of the last three millennia before the Common Era is punctuated by three so-called Dark Ages. In light of more recently discovered evidence and further scholarly discussion, it may be argued that all three of these West Asian Dark Ages, in one way or another, are largely artefacts of the conventional chronological reconstruction, irreal exaggerations of otherwise genuine, but markedly briefer, transitional periods that require further serious scholarly (re)consideration.*

Introduction

The absolute chronology of the Iron Age in the ancient Near East is principally determined by Ptolemy's Royal Canon (Depuydt 1995). Its Babylonian segment establishes the onset of the first regnal year of king Nabonassaros, Nabû-nāṣir in Akkadian, on 26 February 747 BCE. This is the earliest secure date in world history directly linked to the Julian calendar. Nabonassaros' place at the top of that list may well reflect the Hellenistic Babylonian historian Berossos' statement that he, Nabonassaros, 'collected together and destroyed the records of the kings before him in order that the list of the Chaldean (that is, Babylonian) kings might begin with him' (*Babyloniaca* II.5.1; Burstein 1978, 22).

By cross-linking the Neo-Assyrian Eponym, or *līmu*, Lists (Millard 1994) with the Canon, the absolute chronology of neighbouring Assyria may be extended reliably back to the first regnal year of king Adad-nirari II in 910 or, less likely, 911 BCE.[2] This was accomplished by equating the solar eclipse noted during the eponymate of Bur-Saggile with that calculated from the Canon to have been observable in 763 BCE (Rawlinson 1867). On the basis of this Assyro-Babylonian chronological backbone, the earliest secure Julian-year dates in adjacent states may be determined. Thus, the reign of king Ahab of Israel ended in his 22nd regnal year in 853/52 BCE (Galil 1996, 34–37; cf. Thiele 1983, 94–95), while the reign of Egyptian king Psamtek I, founder of the 26th Dynasty, began in 664 BCE (Kitchen 2002, 5–6). All further proposed historical dates prior to these earliest secure dates, both for these states and for those of neighbouring regions whose chronologies are built upon them – Nubia, the Aegean, Cyprus, Syria, Anatolia, Elam – must all be understood to be modern conventions for which at present no further direct confirmation, scientific or otherwise, exists.

Dark Ages

The earlier, conventionalized, history of the Ancient Near East at large is epitomized by the Third Edition of *The Cambridge Ancient History* (*CAH*), volumes I and II (Edwards, Gadd and Hammond 1970, 1971; Edwards *et al.* 1973, 1975). Published between 1970 and 1975, the *CAH* remains the self-styled standard for the field. Significantly, its history of the last three millennia before the Common Era is punctuated by three so-called Dark Ages. These are poorly documented transitional periods of uncertain length, two within and one at the end of what in broad Levantine archaeological terms may be loosely called the Bronze Age (see *Figure 1*).

Over the decades, archaeological, art-historical, philological and scientific evidence has been introduced to argue for the status quo, or to lengthen or to

[1] Words and music by R. Lamm, *Chicago Transit Authority* (1969).

[2] Year 1 Adad-nirari (II) = 910 BCE (Millard 1994, 23, rejecting without comment the restoration of the eponymate of Ashur-raim-nisheshu for 903 BCE as proposed by Forrer 1929, 212–213, followed by, e.g. Ungnad 1938, 418; cf. Weidner 1939–1941, 313, r. Ashur-bel-kala).

	Early Bronze Age (*c.* 3000–*c.* 2200 BCE)	Middle Bronze Age (*c.* 2200–*c.* 1600 BCE)	Late Bronze Age (*c.* 1600–*c.* 1200 BCE)	Iron Age (*c.* 1200–*c.* 500 BCE)
Mesopotamia	· Sumerian Early Dynastic city-states · Trade with Indus Valley · Empire of Akkad · Collapse of Akkadian Empire · Guti invasion	· Uruk V/Lagash II/Ur III Dynasties · Amorite, Elamite invasions · Isin and Larsa Dynasties · Old Babylonian period · Sealand I Dynasty · Hittite invasion	· "Dark Age" · Kassite conquest · Middle Babylonian period · Middle Assyrian period	· "Dark Age" · Isin II/Sealand II/Bazi/Elam Dynasties · Neo-Assyrian period · Neo-Babylonian · Persian conquest
Anatolia	· Early Bronze Age city-states · Collapse of urban culture	· Old Assyrian colony period · Old Hittite period · Middle Hittite period	· Hittite Empire · Mycenaean incursions	· "Dark Age" · Kingdoms of Urarṭu, Phrygia, Lydia · Cimmerian invasion · Neo-Assyrian expansion · Persian conquest
North Syria & The Levant	· EBA city-states · Egyptian contacts · Ḫurrian kingdoms · Amorite expansion · Akkadians invade north Syria · Collapse of urban culture	· MBA regional kingdoms · Hittite expansion	· Ḫurrian kingdom of Mitanni · LBA regional kingdoms · Egyptian incursions	· "Dark Age" · Luwian/Aramaean/Canaanite regional kingdoms · Successive Neo-Assyrian, Neo-Babylonian and Persian conquests
Egypt	· Early Dynastic period · Old Kingdom · Collapse of unified kingdom	· First Intermediate Period · Middle Kingdom · Second Intermediate Period · Canaanite Dynasty in Delta · Great Hyksos conquest	· Expulsion of Great Hyksos · New Kingdom · "International Period" · Ramesside period · "Sea Peoples" invasion	· Third Intermediate Period · Libyan Dynasties · Nubian Dynasty · Neo-Assyrian invasions · Saite period · Persian conquest
Iran	· Proto-Elamite culture	· Old Elamite kingdom	· Sukkalmaḫ period · Middle Elamite kingdom	· Arrival of Indo-Aryans · Neo-Elamite kingdom · Neo-Assyrian invasion · Median kingdom · Rise of Persians
Aegean/Mediterranean	· Early Cycladic culture · Early Minoan culture on Crete · Early Helladic culture on mainland · Collapse of urban culture	· Middle Minoan civilization on Crete	· Eruption of Thera · Decline of Minoan civilization on Crete · Rise of Mycenaeans on mainland and conquest of Crete	· "Dark Age" · Geometric period · Archaic period · Greek colonization of Aegean, Italy and Sicily

Figure 1. A conventional timeline of the Ancient Near East (after Wallenfels 2000, I, xvi–xix).

shorten by up to several centuries the conventional chronologies for these dark periods. However, in light of more recently discovered evidence and on-going scholarly discussion, it would appear that all three of these ancient Near Eastern Dark Ages, in one way or another, are largely artefacts of the conventional chronological reconstruction, irreal exaggerations of otherwise genuine, but markedly briefer, transitional periods that require further serious scholarly (re) consideration. The following remarks are not intended to be exhaustive, but rather to demonstrate to the reader the sorts of anomalies noted by contemporary scholars in the conventional reconstruction of the history of the ancient Near East during the Bronze and early Iron Ages.

The Gutian Dark Age

Virtually coincident with the publication of *CAH*, W. W. Hallo (1971, 2005) argued from textual evidence for the reduction of the earliest of the three Dark Ages, the so-called Gutian Dark Age. This is the period between the collapse of the Akkadian Empire at the end of the reign of Shar-kali-sharri and the beginning of the reign of Ur-Namma, founder of the Third Dynasty of Ur. This period corresponds in Levantine archaeology with the transition from the Early to the Middle Bronze Age toward what is conventionally the end of 3rd millennium BCE. Specifically, Hallo posited lowering the length of the Gutian Dark Age from 117 years – more or less in line with the variants (91–125 years) of the so-called Sumerian King List – to certainly no more than four decades.[3] The Sumerian King List, or the Chronicle of the Single Monarchy, as termed by J.-J. Glassner (2004, 117–126), presents the pious myth of a single unbroken line of dynasties ruling Mesopotamia sequentially[4] since before the Flood 'when kingship came down from heaven'. Significantly, the contemporary native texts themselves not infrequently belie this conceit.

Thus, P. Steinkeller (1988), for example, has argued that the Second Dynasty of Lagash was contemporary with, not prior to, the early Ur III period.[5] Since there exists no real chronology for the Early Dynastic period[6] which immediately preceded the Akkadian period, neither proposals *per se* for shortening the Gutian Dark Age nor for telescoping the Lagash II and early Ur III dynasties have occasioned much controversy and today both appear to be well-accepted notions – although not in specific detail – by many scholars in the field.[7] More recently, B. Mahieu (2019) proposed that in fact the Gutian Dynasty – which she argues to be just 26 years long[8] – should in fact be removed entirely from the conventional consecutive chronology, being fully contemporary with the last years of the reign of Naram-Sin and the reign of his successor, Shar-kali-sharri, as were the pretenders to the throne of Akkad following the rebellion at the end of Naram-Sin's reign. Thus, in her opinion, just the seven-and-a-half-year long reign of Utu-hegal, the sole king of Uruk V, would separate Shar-kali-sharri from Ur-Namma.[9] The catastrophic collapse of the Akkadian Empire and of Early Bronze Age culture in the Near East at large, including Old Kingdom Egypt, due to wide-spread severe drought, is now broadly seen to be among the numerous manifestations of the abrupt globally-observed *c.* 4.2 kiloyear (ka) BP (*c.* 2150±150 BCE) climate event.[10] Although the Gutian Dark Age itself certainly now appears to have been short-lived, no more than a few decades at most, the impact of the *c.* 4.2 ka BP climate event, which produced vast numbers of climate refugees, especially from the traditionally dry-farming communities across north Syria, appears to have lasted several centuries (Kaniewski *et al.* 2015).

Dating the Fall of Babylon I

H. Gasche *et al.* (1998a, 1998b), per textual and archaeological evidence, have argued that the conventional Mesopotamian chronology for the first half of the 2nd millennium – the so-called Middle Chronology[11] – creates a century-long elongation of the transitional period between the Middle and the Late Bronze Ages. This period spans the decline and fall of the First Dynasty of Babylon, or the Old Babylonian period, the rise and fall of the First Dynasty of the Sealand and the rise of the Kassites.[12] Gasche *et al.* (1998a) contended that the similarities between the shapes of

[3] Cf. 70 years: Sallaberger and Schrakamp (2015, 113–130); cf. 100 years: Steinkeller (2015, 281–288).

[4] '[A]n idea that is manifestly a fiction, not a fact' (Charvát 2010, 20).

[5] Cf. Mahieu (2019); Mahieu (2021) now posits Ur-Ningirsu, founder of Lagash II, as a contemporary of Sargon of Akkad, and Ur-abba, the last ruler of Lagash II, as a contemporary of Ur-Namma, founder of Ur III. Contrast Wilke (2011), rejected by Suter (2012, 59–60).

[6] For the periodization of the Early Dynastic (ED) period, see now Evans (2007); for an examination of the evidence suggesting a chronology of the later ED I ('ED II') period, see Frayne (2009).

[7] Cf. Kuhrt (1995, 44–48).

[8] Cf. Visicato (2010, 451), 35–40 years.

[9] Mahieu (2020, 220), now suggests, although '[f]urther research is needed ... the figures in the king lists at least suggest contemporaneity between Ur-Namma and Utu-hegal.'

[10] See Weiss *et al.* (1993), and more recently, e.g. Staubwasser and Weiss (2006, 380–383).

[11] See Åström (1987–1989).

[12] The chronology of the Kassite period is to a significant degree dependent upon claimed synchronisms with New Kingdom Egypt, whose purported absolute chronology for the conventional 2nd mill. BCE is at the core of the present debate.

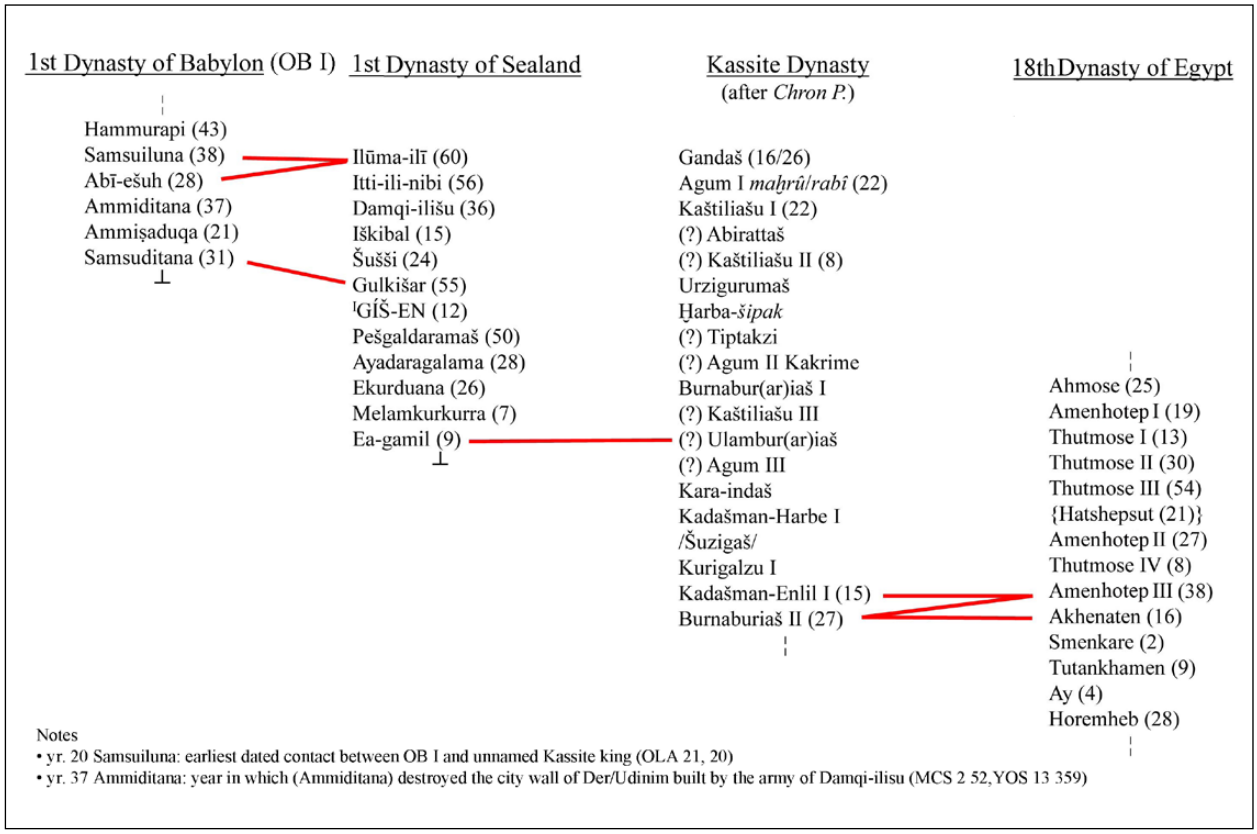

Figure 2. Secure synchronisms during the later MBA–earlier LBA.

pottery and the associated pottery-making techniques at the end of a four-hundred-year long sequence of continuous development prior to this Dark Age and at the beginning of such sequences of continuous development during the four centuries following it 'do not allow for the intervention of so great a span of time (1998a, 1) ... because we cannot justifiably stretch out the available archaeological material to fill the additional time mandated by those (conventional) systems' (1998a, 42). The authors offered a new lower absolute chronology based on lunar eclipse omens recorded in *Enūma Anu Enlil* (*EAE*) Tablets 20–21 (Rochberg-Halton 1988) referring to seemingly specific events in the history of the Ur III Dynasty. The conventional Middle, as well as the directly competing High and Low (or Short) chronologies relay rather on observations in the so-called Venus Tablet of Ammisaduqa in *EAE* 63 (Reiner 1975), whose data is considered by many to be hopelessly corrupt (e.g. Huber 2000). Thus, the date of the fall of the First Dynasty of Babylon at the end of the reign of Samsuditana, after decades of decline (Richardson 2005) – the city itself is believed to have been sacked in a raid by the Hittite king Mursili I[13] – was lowered by 96 years from *c.* 1595 BCE in the Middle Chronology to *c.* 1499 BCE in the New (Low) Chronology (NLC). As a direct consequence, Ur-Namma's regnal dates become *c.* 2018–*c.* 2001 BCE in order to maintain the currently accepted relative chronology for the full five century-long Ur III–Isin and Larsa–Old Babylonian period.

Anachronisms

Almost immediately, G. Gualandi (1998) and G. Stiehler-Alegria (1999) independently recognized the advantages of a significantly shorter NLC-like chronology for the study of contemporary engraved cylinder seals. Stiehler-Alegria noted that, not unlike the pottery in the conventional chronology, late Old Babylonian cylinder seals also appear to span an overly long period evidencing only negligible stylistic developments. Gualandi, for his part, remarked how – because of the rapid stylistic evolution evident among the dynastic cylinder seals from Terqa on the Middle Euphrates – the NLC 'would account for the otherwise spectacular eventuality of finding Kassite styles in Terqa before the Kassite period' (1998, 133; cf. Podany 2014).

Although J. Reade (2001) was able to argue for emendations to the Assyrian King List (AKL) that readily brought the affected portions of that list into

[13] Hittite chronology, too, is nearly entirely dependent upon synchronisms with New Kingdom Egypt.

conformity with the New Low Chronology,[14] for a variety of specific reasons the NLC *per se* has not gained wide acceptance among scholars.[15] Nonetheless, F. van Koppen (2010) has reexamined AKL and the relevant Babylonian historical sources in the light of the New Low Chronology and has concluded that they too may be readily reinterpreted to support such a lower chronology.[16] And M. Bietak (2015, 323–324) has added that 'the synchronism of the Mari-correspondence, Hazor and Tell el-Dabᶜa during the transition within the Middle Bronze Age, from MB IIA–B to early MB IIB' works best with significantly lower Mesopotamian chronologies such as that proposed by Gasche *et al.*

Coeval Dynasties

Although certainly not the complete answer, it was already recognized by the *CAH* that in fact two or more canonical dynasties could have ruled simultaneously. But ancient Near Eastern king lists in general are largely devoid of any indications of possible dynastic overlap. Babylonian King List A,[17] for example, locates the 11 named kings of the First Dynasty of the Sealand – whose reigns add up to a stated total of 368 years[18] – immediately after the list of 11 Old Babylonian kings, now largely lost, and before the incomplete list of 36 Kassite kings. However, we are not entitled to conclude that 368 years separated the last Old Babylonian king Samsuditana from Gandash, the first Kassite king.[19] On the contrary, The Chronicle of the Early Kings (Chronicle 20B)[20] states quite plainly that the eighth Old Babylonian king Abi-eshuh, the son of Samsuiluna, set out, unsuccessfully, like his father, to conquer Ilumael (r.8–r.10) ,[21] the founder of Sealand I[22] – thus, the last five kings of the Old Babylonian dynasty were in fact contemporaries of the early kings of the First Dynasty of the Sealand (see *Figure 2*). The recently published Epic of Gulkishar[23] adds additional details describing the defeat of Samsuditana, the 11th and final king of OB I, by Gulkishar, the sixth king of Sealand I.

Chronicle 20B goes on to recount that the 11th and final king of Sealand I, Ea-gamil, fled to Elam when one 'Ulamburiash, brother of Kashtiliashu (III?) the Kassite[24]... conquered the Sealand' (r.12–r.14), thus evidencing further dynastic overlap, now between the later Sealand I and the early Kassite dynasties.[25] Unfortunately, prior to Kadashman-Enlil I, no dates can be assigned to any of the Kassite kings, most of whose regnal lengths are unknown. Kadashman-Enlil I himself, per the Amarna letters (EA 1–5), was a contemporary of Amenhotep III of the later 18th Dynasty of Egypt, whose own regnal dates are intimately tied to the larger debate here at hand.[26] The earliest reference to a Kassite king, here unnamed, receiving diplomatic gifts, is dated yr. 30 Samsuiluna (OLA 21, 20: Nicolas 2021, 168).

Additional Anachronisms

But lowering the date of the fall of Babylon by nearly a century, as first proposed by Gasche *et al.* (1998a), does not resolve all of the remaining chronological conundrums. Suggestive of yet further misalignment between the chronologies of Mesopotamia and of Egypt and the Levant are the observations made by L. Colonna d'Istria (2012) who has offered readings for two (nos. 149 Lo.E. and 435 R.E.) of the four West Semitic linear alphabetic epigraphs inscribed among a corpus of unprovenienced cuneiform administrative tablets in the Schøyen Collection. Published by S. Dalley (2009), these tablets,[27] and their clearly linked epigraphs, are dated to the reigns of the seventh and eighth kings of the Sealand I Dynasty, Peshgaldaramash, who ruled for 50 years, and Ayadaragalama, who ruled for 28 years, both now presumably in the century immediately after the Fall of Babylon – *viz.* the 16th

[14] Just such a chronology was used in *The Art of the First Cities* exhibition catalogue (Aruz and Wallenfels 2003).

[15] See further *Just in Time* (2000); Hunger and Pruzsinsky (2004).

[16] Cole (2014) succinctly updates and summarizes the cuneiform evidence pertaining directly to the NLC.

[17] Grayson (1980–1983, 90–96; Assur 14616c).

[18] Note that the total of the individual reigns given is only 347/357 years, but with the addition of *12 years for king 6a, 359/369 years (Brinkman 1977, 347, n. 8).

[19] The *Distanzangabe* denoting 696 years from Gulkishar, sixth king of Sealand I, to Nebuchadnezzar I, fourth king of Isin II (BE I/1 83: 5–8) 'probably represents an artificial calculation by a scribe on the basis of the canonical king lists, in which it is assumed that the First Sealand and Kassite Dynasties were consecutive' (Brinkman 1968, 83–84).

[20] Grayson (1975, 155–156; BM 96152).

[21] (r.8) Abishi(*sic*), son of Samsuiluna, set out to conquer Ilumael. (r.9) He decided to dam the Tigris. (r.10) He dammed the Tigris but did not capture Ilumael. Cf. Abi-eshuh yr. o: 'Year in which Abi-eshuh the king through the magnificent strength of Marduk dammed up the Tigris' (CT 8 33a).

[22] For Samsuiluna's loss of the Sealand in his year 11, see Dalley (2009, 8).

[23] Zomer (2019, 3–38, no. 1; Middle Babylonian copy from Nippur).

[24] 'Son of Burnabur(ar)iash (I?)' (Khanzadian *et al.* 1991–1992).

[25] The reigns of the last five OB I kings total 155 years, while the reigns of the first six kings of Sealand I total 215–236 years; the 60 years attributed to Ilumael has been questioned (cf. Brinkman 1977, 346–347, nn. 6–8; note the sign read GÍŠ = 60 may, of course, also be read DIŠ = 1). Cf. Mahieu (2022), who now proposes that Sealand I comprised two parallel lines of rulers, each 184 years long.

[26] Conventional so-called Sothic dates for Amenhotep III range from a high of 1417–1379 BCE (Memphite), to 1403–1364 BCE (Elephantine), to a low of 1390–1352 BCE (Theban); see further below.

[27] Dalley (2009, nos. 67, 134, 149, 435).

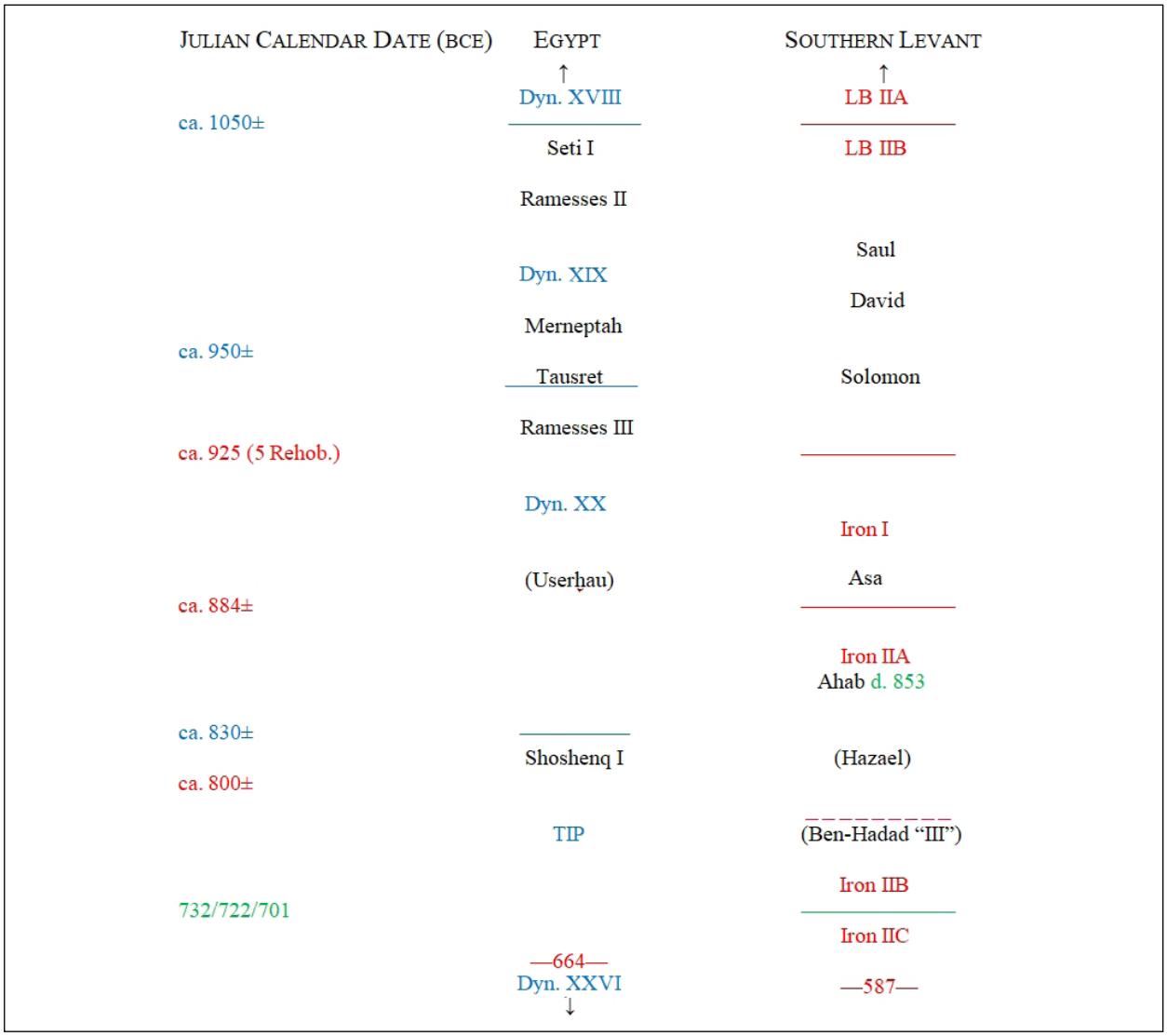

Figure 3. Outline of CoD-like revisions to the relative chronologies of Egypt and the Levant at the Late Bronze Age–Iron Age transition (after Wallenfels 2019, 497, Fig. 1).

century in the conventional Middle Chronology, or the 15th century in the NLC. However, Colonna d'Istria dated the epigraphs themselves palaeographically to no earlier than the conventional 14th century BCE, when in the Levant, such inscriptions are today commonly termed 'Old Canaanite'.[28] Further suggestive of such a chronological misalignment is the more recent observation by two of the principle authors of *Dating the Fall of Babylon* (1998), Armstrong and Gasche (2014, 2): that in addition to what is now seen as a **two** century-long gap in the conventional chronology of the ceramic sequences between the end of the Old Babylonian Period and the beginning of the Kassite Period, a second, far more significant, gap appears between the conventional late 13th century and the latter half of the historical 8th century BCE – at the transition from the Late Bronze Age to the Iron Age – with evidence for the conventional 11th century being 'virtually nonexistent'. It is this last Dark Age – perhaps better termed an 'empty age' – at the transition from the Late Bronze Age to the Iron Age that especially led to the organisation of the fourth BICANE conference.

Conclusions

I close here with two simple chronological charts (*Figures 3–4*) in which I summarize most briefly for the moment the revisions proposed for the reduction of this last transition by P. James and his collaborators in their 1991 *magnum opus*, *Centuries of Darkness*, with additional support from among the proceedings of BICANE 3, *Solomon and Shishak*, published in 2015 with Pieter van der Veen. The first of the two chronological revisions

[28] See further Hamidović (2014).

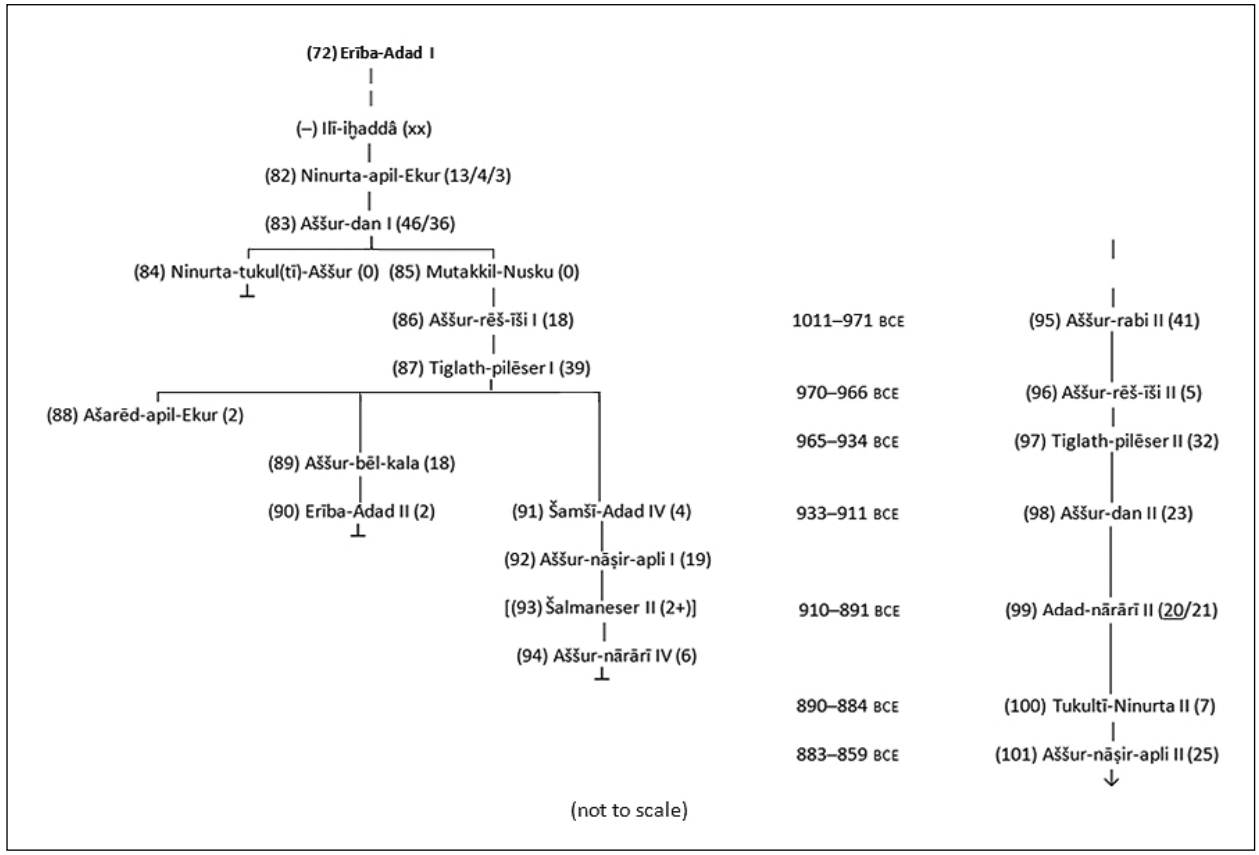

Figure 4. A CoD-like proposal for restructuring late Middle–early Neo-Assyrian chronology (after James et al. *1991).*

central to the *Centuries of Darkness*, or *CoD*, model is the nearly two-and-a-half century reduction in the dating of the transition from the Late Bronze Age to the early Iron Age in Egypt and the Levant (see *Figure 3*).

This is achieved by overlapping portions of the Egyptian Third Intermediate Period traditionally seen as successive, the removal of several spurious pharaohs and the shortening of others' ascribed reigns; in the Levant the Iron Age I period is reinterpreted and shortened dramatically while the Iron Age IIA period is redefined as 'Omride', not 'Solomonic'. The second chronological revision proposes that some 110–125 years be removed from the Middle Assyrian chronology achieved by positing two partially overlapping royal lines in the aftermath of the murder of Tukulti-Ninurta I (see *Figure 4*; see further Wallenfels, 'Middle Assyrian History and Culture', elsewhere in these Proceedings, pp. 63–79).

In conclusion, it should be manifestly obvious that the proposed unequal lowering of the Mesopotamian and Egyptian and Levantine chronologies will cause innumerable supposed historical synchronisms between these regions to be in need of serious re-examination and re-evaluation. Ladies and gentlemen, welcome to BICANE 4!

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Assyrian and Mitannian Pottery Styles: From Assur to the Euphrates and Beyond

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For the Mitannian/Middle Assyrian pottery 'sequence', it will be suggested that contemporary styles have been misinterpreted as sequential. There is stratigraphic evidence from many sites, but this evidence may have been force-fitted into a wrong historical chronology. Also, there is a genuine difficulty in finding long-range pottery links from east to west across the Euphrates, an exception being Nuzi Ware. This ware and the site of Tell Shiukh Fawqani show the true relation between Middle Assyrian and Levantine chronological systems. For early Neo-Assyrian pottery there is an almost complete lack of data — no pottery for the powerful ninth-century BC kings Aššurnaṣirpal II and Shalmaneser III — can that be correct? We accept orthodox Assyrian history back to at least 910 BC but where is the pottery? It will be suggested that early Neo-Assyrian pottery has been wrongly labelled as late Middle Assyrian. These adjustments all combine to substantially shorten chronology.

Introduction

If Late Bronze to Iron Age chronology is to be shortened by some centuries as per, e.g. James *et al.* (1991), it might be expected to have an impact on pottery sequences. This is not necessarily the case because pottery sequences can be compressed or stretched to any reasonable length to match a historical chronology. However, provided they are properly excavated, pottery sequences cannot be inverted or have parts omitted (except locally). They should act as a check on the correctness of historical sequences and inter-regional historical links. This ideal has frequently not been achieved in practice because philologists and archaeologists have sometimes not collaborated, or there is insufficient data, or a lack of interest in pottery. Also, important historical texts are often found out of context, e.g. on stonework that was subsequently reused, or, in the excitement of discovery, the exact context failed to get recorded properly, and the pottery of the relevant stratum only got published decades later, if at all. Linking archaeology and history is simpler if there are plenty of stratified clay tablets with dates.

When trying to link archaeology across long distances, e.g. from Assyria to the Levant, the pottery is not the same from east to west and one has to look for some special ware that was exported (see below on Nuzi Ware) or find some point in between where both Assyrian and Levantine pottery are found together (see

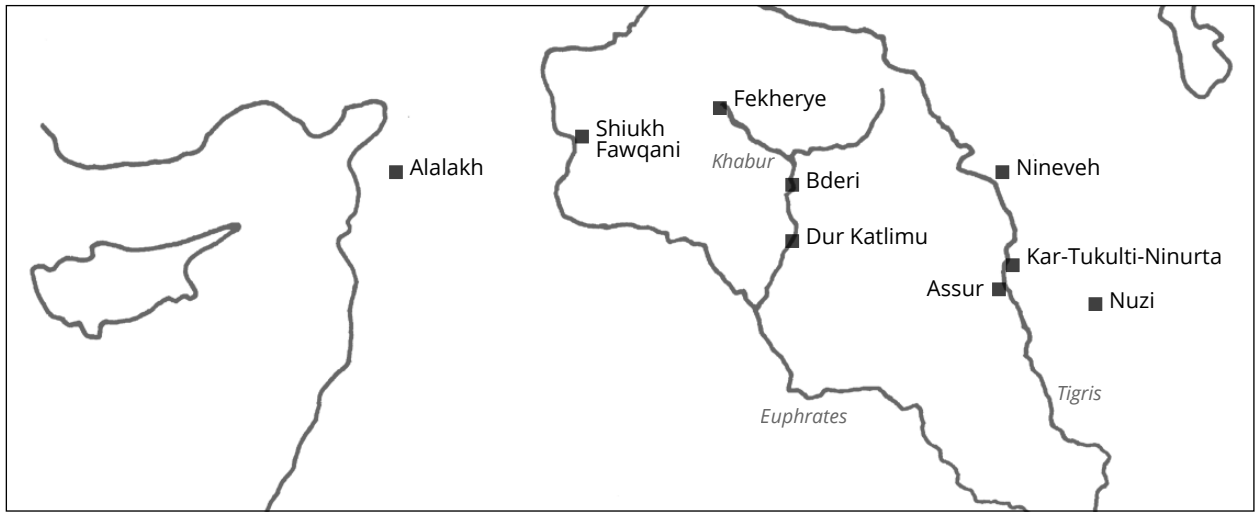

Figure 1. Map: places mentioned in text.

below on Tell Shiukh Fawqani). The Syrian Euphrates tends to be a barrier with little overlap between Middle Assyrian (MA) pottery and Late Bronze Age (LBA) Syrian pottery. Peter Pfälzner (1995) wrote: 'The border of MA official pottery in the west is in the region of the Syrian Euphrates bend ... [where there is a] north-west Syrian pottery tradition of the LBA with no typological comparison to MA pottery'[1] (see map, *Figure 1*).

Nuzi Ware

A type of pottery that does span the Euphrates is Nuzi Ware. It typically has white painted decoration on a dark background, often on tall, thin-walled beakers (*Figure 2*).

Figure 2. Nuzi ware goblet. Atchana Höyük, Amuq Phase M. (© Oriental Institute Museum, University of Chicago)

The designs can be geometric or floral and occasionally with animals or birds. This ware appears to be associated with the Hurrian kingdom of Mitanni because finds of Nuzi ware stretch from Nuzi in the East to Alalakh in the west, both places with Mitannian influence or control. At Nuzi the orthodox date is in the first half of the 14th century BC, a date based on the town of Nuzi probably having been destroyed in the mid 14th century[2] by Aššur-uballiṭ I, the presumed Assyrian king at the time of the Amarna period. Similarly, in the Levant at Alalakh, Nuzi ware is again dated in the first half of the 14th century, this time based on Late Helladic (LH) pottery which is also found in Egypt. Nuzi ware at Alalakh comes mostly before LH IIIA2, a type of Mycenaean pottery that was also found at Tell el-Amarna,[3] so again shortly before the Amarna period. Therefore, first half of 14th century at both Nuzi and Alalakh. Chemical and mineralogical evidence suggests that Nuzi ware was made in the vicinity of both Nuzi and Alalakh, but likely in other places too[4].

Nuzi ware also occurs at Kar-Tukulti-Ninurta[5], the new capital of Tukulti-Ninurta I slightly upstream from Assur. But here, Nuzi ware is dated in the second half of the 13th century BC, which is of course the dating of Tukulti-Ninurta I and is approximately a century and a half later than at Nuzi and Alalakh. Orthodox scholars explain the dating discrepancy by giving a long life to Nuzi ware[6]. But alternatively, it may be indicating some

[1] 'Die geographische Abgrenzung der mittelassyrischen offiziellen Keramik bildet im Westen die Region des Syrischen Euphratknies ... [mit einer] "nordwestsyrischen Keramiktradition" der Späten Bronzezeit. Diese zeigt in ihren kennzeichnenden Formen und Waren keine typologische Verbindung zur mittelassyrischen Keramik.' (Pfälzner 1995, 227).

[2] Wilhelm (1998–2001, 637); Stein (1998–2001, 641).

[3] The stratigraphy of Alalakh (Tell Atchana) seems to have been wrongly set out by excavator Sir Leonard Woolley. Fink (2010, 103, 137) reassessed the stratigraphy and the occurrence of Nuzi ware within it and wrote: 'there are several strong arguments for placing Atchana/Nuzi ware in the first half of the fourteenth century'. He placed Nuzi ware mainly in his Str. IVB^{F} (the superscript F indicates Fink's stratigraphic system) corresponding to Woolley's Str. III and II houses. Fink's Str. IVB^{F} comes immediately before his Str. III^{F} which has LH IIIA2 pottery (2010, 136–137).

[4] Erb-Satullo *et al.* (2011). My thanks to a colloquium attendee who provided the reference.

[5] Andrae (1923 or translation 1925, Pl. 5). The title of the plate is 'Fragments ... from Kar-Tukulti-Enurta' – the term Nuzi ware was not coined until later and, in any case, not all of the fragments illustrated are Nuzi ware. That some are Nuzi ware is confirmed, for example, by Hrouda (1957, 19): 'Für die Datierung der Nuzi-Keramik ... [d]ie "absolute" untere zeitliche Begrenzung ist durch das Vorkommen in Kar-Tukulti-Ninurta gegeben.' (Translated: 'For the dating of Nuzi pottery ... The "absolute" latest time is the building of Kar-Tukulti-Ninurta.')

[6] For the assumed latest date in the Assyrian heartland, see Hrouda in the previous note. Also, Pfälzner (1995, 46) wrote, 'Möglicherweise wurde die Nuziware in Assyrien länger produziert und verwendet als in anderen Regionen'. (Translated: 'Possibly, Nuzi ware was produced and used longer in Assyria than in other regions'.) The start date is typically thought to lie in the 16th century BC. Oguchi (2014, 215) wrote, 'At least within the upper Khabur basin, the use of Nuzi ware chronologically ranges from the mid-16th to

kind of chronological error. Do the Levantine dates need to be lowered by a century and a half to match Assyrian dates? If the Levantine dates are lowered, it would also be necessary to lower the dates of Mitanni, including its Amarna period king, Tushratta. That would then require an unrecognised and complicated overlap between Mitannian and Assyrian history, probably involving Mitannian incursions into Middle Assyria, a defeat or defeats that went unacknowledged by the Assyrians.[7]

Although unable to resolve such a complicated historical scenario, we can see some indications from an archaeological perspective. Pfälzner worked at Tall Sheikh Hamad, ancient Dur Katlimmu on the lower Khabur river, and he put North Mesopotamian pottery into the sequence: Mitannian, mA I, mA IIa, b, c and mA III.[8] The roughly contemporary Levantine system runs LB I, LB II and Iron I. Pfälzner's mA I–III types were low grade, mass produced and used mainly at Assyrian administrative centres (see *Figure 3*).

In successive phases of an administrative building at Dur Katlimmu, mA I and II types were dated by associated tablets to the period of Shalmaneser I and Tukulti-Ninurta I and were thought to continue into some of the following reigns. Although Pfälzner made his mA I follow Mitannian, he frequently noted that Mitannian strata had small quantities of pots similar to Middle Assyrian types; he wrote, 'nearly all common Middle Assyrian types ... occurred already in Mitannian pottery ... but in insignificant proportions'.[9] Some experts have queried whether there might have been an overlap between Mitannian and Middle Assyrian pottery, and it seems to me that we do have an overlap between Mitannian and mA I–II. A. Tenu (2013, 578) cautiously wrote, 'Assyrian pottery did not replace at every site and in every context the material previously used [i.e. Mitannian pottery]'. Furthermore, I reviewed the stratigraphy at various sites in North Mesopotamia showing that Mitannian and Middle Assyrian pottery did overlap, although the various excavators had tried to 'force fit' the evidence into the orthodox scheme (Porter 2007). This all pulls the dates of Nuzi and Alalakh closer to those of Kar-Tukulti-Ninurta thus giving Nuzi ware a shorter, more reasonable, time span.

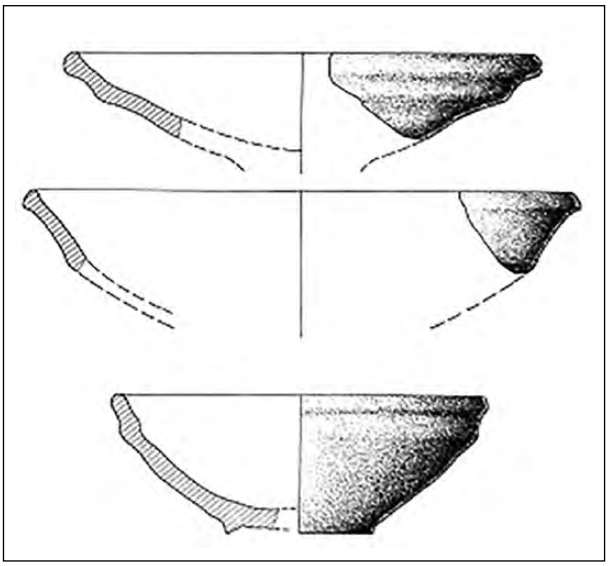

Figure 3. Carinated bowls of the mA II period. (Adapted from Pfälzner 1995, Pl. 104)

Tell Shiukh Fawqani[10]

This fairly small site on the Euphrates is on the east bank about 5km south of Carchemish and was a rescue excavation for the Tishrin dam project (Bachelot and Fales 2005). MA pottery was found and was definable according to Pfälzner's system as mA IIa–b (Capet 2005, 384–385), i.e. the time of Tukulti-Ninurta I and into the following reigns. Fawqani is apparently the only known site on the Syrian Euphrates with MA pottery (Capet 2005, 385). Possibly this site was an Assyrian trading post in the late reign of Tukulti-Ninurta when Assyrian

the second quarter of the 13th century B. C.' (note that Kar-Tukulti-Ninurta in the later 13th century is excluded by this quote's geographical restriction to the upper Khabur).

[7] Rearranging Mitannian and Assyrian interactions is beyond the scope of this article (and currently beyond this author's ability), but one might consider the following points. Rulers do not record their defeats, so for example, if Saushtatar defeated Shalmaneser I for a time, we would not expect to hear of it in Shalmaneser's texts, and we do not have records from Saushtatar. A sequence of Mitannian rulers is mentioned by Adad-nirari I and Shalmaneser I, namely Shattuara I, Wasashatta, Shattuara II. These three sound suspiciously like Shuttarna, Saushtatar and a second Shuttarna, allowing for problems of pronunciation and transliteration between Hurrian and Akkadian.

[8] Pfälzner (1995). This nomenclature was subsequently adapted to Jazira terminology: MJ I (was Mitannian), MJ IIA (was mA I), MJ IIB (was mA II) and MJ III (was mA III; Pfälzner 2007, 232, Fig. 1). Pfälzner's mA system was corrected in Kühne (2021), the final publication of the stratigraphy of the Middle Assyrian period at Dur Katlimmu (not seen until after the colloquium). Pfälzner's view of the stratigraphy, written in 2011, is given in chapter 3 but is overruled by Kühne in chapter 2, based on a final analysis of the excavation records. Pfälzner always admitted that his rather detailed pottery sequence was the result of quantitative analysis rather than obvious changes. This revision of the stratigraphy seems to condense his sequence to only two main styles, mA I/IIa and mA IIc/III. These would correspond to MJ II and MJ III on the Jazira system.

[9] Pfälzner (1995, 227): 'nahezu alle häufigen mittel-assyrischen Typen ... bereits in der mittanischen Keramik vorhanden waren ... aber anteilsmäßig unbedeutend.'

[10] This section was not included at the colloquium. It is based on Porter (2007, 619–621), with some minor corrections.

prestige was at its peak and nearby Carchemish was willing (or coerced) to permit a small Assyrian controlled outpost. Such a scenario looks improbable if Tukulti-Ninurta I was warring with Tudhaliya IV (the site is too small for major fortified opposition to Carchemish).

What is the stratigraphic position of this MA pottery? Does it confirm that Tukulti-Ninurta I, typically dated 1243–1207 BC, lived well after the Amarna period (LB IIA, 14th century BC) or does it show that Tukulti-Ninurta was contemporary with or earlier than that period (see the articles by James and Wallenfels elsewhere in this volume)? The MA sherds were found in Area E where a burnt building, 'la maison brûlée', had much material sealed *in situ* on the floors by its fiery destruction.[11] This building was dated by L. Bachelot to LB II, 14th century[12], and had MA pottery on its floors! Bachelot's MA pottery includes his vessel nos. 11–12, 19, 22, 101 and 104 (2005, 334–359), all found sealed on floors of the building, and he gives as *comparanda* pottery from MA Tall Sheikh Hamad (Dur Katlimmu) which Pfälzner dated as three each of mA I and mA II on his system.[13] Some other MA sherds were found in a higher layer, apparently eroded down the steep slope of the tell from a building that had stood further up. They were said to be totally disconnected from the layers above and below.[14] E. Capet freely attributed this washed down material to the MA period[15] in contrast to Bachelot's chapter on the maison brûlée with its sealed fourteenth-century material, which avoided describing any of the material as Assyrian although some of it clearly was. Bachelot's conclusions on the maison brûlée use many words to say very little, but he mentions possible Assyrian expansion towards the west and possible links through trade, etc. and 'similarities between different assemblages, sometimes very far apart from each other'[16] – could he have been puzzling over his unnamed MA pottery far from its homeland in Assyria?!

Bachelot had a further problem in that the Syrian pottery development sequence MB – LB I – LB II was said to be continuous, and it was difficult to say precisely where his local pottery came in this spectrum (Bachelot 2005, 329). He opted for the 14th century (LB IIA) but noted that comparisons with Tell Hadidi were fairly strong (2005, 327) although this site was destroyed in the 15th century (2005, 329), i.e. in LB I. Capet cautiously refers to Bachelot's maison brûlée as LB I–II (2005, 379). Three radiocarbon samples all gave 15th century dates,[17] although stated by Bachelot to indicate the 14th century (!; 2005, 326), apparently assuming the 'old wood effect', which is quite possible. Thus it seems that the destruction of the maison brûlée was LB I or LB IIA, either of which would be long before the dates attributed to the mA II pottery, i.e. later 13th or early 12th century (later Tukulti-Ninurta I or his near successors). Fawqani's mA II pottery seems to date roughly in the LB I–IIA periods, i.e. contemporary or pre-Amarna, hardly an orthodox time for Tukulti-Ninurta I.

The Pottery of Early Neo-Assyria

We move now from the earlier Middle Assyrian period to late Middle Assyrian and how its pottery links to Neo-Assyria. Can it be correct that the period of the early Neo-Assyrian kings Aššurnasirpal II and Shalmaneser III, a high point of Assyrian power in the 9th century, had no pottery? A. Hausleiter wrote, 'The early Neo-Assyrian period is, for material culture, widely unknown or unrecognised and would be dated in the time from the 10th century to *c.* 800.'[18] And S. Anastasio wrote, 'The period between the XI and X centuries BC is particularly obscure, and only a few more data can be ascribed to the IX and early VIII centuries BC.'[19] I think the reason the early Neo-Assyrians are not given any pottery is because their pottery is misleadingly called mA III (or MJ III) and is wrongly restricted to the approximate time of Tiglath-pileser I. Other articles in this volume consider how to shorten the Assyrian King List in the dark age but, briefly, on the *Centuries of Darkness* chronology (James *et al.* 1991), Tiglath-pileser I might be dated early 10th century

[11] Bachelot (2005, 302): 'Tous les toits était effondré à l'intérieur des pièces, scellant tout le matériel qui se trouvait sur les sols.' ('All the roofs were collapsed into the interior of the rooms, sealing all the material found on the floors.')

[12] Bachelot (2005, 330): 'nous font attribuer "la maison brûlée" à l'âge du Bronze récent II (durant le XIVe siècle av. J.-C.)'; ('we attribute the maison brûlée to LB II during the 14th century BC').

[13] E.g. bowl no. 11 on pp. 334–335 (Pl. 2) references Pfälzner (1995, Pl. 107c) which illustrates a bowl from Sheik Hamad and is given in the table on that plate as from the mA IIa stage.

[14] Capet (2005, 381): 'en total rupture avec les périodes antérieure et postérieure' ('totally disconnected from the periods before and after').

[15] Capet (2005, 379): 'céramique clairement médio-assyrien'. Capet's section describes an ephemeral layer just above the maison brûlée, possibly an unfinished attempt to rebuild, to which he attributes some of the MA sherds. Note that if Capet is correct that his area was above floors 628 and 632 (Capet 2005, 381) of the maison brûlée, the numbering of the excavation squares of the maison brûlée (Bachelot 2005, 306, Fig. 5) seems wrong: 344 and 345 should be 534 and 535. There are a number of misprints in the book.

[16] Bachelot (2005, 330): 'similitudes entre différent assemblages, parfois très éloignés les uns des autres'.

[17] Saliège and Pessin (2005, 1077): but they invoke the 'old wood effect' in suggesting a later date.

[18] Hausleiter (2010, 14): 'Die frühneuassyrische Zeit ist in der materiellen Kultur weitgehend unbekannt oder unerkannt und wird in die Zeit zwischen dem 10. bis zur Wende vom 9. Jh. zum 8. Jh. datiert'.

[19] Anastasio (2010, 4).

instead of *c.* 1100 BC, much closer to Aššurnaṣirpal and Shalmaneser (see Wallenfels elsewhere in this volume). Pfälzner linked mA III pottery to Tiglath-pileser I based on a pit at Tell Bderi, a small site on the river Khabur, in which this pottery was found together with clay cylinder inscriptions of Tiglath-pileser I.[20] It seems to me that mA III pottery, or something very similar, extended from the time of Tiglath-pileser I to at least Shalmaneser III. This extension of the dates of mA III pottery has already been suggested by C. Beuger, one of the Assur excavators, who wrote that in early Neo-Assyrian strata 'we propose that here we have a continuing use of typical Middle Assyrian forms such as Pfälzner's *Standard-Knickwandschale* [standard carinated bowl], at least until the 9th century'.[21] Similarly, she wrote that at Tell Billa, north of Nineveh, 'level 1 is dated by a tablet to Salmanassar III, but the level contains a great amount of significant Middle Assyrian pottery'.[22] R. Dittmann also thought that MA pottery continued into NA times. In an article originally sent to *Sumer* in 1988 and not published due to the war, but subsequently put on the internet, Dittmann wrote, 'an abundance of so-called "typical" Middle Assyrian forms persisted into the Neo-Assyrian period'.[23] This redating would affect sites from Assyria across North Mesopotamia to the Euphrates, sites and strata which are currently labelled late Middle Assyrian but are actually early Neo-Assyrian.

Middle Assyrian III pottery is sometimes found west of the Euphrates, and indeed Shalmaneser III frequently campaigned there and set up a few settlements slightly west of the Euphrates. Shalmaneser III's immediate successors were also influential west of the Euphrates, but mA III is not known to provide a link as far west as the Levant. However, there is a textual link which makes the jump from the ninth-century Assyrian empire to the Levant. It is the bilingual statue inscription in Akkadian and Aramaic from Tell Fekherye at the NW corner of the Khabur Triangle. This Assyrian style statue is of Hadad-yis'i, ruler of Gozan, son of Shamash-nuri who is thought to be the Assyrian eponym for 866 BC, thus putting Hadad-yis'i at *c.* 850 BC. The primary publication (Abou-Assaf *et al.* 1982) dated the style of the Aramaean text to the 10th or 9th century, but both Cross and Naveh dated the letter forms to the 11th century: Cross, 'The letter forms of the Tell Fekheriyeh text are all pure Phoenician of the 11th century'[24], and Naveh, 'the script of the Aramaic text is totally different from those of the so far known ninth century Aramaic and Phoenician inscriptions, and it is reminiscent of the early eleventh century BC Proto-Canaanite script'.[25] On the shortened chronology suggested here, we have eleventh-century Levantine letter forms brought down to the 9th century to match the Assyrian date of Hadad-yis'i at *c.* 850 BC.[26]

[20] Pfälzner (1995, 114). It is worth noting that we cannot be completely certain that the cylinders date from the same time as the pottery but merely that they were discarded with the pottery.

[21] Beuger (2014, 274).

[22] Beuger (2014, 280).

[23] Dittmann (1988, 14): 'eine Fülle von sogenannten "typischen" mittelassyrischen Formen bis in die neuassyrische Periode anhalten'. Note that there are two versions of this article on Dittmann's academia.edu page. The other one is 'Feldforschungen in Kar-Tukulti-Ninurta und Assur (Iraq) ... long version'; this version is poorly legible, but the quote comes on p. 19, and note that the table at the end of the article is legible in this version but scrambled in the other! Both versions accessed 20th June 2022.

[24] Cross (1993, 541, n. 23).

[25] Naveh (1987, 214).

[26] See already Sass (2005, 52).

Synthesis

What would be the effect on Levantine chronology of combining the lower dates suggested above for Nuzi ware, Tell Shiukh Fawqani and the mA III pottery? The LBA is normally dated *c.* 1550–1200 BC, but it was suggested above to lower the dating of Alalakh by at least a century so that its Nuzi ware comes at about the same time as Tukulti-Ninurta I in Assyria. This lowering was confirmed by pottery from the approximate time of Tukulti-Ninurta I found at Shiukh Fawqani in an LB IIA context. It was further suggested that we might lose another century or more by shortening the Assyrian dark age to make mA III the pottery of the early Neo-Assyrian period. We could easily have the two-and-a-half centuries shortening proposed in *Centuries of Darkness* (James *et al.* 1991).

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Discussion 1: The North Syrian Hurrian Kingdoms and Dark Age Chronology

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We much appreciate Bob Porter's arguments in the preceding essay for restructuring and lowering the dates for Mitannian and Middle Assyrian pottery and his conclusion that, '[w]e could easily have the two and a half century shortening proposed in *Centuries of Darkness* (James *et al.* 1991).' Further, Porter noted (p. 25):

> If the Levantine dates are lowered, it would also be necessary to lower the dates of Mitanni, including its Amarna period king, Tushratta. That would then require an unrecognised and complicated overlap between Mitannian and Assyrian history, probably involving Mitannian incursions into Middle Assyria, a defeat or defeats that went unacknowledged by the Assyrians.

And in an accompanying footnote 7, he added:

> Rearranging Mitannian and Assyrian interactions is beyond the scope of this article (and currently beyond this author's ability), but one might consider the following points. Rulers do not record their defeats, so for example, if Saushtatar defeated Shalmaneser I for a time, we would not expect to hear of it in Shalmaneser's texts, and we do not have records from Saushtatar. A sequence of Mitannian rulers is mentioned by Adad-nirari I and Shalmaneser I, namely Shattuara I, Wasashatta, Shattuara II. These three sound suspiciously like Shuttarna, Saushtatar and a second Shuttarna, allowing for problems of pronunciation and transliteration between Hurrian and Akkadian.

There is much to both agree and disagree on here – when it comes to the fine detail. We agree that on a revised model we should seek the familiar kings of Mitanni in the rulers of 'Ḫanigalbat' (an alternative name well-attested in many sources such as the Amarna letters) and have long been working on models along these lines. But Sauštatar and Uasašatta do not make a good match philologically. On the other hand, the names Šattuara (Ḫanigalbat) and Šuttarna (Mitanni) are surely the same but all depends on *which* individuals of those names. In the published version of his PhD thesis on Mesopotamian chronology Pierce Furlong (2010) proposed the identification of Šattuara I of Ḫanigalbat and Šuttarna II of Mitanni – but then his model, rather inconsistently, did not allow him to identify Šattuara II. While Furlong's thesis made many valuable observations, his rigid adherence to a precise 200-year reduction in the Egyptian high chronology led to many problems in his model such as this. However, on the roughly 250-year *CoD* reduction it is possible to identify both Šattuara I with Šuttarna I and Šattuara II with Šuttarna II.[1] Our survey below of the historical materials hopes to throw more light here and offer better identifications.

The conventional history of the Hurrian kingdoms of northern Syria, variously termed *n-h-r-n* and *m-t-n* in Egyptian hieroglyphs and Maitani/Mittani/Mitanni/Mittanni,[2] Ḫurri-land and Ḫani-rabbat ('Ḫanigalbat')[3] in the cuneiform sources, is built around two dynasties: the first, known from a variety of native as well as

[1] Contrast Weidner (1928–1929, 94) who suggested equating Šattuara I with Šuttarna III.

[2] The writing *ma-i-ta-ni* occurs exclusively in the earlier native Hurrian sources, including the cylinder seals of Šuttarna I and Sauštatar and a letter from Nuzi, whereas the sole case of a later native writing shows *mi-i-it-ta-a--an-ni* (*EA* 24: iii 104), suggesting the contraction of the diphthong *–ia-* > *î*, the presence of a long a-vowel in what is now the second syllable and the apparent doubling of the consonants, taken together with the later Akkadian writings which include *mi-it-ta-an-ni*; *mi-i-it-ta-an-ni*; *mi-i-ta-an-ni*; *mi-ta-an-ni*; *mi-it-ta-a-an-ni* (Knudtzon 1915, II, 1578–1579, s.v.) suggest an earlier vocalization '*Maittānni' and later '*Mîttānni'. Regardless, 'Mitanni' is used by convention throughout.

[3] For the reading *ḫa-ni*-GAL-*bat* = 'Ḫani-rabbat', see Valério (2011).

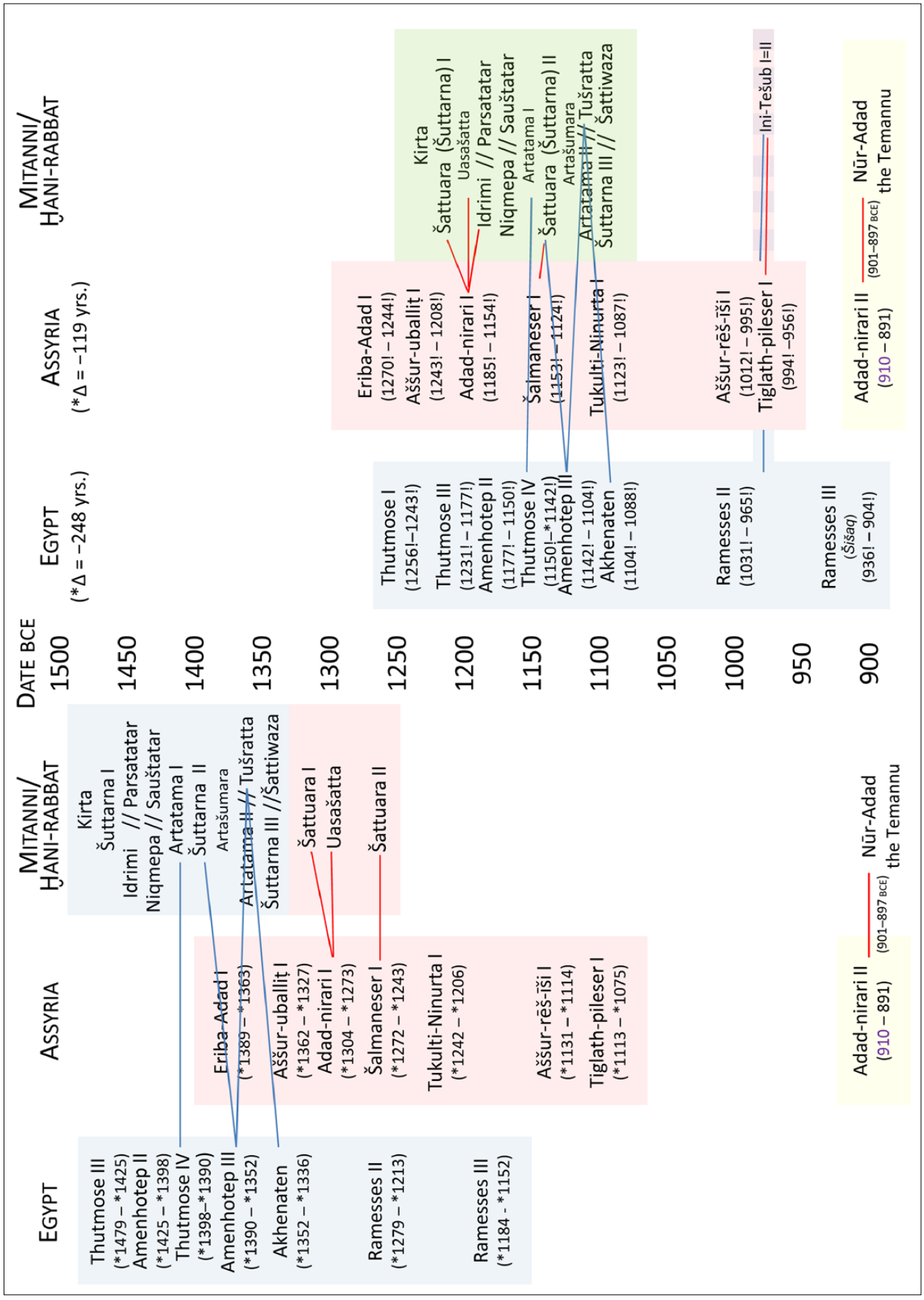


Figure 1. A CoD*-like realignment of the relative chronologies of New Kingdom Egypt, Middle Assyria and Mitanni/Ḫurri/ Ḫani-rabbat.*

Egyptian and contemporary Hittite and Hanaean sources, spans the 15th and 14th centuries BCE; the second, known only from Middle Assyrian sources, dates to the 13th century (see, e.g. Maidman 2017). However, when the Egyptian and Assyrian dynasties are realigned to reflect a *CoD*-like foundation (see *Figure 1*), wherein some 248 years are removed from the New Kingdom chronology and some 119 years from the Middle Assyrian (cf. Wallenfels elsewhere in this volume, after James *et al.* 1991, 317–20), the Assyrian segment now appears to be more-or-less contemporary with the Egyptian, implying that we are looking at the same history from two different vantage points.

From this new perspective, two pairs of quite similar-sounding names now align from which it may be postulated that Šuttarna I = Šattuara I and Šuttarna II = Šattuara II, each pair reflecting, in part, the different cuneiform script traditions, Syro-Mitannian and Middle Assyrian, employed in the writing of such Hurrian names.[4] Additionally, the Assyrian writing of Hurrian names was likely conditioned by how the Assyrian-speaking scribes heard, probably at second-hand at best, the vocalization of what to them may have been quite alien-sounding non-Semitic names.

In the light of this rearrangement and the acceptance of the new identifications, the following outline of the order of the Hurrian kings of northern Syria emerges, with a summary of the primary data available for each:[5]

(1) Kirta

King of Maitani, father of Šuttarna[6] (Šattuara) I; cf. Ugaritic 'Epic of *Krt*', king (*mlk*) of *Bt-Ḫbr* and his wife the Lady *Ḥry*.[7]

(2) Šuttarna[8] I/Šattuara I

Son of Kirta, king of Maitani;[9] following rebellion, 'king of Ḫani-rabbat' becomes vassal of Adad-nirari I[10] (1185!–1154! BCE); contemporary of Qiš-Addu of Hana.[11]

(3) Uasašatta

Son of Šattuara (Šuttarna) I, rebels against, then defeated by Adad-nirari I.[12]

(4) B/Parattarna[13]/Parsatatar[14]

Without patronymic; tributary of Idrimi (son of Ilim-ilimma) of Aleppo and Alalakh, himself tributary to Adad-nirari I;[15] contemporary of Qiš-Addu of Hana.[16]

(5) Sauštatar

Son of Parsatatar, king of Maitani;[17] adjudicated right of Niqmepa (son of Idrimi) of Alalakh to his status of 'Ḫani-rabbat-ship';[18] contemporary of Qiš-Addu of Hana,[19] Itḫi-Teššup (Itḫīya) of Arapḫa;[20] door of gold and

[4] Note especially the variant Šutatarra, for Šuttarna (III), in a Hittite text (Beckman 1999, 44 and n. 8). Cf. further distinct 'Syrian', with (local) Old Syrian features, and 'Syro-Hittite' scribal traditions at Emar, likely reflecting its shift from Mitanni to Hittite rule under Suppiluliuma I (Pruzsinsky 2007, 22).

[5] Cf. Maidman (2017).

[6] (1) *šu-ut-tar-n*[*a*] (2) DUMU *ki-ir-ta* (3) LUGAL *ma-i-ta-ni*: seal impression legend on legal tablet BM 131452; seal reused twice by Sauštatar on legal cases involving Niqmepa of Alalakh (Collon 1975, 131, n. 3).

[7] *CTA* 14: 143 *tn.ly.mṯt.ḥry* 'Give me the Lady Ḫurraya!' Ḫurraya bears *Yṣb* and *Ilḥ'u*, among their 7 (or 8) sons, and *Ṯtmnt*, among their 7 (or 8) daughters (Greenstein 1997, 23 [22–23]). The *krt* ('Kirta') epic is considered to be a relatively late Ugaritic composition, being recorded in alphabetic cuneiform during the reign of Niqmaddu III (r. *c.* *1225/1220–*1215 = 977!/972!–967! BCE; Wyatt 2015, 403–404); cf. mid-14th century (e.g. Greenstein 1997, 48, nn. 175–176). Rendsburg (2021, 15) observes that 'ancestral narratives (viz. 'Kirta') likely contain both a kernel of history and epic or legendary elements interleaved by the brilliant literati responsible for the canonical version.'

[8] Cf. the later Hittite writing Šutatarra for Šuttarna III (see n. 47, below).

[9] See n. 6, above.

[10] RIMA 1, 136, A.0.76.3: 4–14 (foundation stone BM 115687).

[11] Note the writing *Sa-i-tar-na* (*TQ* 12 T19: Mladjov 2019, 33).

[12] 'After his death, Uasasatta, his son, revolted … I (Adad-nirari I) captured by conquest the city Taidu, his great royal city, the cities Amasaku, Kaḫat, Šuru, Nabula, Ḫurra, Šuduḫu, and Waššukkanu' (RIMA 1, A.0.76.3: 15–51); several texts indicate Adad-nirari's intended building activity at Taidu (A.076.4–5, 22). Against the restoration ^{I}*wa-ša-š*[*a-at-ta*] in a fragmentary Hittite letter from an unnamed Hittite king to an unnamed Assyrian king (e.g. Hoffner 2009, 322–323), see now Mora and Giorgieri (2004, 191).

[13] *ba-ar-at-tar-na* LUGAL ERIN$_2$.MEŠ *ḫur-ri*^{ki} 'RN king of Ḫurri warriors' (*Idrimi* 43–44: Greenstein and Marcus 1976, 65); *pa-ra/i-tar-na* (*TQ* 12 T9, T 10, T15, T16: Mladjov 2019, 33).

[14] See n. 17, below. Several commentators distinguish between these two names, e.g. Maidman (2017); Stein (1989, 43–44).

[15] *Idrimi* 43–58 (Greenstein and Marcus 1976, 67); Idrimi concludes with, 'These things I did and then I entrusted them to the hands of Adad-nirari, **my son**' (ll. 90–91 [*ibid.*, 68]; emphasis added); cf. Collon (1975, 167, n. 3).

[16] See n. 13, above.

[17] (1) *sa-uš-ta-at-tar* (2) DUMU *par-sa-ta-tar* (3) LUGAL *ma-i-ta-ni* (seal impression legend: RIMA 1, N.0.1001; cf. Stein 1989); reused by Artatarma I (Tell Bazi), Šuttarna II (Umm el-Marra), Artašumara (Tell Brak), Tušratta (Tell Brak; Cooper *et al.* 2005, 43).

[18] *AT* 13 (BM 131452); for a recent edition, see Niedorf (2008, 239–244 [31.1]).

[19] *sa-u-sa-da-at-ra* (*TQ* 12 T6, T12: Mladjov 2019).

[20] Stein (1989).

silver 'stolen' from Assyria[21] taken to Waššukkani.[22]

(6) Artatama I

Without patronymic, king of Hurri;[23] treaty with his son-in-law [Thutmose IV] (1150!–1142! BCE).[24]

(7) Šuttarna II/Šattuara II

Son of Artatama I;[25] father of Keluhepa wife of Amenhotep III (1142!–1104! BCE);[26] 'king of Ḫani-rabbat' defeated by Shalmaneser I (1153!–1124! BCE).[27]

(8) Artaššumara

Son of Šuttarna (Šattuara) II,[28] murdered after a brief reign;[29] contemporary of Niqmepa of Alalakh.[30]

(9) Artatama II

Usurper; son of Šuttarna (Šattuara) II;[31] king of Ḫurri;[32] contemporary of Tušratta;[33] treaty with Suppiluliuma I.[34]

(10) Tušratta

King of Mitanni,[35] son of Šuttarna (Šattuara) II, grandson of Artatama I,[36] husband of Yuni;[37] sister of Keluhepa,[38] married to Amenhotep III (1142!–1104! BCE)[39]; father of [...],[40] married to Amenhotep III, and of Taduhepa,[41] married to Amenhotep III and Akhenaten (1104!–1088! BCE);[42] contemporary of Artatama II, Šuttarna III, Suppiluliuma I;[43] father-in-law of Amenhotep III (1142!–1104! BCE),[44] Akhenaten (1104!–1088! BCE);[45] built a palace; in possession of Šauška of Nineveh;[46] murdered by son,[47] Mitanni divided between the lands of Alši and Assyria.[48]

(11) Šuttarna (Šutatarra)[49] III

Son of Artatama II;[50] destroyed Tušratta's palace; humbled self before Assyrians, returned door stolen from Assyria by Sauštatar;[51] Aki-Teshub flees to Karduniash.[52]

(12) Šattiwaza

Son of Tušratta;[53] <great>-great-grandson of Sauštatar;[54] vassal,[55] son-in-law[56] of Suppiluliuma I; after Šattiwaza fled for safety to Suppiluliuma I, the latter's sons, Telipinu[57] and Piyassili (Sharri-Kushuh),[58] appointed Viceroy of Aleppo and Viceroy of Carchemish, respectively.

[21] The god Ashur (Akk. *Aššur*) and the city and country named for him can ordinarily only be distinguished out of context in the writing by their respective determinatives. The scribe of the Akkadian version of Beckman (1999, Treaty 6B), ambiguously wrote the determinatives for both land (KUR) and city (URU) before most place names in this text (KBo I, 3, *passim*), reserving URU alone for Mitannian cities only. Nonetheless, Adad-nirari I's extensive building program, including temples, ziggurat, palace, walls and gates at the city of Ashur itself (Grayson 1987, 128) must have made that city's new-found wealth a tempting target for Sauštatar in Assyria's presumed moment of weakness.

[22] *CTH* 51, 52.

[23] Beckman (1999, Treaty 6A).

[24] *EA* 29: 16–17.

[25] *EA* 24: 52–53; Beckman (1999, Treaty 6B).

[26] *EA* 17: 26–29; 29: 16–27.

[27] EŞ 6688 (RIMA 1, A.0.77.1, 1).

[28] *TB* 6002.

[29] *EA* 17: 19–20.

[30] BM 131497 (Wiseman 1953b, 108).

[31] *EA* 24: 47.

[32] Beckman (1999, Treaty 6A, §1).

[33] Beckman (1999, Treaty 6A).

[34] Beckman (1999, Treaty 6A).

[35] Refers to his own land as Ḫani-rabbat in letter to Amenhotep III (*EA* 20: 17); cf. king of Mitanni (*EA* 24); Beckman (1999, Treaty 6A, §1).

[36] *EA* 24: 52–53.

[37] *EA* 26: 60, 62.

[38] *EA* 17: 5, 41.

[39] *EA* 17, 19, 20, 21, 22 (Taduhepa, Tušratta's daughter, dowry), 23, 24 (Ḫurri king), 25, 26 29.

[40] *EA* 19: 2; 20: 1; 21: 3; cf. 29: 21.

[41] *EA* 22: 46–47; 23: 7; 24: 103–104; 26:4; 29: 3

[42] *EA* 26–29.

[43] Beckman (1999, Treaty 6A).

[44] See n. 39, above.

[45] *EA* 26–29; Akhenaten would appear to have taken his father's recently acquired bride, Tušratta's daughter, Taduhepa, as his own (cf. n. 39, above).

[46] *EA* 23: 13–17; 24: 98–99.

[47] Beckman (1999, Treaty 6A).

[48] Beckman (1999, Treaty 6A, §6).

[49] Treaty 6A, §6 = Šuttarna (6B; Beckman 1999, 44 and n. 8).

[50] *CTH* 51, 52 (Beckman 1999, Treaty 6A, 6B).

[51] *CTH* 51, 52 (Beckman 1999, Treaty 6A, 6B).

[52] *CTH* 51, 52 (Beckman 1999, Treaty 6A, 6B).

[53] *CTH* 51, 52 (Beckman 1999, Treaty 6A, 6B); HSS 15 105 (captioned seal impression).

[54] *CTH* 51, 52 (Beckman 1999, Treaty 6A, 6B).

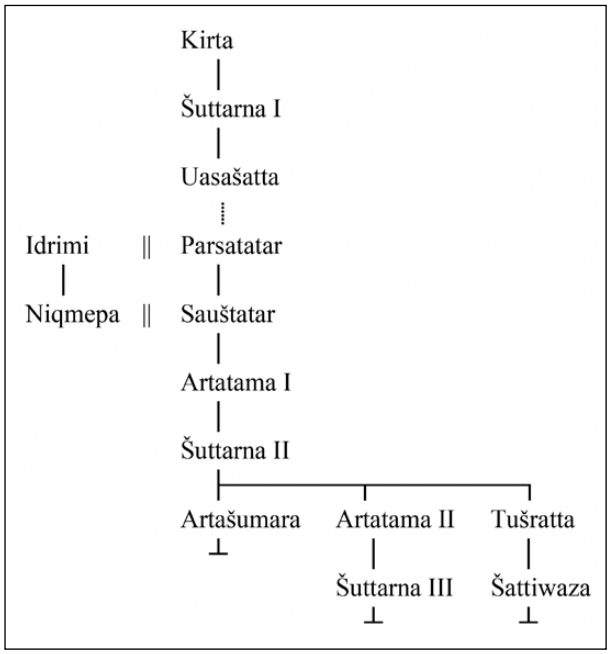
[55] *CTH* 51, 52 (Beckman 1999, Treaty 6A, 6B).

[56] Beckman (1999, Treaty 6A, §6).

[57] Bryce (2005, 180).

[58] Bryce (2005, 179 and n. 127).

These data suggest the following genealogical relationships:



Selected Historical Outcomes

Certain patterns emerge from the distribution of the terms Maitani, Mit(t)an(n)i and Ḫani-rabbat. In the time of Sauštatar, Mitanni and Ḫani-rabbat may not have been seen as entirely synonymous: Sauštatar was king of Maitani when he appointed Niqmepa of Alalakh, son of Idrimi, rather than another claimant, one Irib-ḫazi, to 'Ḫani-rabbat-ship', or the like.[59] During the Amarna period, the Hurrian kings called themselves kings of 'Mitanni', written in numerous ways,[60] but note especially *mi-i-ta-an-ni*, implying the normalization *Mītanni*, in *EA* 24: 3, which is composed entirely in Hurrian. Note however, that Tušratta, who calls himself 'king of Mitanni', also refers to his land as Ḫani-rabbat.[61] Note further, during the reign of Suppiluliuma I, Tušratta styled himself king of Mitanni, while his rival sibling Artatama II was termed king of Ḫurri.[62] Petrographic analysis of the clay of the Amarna tablets from Tušratta[63] suggest their origins in the vicinity of Tell Fekheriye (Akk. ^{uru}*si-ka-né*; Aram. *skn*, during the 9th century BCE), which has often been suggested to be the site of the Mitanni royal city of Waššukkanni.[64]

It is clear that to the Assyrians, from their perspective, everything to the west of Assyria-proper was 'Ḫani-rabbat'. The term Mitanni or the like does not appear in Assyrian texts until the reign of Tiglath-pileser I (r. *c.* 994!–956! BCE).[65] A mention of *m-t-n* in the Great Asiatic List (XXVII) of Ramesses III (no. 28), next to *k-r-k-m-s* (no. 29),[66] a few decades after Mitanni's mention by Tiglath-pileser I, suggests that some sort of entity near Carchemish called 'Mitanni' survived into what is now the 10th century BCE. And thus, the reference to Shoshenq I 'repress(ing) ... the militia of Mitanni' in his Karnak inscription – now redated to the later 9th century BCE – may not be the anachronistic 'flourish' commonly ascribed to it (e.g. Kitchen 2017, 14–15; cf. James and van der Veen 2015, 130–131).

The full meaning of Idrimi's concluding remark to his autobiographical inscription (ll. 90–91), that he 'entrusted' (*aptaqissunu*) the kingdom he has just reacquired and expanded into the hands of one ^{d}IM-*ni-ra-ri*, without much further explanation becomes clearer.[67] Although in this cultural setting, the logogram for the divine name might be read Adad or Teššub or any other local storm god, the *CoD*-like chronology proposed here, clearly situates *the* Adad-nirari I of Assyria, who certainly campaigned quite far west,[68] as the intended recipient. In this light, Idrimi, who 'was king for 30 years', (l. 102) acknowledging Adad-nirari I as his overlord (while seemingly affectionately calling him 'my son'[69]), establishes yet another secure relative synchronism for the new paradigm.

A newly edited Middle Assyrian letter in the British Museum (BM 103203), published by M. Maidman (2024), on the one hand, 'looks very much like a Nuzi text' and mentions two well-known Ḫurrian names, Utḫiya and Kipaya, while at the same time it 'indeed, resembles a Middle Assyrian document in both language and ... contents.' If, in fact, the name of the

[59] The writing *ḫa-ni*-GAL-*ba-tu-ti*, a place name with the Akkadian abstract suffix *-ūt* (Assyr. *-utt*) is unusual, being typically attached to bases designating persons and professions, here apparently designating some particular, presumably high, social status (e.g. *CAD* Ḫ, 80, s.v.; cf. de Ridder 2018, 170–171). The fact that Niqmepa, king of Alalakh, was in some sort of competition with Irib-ḫazi for the designation of Ḫani-rabbat-ship, that only one or the other might possess it, suggests that there might be some level of political power associated with the position; note that as a result of having lost his case, Irib-ḫazi returned to Niqmepa's service; cf. Niedorf (2008, 241–243).

[60] See n. 2, above.

[61] E.g. *EA* 20: 17; 29: 2, 49.

[62] Beckman (1999, Treaty 6A, ¶1).

[63] *EA* 17–30.

[64] Goren *et al.* (2004, 38–44).

[65] ^{kur}*mi-ta-ni* (RIMA 2, A.0.87.1: vi 63); cf. the reference to the city of Milidia (Melid, modern Arslantepe) in the land of Ḫani-rabbat earlier in the same text (v 34), but this is assumed by Grayson (1991, 22, n. v 34) to be an error for Ḫatti.

[66] Simons (1937, 166).

[67] E.g. Greenstein and Marcus (1976, 69, s.v. *Line 2*).

[68] Note the tablet copy of a label originally affixed to cedar pillars, booty from Naḫur, near Harran (RIMA 1, A.0.76.25).

[69] Cf. Niqmepa son of Idrimi (e.g. *AT* 17, 69); note, e.g. Drower (1973, 435–436), who supposed rather that Adad-nirari was an elder son of Idrimi who predeceased his father, leaving Niqmepa to later assume his father's throne.

addressee, Utḫiya, is but a graphic variant to the RN Itḫiya, itself a hypochoristicon for Itḫiya-Teššub, son of Kibi-Teššub, king of Arrapḫa, then, contra Maidman (2014, 3), I/Utḫiya, in our revised *CoD*-like chronology, would have been a contemporary of Adad-nirari I. As argued similarly by Wallenfels elsewhere in this volume (p. 69), the presence of Middle Assyrian philological features in Nuzi texts are not anachronisms but rather accurate reflections of the actual political situation as outlined herein during what is now the 12th! century BCE.

Sauštatar's attack on Assyria, permitting his 'stealing' a door of gold and silver – presumably from one of the recently rebuilt edifices that Adad-nirari I commissioned within the city of Ashur itself – and removing it to Waššukkani, must have occurred at a point of weakness towards the end of the reign of Adad-nirari I, perhaps following his defeat by one Kurigalzu at Sugaga on the Tigris (see further James, 'A Critical Look...', p. 47, elsewhere in this volume),[70] prior to the reign of Shalmaneser I. Note the unfinished inscriptions excavated in Ashur[71] intended for placement in the Mitannian capital city of Taidu, first captured along with Waššukkani by Adad-nirari I following his defeat of Uasašatta. Note further the fact that Sauštatar was in a position to adjudicate the right of Niqmepa of Alalakh (son of Idrimi, himself a vassal of Adad-nirari I) to a position of 'Ḫani-rabbat-ship' – significant portions of Ḫani-rabbat must have certainly been subject in some fashion to Adad-nirari I during his extensive military campaigns there. The Assyrian sources are, not surprisingly, characteristically silent on such matters. (And how did Tušratta come into possession of Šaušga of Nineveh?!) Shalmaneser I's subsequent defeat of the 'king of Ḫani-rabbat', Sauštatar's grandson Šuttarna (Šattuara) II, speaks to his need to re-conquer lands previously subject to Adad-nirari I. Shalmaneser's further establishment at Dur-Katlimmu on the lower Khabur of a line of viceroys (*sukkallū rabiūtu*), beginning with his own brother Ibašši-ili, ruling as 'kings of Ḫani-rabbat',[72] would place Assyrian forces in the very midst of northern Syria, pressing the growing number of competing Ḫurrian kings ever westward toward Hatti and Egypt. Their fate there is well rehearsed in the literature.

Future research, well-beyond the present note, must be able to successfully realign Mitannian and Middle Assyrian seal styles and iconography in the light of Wallenfels' remarks elsewhere in this volume (pp. 71-74). In particular, the potential impact of redating the so-called Mitanni Elaborate-style cylinder seal impression on VAT 8951 (*KAJ* 177)[73] and the necessary (re)assignment of the *līmu* Aššur-mutakkil son of Adad-eriš, currently assigned to the reign of Aššur-nirari II (e.g. Saporetti 1979, 29), must be re-evaluated.

Abbreviations

AT	Wiseman 1953a
CAD	The Assyrian Dictionary of the Oriental Institute of the University of Chicago
CTA	Herdner 1963
CTH	Laroche 1971
EA	Knudtzon 1915
HSS 9	Pfeiffer 1932
HSS 15	Lacheman 1955
KAJ	Ebeling 1927
RIMA 1	Grayson 1987
RIMA 2	Grayson 1991
TB	Finkel 1985
TQ	Mladjov 2019.

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[70] Grayson (1975, 175; *Chronicle* 22 iii 20–22); cf. Grayson (1975, 159–160; *Chronicle* 21 i 18′–23′).

[71] RIMA 1, A.0.76, 4–5, and perhaps 22.

[72] Szuchman (2007).

[73] Beran (1957, 142–144 and Fig. 1).

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A Critical Look at Mesopotamian-Egyptian Synchronisms during the 14th–13th Centuries BC*

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Editors' Foreword

The following essay was very much a work in progress at the time of the author's untimely passing in August 2024, following several earlier hospitalizations. Prior to this, James and Ronald Wallenfels had been exchanging various possible solutions to the vexing issues presented by Kassite chronology. But by no means had they achieved a mutually acceptable genealogy for the dynasty, despite the increasing convergence of their views. The self-evidently limited, incomplete, and often fragmentary nature of the preserved epigraphic evidence and the laconic prosopographic data on e.g. clay tablets, stone stelae, cylinder seals and seal impressions and other Kleinfunde*, continues to preclude any form of certainty (see further Wallenfels' remarks in 'Middle Assyrian History and Culture …' in these Proceedings, p. 64, n. 7). The last version of James's manuscript published here was developed over a substantial period of time and appears to have never been fully internally harmonized. Thus, so as not to second-guess the author's intentions, the manuscript has been edited minimally principally for typographical errors and bibliographical completeness.*

Introduction

At the outset, I take it as a given that the standard chronology for Egypt, based on Manetho, Sothic dating, and the Shishak-Shoshenq equation, has not been scientifically proven. The fragments and epitomes of the Hellenistic writer Manetho may have been adequate as a guide to reconstruction for 19th-century scholars and his system of dynasties has proved to be a useful convenience. But Egyptian history must now, in the 21st century, be reconstructed *only* from the monuments (by which I include papyri, ostraca, seal inscriptions, etc.) alone, with no filling in the gaps with figures from Manetho (see James and Morkot 2013, Part I). Nikos Kokkinos' paper in these Proceedings, 'Assyrian Synchronisms, the Tyrian Annals, and Carthage …' (p. 280), has some enlightening things to say on the origin of Manetho's dating system. Manetho, who wrote for a Greek audience, relied for his dates on the Sicilian historian Timaeus; not surprisingly then, when Manetho gave a 'synchronism' between Egypt and the Trojan War, the date he gave of 1194 BC was very close to those offered by some Greek chronographers and the eventual canonical date of 1184 BC calculated by Eratosthenes (Kokkinos 2009). When archaeologists of the late 19th century dated Mycenaean pottery through links with Egypt they were relying on a Manethonian chronology. They imagined that the traditional dates for the Trojan War provided added confirmation that the Trojan War and hence the Mycenaean age ended in the early 12th century BC, unaware that this was not independent corroboration but more of a circular argument – they were actually reintroducing a date ultimately derived from Greek chronography back to the Aegean. Had they followed the alternative low chronology which was extant in ancient Greek times, they would have placed the Trojan War *c.* 940 BC and accordingly allowed the much lower dates for Mycenaean civilization as recommended by Cecil Torr (1896) and other scholars such as Jens Lieblein, the founding father of Norwegian Egyptology (see James *et al.* 1991a, 15–17).[1]

Apart from the now completely redundant use of Manetho, there are two main bases of Egyptian chronology. First is Sothic dating, which is frequently misunderstood as being a means of astronomical dating. As the late Archie Roy (Professor of Astronomy at Glasgow) explained many years ago (1982), the astronomical input is minimal. Sothic dating is actually an unproven, and possibly unprovable, theory based on the idea that the Egyptians used a calendar that drifted through the seasons and was never corrected.

[1] Incidentally, in his earliest works – when Shishak's invasion into the kingdom of Judah was still dated to *c.* 975 BC – Lieblein (1863, 128) suggested that Ramesses III was the biblical Shishak (a major crux of the *CoD* model). When Shishak's date was lowered to *c.* 925 BC, he moved away from this idea and followed the standard idea that he was Shoshenq I, though still arguing for a major overlap between the 21st and 22nd Dynasties and an overall lowering of Egyptian New Kingdom chronology (Lieblein 1868, 273, n. 1; 1914, 361).

The theory is difficult to sustain as we still do not fully understand the complexities of the pre-Persian Egyptian calendar(s). Second is the equation of Shoshenq I with the biblical king Shishak which was a major theme of the BICANE 3 colloquium in 2011 (James and van der Veen 2015). It is a philological identification which may well be completely misleading (Bimson 2015; van der Veen 2015). All told, the claim of Kenneth Kitchen (2007, 167) and others (Krauss and Warburton 2006, 473–474) to have 'dead-reckoned' Egyptian chronology back to the New Kingdom, is a demonstrable myth – their actual method is to fill up the time with reigns and regnal years upon a framework predetermined by Sothic dating and the Shoshenq-Shishak equation and with a liberal use (and emendation) of Manetho's figures when it suits them.[2]

As for radiocarbon, I refer here to the article by Zerbst and van der Veen in these Proceedings (see pp. 373-399). But my feeling is that the method is, at present, still too blunt a tool to decide between dating controversies involving one to two centuries – despite many claims to the contrary. The same applies to dendrochronology as it has been applied in the Near East. An overarching problem here is that while I do not doubt the value of these methods *per se*, I seriously doubt the motives and probity of some of their practitioners or rather interpreters (see James 2012). We are still not told all the results done on samples, which are often improperly selected (Johanna Regev, pers. comm.). There is still a lack of proper inter-laboratory testing and above all *blind testing*. After almost a lifetime of anticipation, I am still waiting to hear what radiocarbon genuinely wants to tell me! So it is reassuring that Egyptologists such as Manfred Bietak, Kenneth Kitchen and some others now hold radiocarbon results at arm's length. As David Aston (2012, 289) remarked:

> Despite the impassioned plea of Sturt Manning that 'Radiocarbon dating should become the friend of Egyptologists', Radiocarbon and Egyptology do not seem to make the best bedfellows, particularly in regard to the offset of about 100–120 years between the suggested radiocarbon dates and the archaeological dates proposed by the excavators of the Second Intermediate Period levels at Tell el-Daba.

In order to reconstruct ancient history, the time-honoured methods of matching text and archaeology still remain the most realistic ones.

The Two 'Unshakeable' Pillars of Ancient Near Eastern History

Perhaps because of the many doubts in the field regarding radiocarbon, Sothic dating, and 'dead reckoning', Egyptologists have turned increasingly to Mesopotamia as a way of refining and/or confirming their dates.[3] But there are pitfalls here. To cite the caveat of George Hanfmann (1951, 261) already quoted in the Introduction to these Proceedings:

> ... it is well to remind ourselves from time to time that the two great pillars of the chronology of the Bronze Age, the Egyptian and the Mesopotamian, are not two stout towers resting on immovable foundations. They may rather be likened to two buoys linked by a chain and anchored, to be sure, yet raised or lowered by the waves of the sea. Both chronologies include problems which cannot as yet be solved except by reasonable guesses – the specific years to which observed astronomic data should be assigned, the estimates for the lengths of obscure periods, and the evaluation of possible gaps, duplications, and exaggerations in Royal lists and building inscriptions.

One can read innumerable times how these two buoys, or 'pillars' to use the terminology of this colloquium, are mutually supporting – for example, that we know from the Amarna tablets found in Egypt that there is a firm synchronism between Akhenaten of the 18th Dynasty and Aššur-uballiṭ I of Assyria, and that the Assyrian King List assures us that the latter reigned in the mid-14th century BC, in agreement with Sothic chronology. Yet the misunderstanding by Egyptologists of the actual Mesopotamian evidence can sometimes be alarmingly poor – I have seen at least two imaginary synchronisms cited.[4] Further, while appealing to such king lists, it is often forgotten that they are late texts by their very nature. They date no earlier than the last kings they mention – which are from the late 8th, 7th or even 6th centuries BC. This is not to say that the information in them is false as such. But such texts must be understood as the attempts of late Assyrian and Babylonian chronographers to reconstruct their own history, in much the same way that the later ancient Greeks like Timaeus and Eratosthenes did.

[2] See Morkot and James (2015). Over the last few decades, the chronology of Third Intermediate Period Egypt has come under increasing critical scrutiny (James *et al*. 1987; 1991a, b; 1992; 1998; Hagens 1996; Jansen-Winkeln 2006; Morkot and James 2009; James and Morkot 2010; Dodson 2012/2019; Thijs 2015; James 2017, 2021).

[3] Spalinger (2002, 248); Depuydt (2005, 32; 2007, 2008); Wiener (2006); Krauss and Warburton (2006, 477); Bloch (2010a); Aston (2012, 307–309); Miller (2017, 108–109); Dodson (2019, 181–189).

[4] One, unfortunately, by our colleague Aidan Dodson (2012, 182) but corrected in Dodson (2019, 182). For another, Aston (2012, 307) refers to 'the fact that Tuthmosis IV is known to have been in contact with the Babylonian king Karaindash' – this is not a 'fact' based on documentary evidence, but a guess based on a glance at a modern table of kings.

The Exaggeration of Antiquity

Before using the information that these king lists contain, we need to understand the cultural milieu, mindset and motives of those who compiled them. In his paper 'Middle Assyrian History …' in these Proceedings (see pp. 63–79), Ronald Wallenfels has outlined reasons for understanding that the Assyrian King List (AKL) has masked the existence of two parallel dynasties in Assyria during the early first millennium BC. The same has long been admitted for earlier periods (see Yuhong and Dalley 1990). And the motive is clear – to present the illusion for 'amuletic' purposes of an unbroken sequence of Assyrian kings from the latest times back to those in the dim and distant past who still 'lived in tents'. The illusion is just that. The AKL includes palpable falsehoods, such as the inclusion of Šamši-Adad I's Amorite ancestors among the kings of Ashur,[5] and many inaccurate (sometimes fraudulent from our point of view) genealogical links.[6]

Much the same is true of the Babylonian king lists, which are the product of the same cultural milieu as the Assyrian. Babylonian King List A, the main text employed by Assyriologists, was composed in the late 7th or 6th century BC, perhaps in the reign of Nabonidus (Mahieu 2021a, 373, 376).[7] It is worth noting that an inscription of Nabonidus (Brinkman 1976, 8, n. 5) dated Šagarakti-Šuriaš 800 years before his time which is over a century earlier than that which present conventional chronology allows (*c.* 1245–1233 BC). Even greater exaggerations are given in other texts from Nabonidus: one claims that Hammurapi reigned 700 years before a king Burnaburiaš (Brinkman 1976, 8, n. 5), which if he were the Amarna correspondent (conventionally *c.* 1359–1333 BC) would place Hammurapi in the late third millennium BC (see Albright 1921, 86), an absurdly high result by modern standards. Albright nevertheless found the result acceptable though he baulked at another of Nabonidus' dates, which placed Naram-Sin of the Akkad Dynasty 3200 years before his time, hence around 3750 BC – a full one and half millennia higher than modern estimates would allow. Though Albright felt that the date was based on a genuine calculation, he saw this as a mistaken one, perhaps borrowed from the workings of the scholars at the court of the Assyrian king Sargon II (722–705 BC).

By the time of Nabonidus a competition as to which nation had the greatest antiquity had already started among the civilizations of the Near East. In a plausible tale, Herodotus (2.2–3) claims that Psammetichus I (664–610 BC) had already made an anthropological experiment, in which two infants were kept in complete isolation (and fed by a mute goatherd), to determine what was the original language of the human race – Egyptian or Phrygian. The Phrygians won (see James *et al*. 1991a, 291). A few generations later, in the reign of Amasis (570–526 BC), the Greek philosopher and politician Solon visited Egypt where he was castigated by the priests of Sais for imagining that the Greeks had any serious memory of history, calling them 'children' by comparison with the 8000 years of recorded history of which the Egyptians could boast (according to Plato *Timaeus* 22b; 23e). Elsewhere Plato (*Laws* 656–657) gave his own estimate of 10,000 years for the antiquity of Egyptian civilization. And in the late 5th century BC, Herodotus (2.142, 2) was given an even wilder figure from the Egyptian priests that he consulted: allegedly their records could trace a line of kings reaching back 11,340 years before his time.[8] In Hellenistic times Berossus of Babylonia and his contemporary Manetho both took up the cudgels of presenting their respective homelands as having histories of fantastic antiquity (James *et al*. 1991a, 292–293).

The point to stress here is that the 'dynastic race' between Egypt and other countries had already begun by the late 7th century BC. The ideas of the chronographers at the courts of Assyria and Babylonia, in the service of monarchs like Assurbanipal and Nabonidus (both of whom had antiquarian interests), need to be listened to with some scepticism when referring to periods much earlier than their own. We should have due care when the king lists drawn up by ancient Mesopotamian scholars are invoked, uncritically, by hand-waving scholars in defence of the standard chronology.

Kassite-Egyptian Synchronisms

A newcomer to the field of Mesopotamian chronology might be forgiven for assuming that the textbook Kassite history of the 14th–13th centuries has been reconstructed by recourse to cuneiform sources such as king lists. But most of the central part of the Kassite Dynasty is completely missing from the fragmentary king lists. Still, modern scholars have made use of what remains of Babylonian King List A (trans. Oppenheim 1969, 272). It picks up towards the later part of the Dynasty with some frustratingly fragmentary entries. Two lines give regnal years with no names, followed by the signs *K*[*a*-...] and *Ka*-*d*[*áš*-...], with the next monarch

[5] Perhaps added into the AKL at some point in the Middle Assyrian period (Brinkman 1995, 669–670).

[6] Yamada (2003, 268*) lists seven genealogical discrepancies in the AKL.

[7] The last named king is actually Kandal(anu), a vassal ruler of Babylonia under Assurbanipal, but six (or more?) lost lines follow which may represent the kings of the Neo-Babylonian Dynasty, bringing us down to the reign of Nabonidus (556–539 BC) – unless, that is, the lost lines included the colophon of the scribe (pers. comm. Ronald Wallenfels).

[8] For discussion of these figures see James (1995, 176–177, 273–274).

being given as Kudu[r-Enlil] before continuing with the last kings of the Dynasty (Grayson 1980, 91; Brinkman 1976, 16).[9] *That's it!* Hardly enough to justify the standard reconstruction of these names as belonging to the '13th century' monarchs K[adašman-Turgu] and his son Kad[ašman-Enlil] ('II')], in that order. Though, of course, they may well be those kings – my point being that it is unwise to place one's faith in fragmentary evidence, especially while the preceding lines are blank or almost completely blank. Overall I feel that the Babylonian King List A, possibly composed as late as the mid-sixth century BC should no longer be considered a primary source for reconstructing Kassite history (see James 2020, 153; Wallenfels 2022, 257, n. 5).

In lieu of king list information, we have contemporary Babylonian documents for ordering the sequence of kings and the synchronisms from letters between the Kassites and the Egyptians of the 18th–19th Dynasties. The following Kassite synchronisms are usually held to match, and hence mutually support, the conventional New Kingdom chronology:

14th century BC:

Kurigalzu I || Amenhotep III

Kadašman-Enlil I || Amenhotep III

Burnaburiaš II || Amenhotep III (?), Akhenaten and Tutankhamun

13th century BC:

Kadašman-Turgu || Hattusili III of Hatti || Ramesses II

Kadašman-Enlil II || Hattusili III || Ramesses II

Is the chronology of these Kassite rulers certainly attested in such a way that the synchronisms can be invoked in support of the conventional Egyptian chronology and/or *vice versa*? Or are we looking at the kind of circular arguments that Hanfmann warned of?

To take the last pair of kings first, their '13th century' date comes from a letter addressed to Kadašman-Enlil by Hattusili III of Hatti (KBo 1.10+KUB 3.72), the contemporary and ally of Ramesses II. Hence their date relies ultimately on Egyptian chronology. The kings of Karduniash, Kadašman-Enlil and his father Kadašman-Turgu are considered to be sole rulers of Babylonia – but there is some intriguing evidence that this may not have been the case. The letter of Hattusili clearly tells us about *another* power in Babylonia, one Itti-Marduk-balaṭu. Hattusili repeatedly complains about the interference of this individual in his missive to Kadašman-Enlil. Relations between Hatti and Karduniash had been friendly during the reign of Kadašman-Turgu, but Kadašman-Enlil had apparently been too young to know about the correspondence:

> Now are none of those scribes still living? Are the tablets not filed? Let them read those tablets to you now. I wrote these words to them with good intentions but Itti-Marduk-balatu – whom the gods have caused to live for far too long, and in whose mouth unfavorable words never cease – he froze my heart with the words he wrote to me: 'You do not write to us like a brother. You pressure us as if we were your subjects.' ... have the people of Hatti indeed ever pressured the people of Babylonia? I wrote to them with good intentions: 'The progeny of my brother Kadashman-Turgu shall be protected,' but Itti-Marduk-balatu wrote this to me. How did I write a malicious word to them, that Itti-Marduk-balatu should write these things to me? Indeed I wrote to them thus: '<lf> you do not protect the son of your lord in regard to rule, and if an enemy land arises against you, I will not come to your aid.' I have by no means taken the word of Itti-Marduk-balatu to heart. In those days my brother was a child, and Itti-Marduk-balatu, that evil man, spoke as he pleased. How should I take his word seriously?

The letter also complains about the activity of the Aḫlamû, a term for the Aramaeans, who were interrupting communications between Hatti and Babylonia:

> Furthermore, my brother: Because my brother wrote to me: 'Concerning my cutting off my messengers – since the Ahlamu are hostile I have cut off my messengers – how can this be, that you, my brother, have cut off your messengers on account of the Ahlamu? Is the might of your kingdom small, my brother? Or has perhaps Itti-Marduk-balatu spoken unfavorable words before my brother, so that my brother has cut off the messengers? In the land of my brother horses are more plentiful than straw. Should I indeed have dispatched a thousand chariots to meet your messenger in Tuttul, so that the Ahlamu would have kept their hands off?'[10]

The letter continues with a brief description of relations with Assyria, in which Hattusili states that the forces of Karduniash were far stronger and able to meet any threat from that quarter. This reads oddly in the standard model in which Kadašman-Turgu (*c.* 1281–1264 BC) and Kadašman-Enlil 'II' (*c.* 1263–1255 BC) are placed contemporary with the powerful Assyrian

[9] There is no contemporary inscriptional evidence that links them with the next king in the List, Kudur-Enlil. When Brinkman (1976, 203) refers to Kudur-Enlil's alleged filiation from Kadašman-Enlil 'II' (based solely on the order given in the King List) he follows it with a question mark.

[10] Trans. Beckman (1999, 140).

kings Adad-nirari I and Shalmaneser I (*c.* 1273–1244 BC). Further there is no evidence for these placements in terms of contemporary evidence for synchronisms (see below under **Wider Fallouts**). And rather than the Assyrians, the Aḫlamû/Aramaeans seem to have been of far more concern to Hattusili and Karduniash, and there is a strong implication that Itti-Marduk-balaṭu was colluding with them.

The letter reveals a unique situation. While Hattusili addresses himself to the young Kadašman-Enlil, real control seems to have been in the hands of the *eminence grise*, Itti-Marduk-balaṭu, who is usually assumed to have been a 'vizier' (see e.g. Bryce 2005, 167). Yet he seems to have been more than that. Hattusili addressed his remark about not sending military aid directly to Itti-Marduk-balaṭu, as if he were a king himself. The clues suggest that Itti-Marduk-balaṭu was an independent ruler based elsewhere in Babylonia. His name is familiar from other documents as that of the second king of the Second Dynasty of Isin,[11] usually dated to the late 12th century BC when the Aramaeans (Aḫlamû) were infiltrating north-western Babylonia. Itti-Marduk-balaṭu may himself have been of Aramaean origin. He is known from inscriptions as the grandfather of Nebuchadrezzar I and is conventionally dated some 130 years later than the vizier of Kadašman-Enlil.

Brinkman (1968, 97) noted a 'striking affinity' between the economic texts from the reign of Itti-Marduk-balaṭu (from an archive at Babylon) with those from Nippur 'several generations earlier', which may be no coincidence. In the published version of his PhD thesis on Mesopotamian chronology Pierce Furlong suggested that the two Itti-Marduk-balaṭus were actually the same person.[12] This eminently plausible suggestion would mean that the pair Kadašman-Turgu and Kadašman-Enlil (from the letter of Hattusili) should be removed from the main Kassite dynasty and seen as later kings of Karduniash, probably based at Nippur and ruling in parallel with the early Second Dynasty of Isin (conventionally mid-12th century BC onwards) who were focussed on the area around Babylon.[13] The idea

Figure 1. Limestone entitlement stele ('kudurru') *of Nebuchadrezzar I (conventionally dated* c. *1125–1104 BC), the fourth and most powerful monarch of the Second Dynasty of Isin, whose effective founder was Itti-Marduk-balaṭu (photo: Gary Todd, Wikipedia Commons, free domain).*

[11] For his texts see Brinkman (1968, 94–98).

[12] Furlong (2010, 56–57). Many years ago Pierce Furlong, then a graduate student in ancient history in Australia, contacted me about the theory proposed in *CoD*, and I encouraged his postgraduate work on Mesopotamian chronology. From our correspondence and meetings I found that we had been working in parallel on the idea of a relative shift in Kassite chronology by about a century with respect to Assyrian chronology, as expressed in the published version of his thesis. Furlong also gave a lengthy paper on his revised model at the Second BICANE meeting held in Cambridge in June 2009. While Furlong made many valuable observations, his rigid adherence to a precise 200-year reduction in Egyptian chronology (as opposed to the roughly 245 years of the *CoD* model) leads to problems in his model (cf. e.g. Wallenfels and James, 'Discussion 1', p. 29, elsewhere in these Proceedings).

[13] Regarding his father Marduk-kabit-aḫḫešu, purportedly the founder of the Second Dynasty of Isin, traces of his name in Babylonian King List A are uncertain. He is described as a 'shadowy figure' with no known inscriptions and is really only known from Babylonian King List C (Brinkman 1968, 40; Grayson 1980, 97). There he is accorded a reign of 18 years (and Itti-Marduk-balaṭu 7 years). As he preceded Itti-Marduk-balaṭu his reign (perhaps at Isin) may have overlapped with that Kadašman-Enlil; it seems reasonable to count his undocumented reign as ephemeral and of little or no chronological significance.

of parallel dynasties within Babylonia for this period should hardly seem surprising. It is clear, for example, that the earliest Kassite dynasts reigned in parallel with the last kings of the First Dynasty of Babylon (whose best-known ruler was Hammurapi),[14] as well as the First Dynasty of the Sealand (see Wallenfels' 'Introduction to Session One', in these Proceedings, p. 16).

The Amarna Kassites

If the '13th century' pair Kadašman-Enlil and Kadašman-Turgu have been displaced too early in time by a full century, could the same have happened with the earlier '14th century' group of Kassite monarchs, those known from the Amarna letters? Four names are known, given here in the order which is self-evident from the internal evidence of the correspondence:

1. Karaindaš – EA 10:8 from Burnaburiaš refers to him as a predecessor – in whose time friendly relations between Egypt and Babylonia first began (Moran 1992, 19).

2. Kurigalzu – In EA 9:19, addressed to either Akhenaten or Tutankhamun, Burnaburiaš refers to Kurigalzu literally as 'my father' (*a-bi-ia*), rather than *aba/i abi* 'grandfather'. Moran (1992, 18) suggested rather here *abu* means 'ancestor', as it may also in EA 16:19 (Moran 1992, 39; Knudtzon 1915, 1361, s.v. I *abu*). [However, earlier in EA 9 (l. 7) Kurigalzu's references to his and Pharaoh's ancestors are written correctly as plurals *ab-bu-ú-a* 'my ancestors' and *ab-bu-ka* 'your ancestors'. Further, in EA 11:19–23, Burnaburiaš speaks clearly of Kurigalzu, a contemporary of Pharaoh's father, as if it were well-understood that Kurigalzu was Burnaburiaš's father; the common restoration (*a-bi-ia*)'my father' at the end of l. 20 seems warranted (ed.).] This would mean that Kurigalzu was the contemporary of Amenhotep III (or Akhenaten).

3. Kadašman-Enlil – Four letters (EA 1–3, 5) are known between this ruler and Amenhotep III (Moran 1992, 1–8, 10–11). EA 3 refers to correspondence between Kadašman-Enlil's father (unnamed) and Amenhotep III.

4. Burnaburiaš – Six letters: EA 6 appears to be addressed by this ruler to Amenhotep III, EA 7–8 and 11 to Akhenaten (Amenhotep IV) and EA 9–10 to either Akhenaten or more likely Tutankhamun (Moran 1992, 12–20).

5. King [*X*] – Two documents survive in which the names of both correspondents are missing. One (EA 4) contains a demand for a bride as well as gold. The Babylonian correspondent is usually assumed to be Kadašman-Enlil, though as Moran (1992, 9, n. 1) stresses this is 'not completely certain'. The second (EA 13) concerns an inventory of gifts, 'apparently the dowry of a Babylonian princess' (Moran 1992, 26, n. 1). These two documents could belong to either Kadašman-Enlil or Burnaburiaš, or one or both might belong to an otherwise unknown Kassite correspondent.

The Amarna letters were assembled and housed at the short-lived capital of Akhetaten (Tell el-Amarna) by Akhenaten's scribes and included correspondence of his father Amenhotep III. But it is very important to remember that the collection as we have it was not an excavated archive like that at Ras Shamra (Ugarit).[15] The 'Amarna Letters' are a completely random selection from the original archive, those that came into the hands of museums through local diggers and then antiquities dealers. Many were damaged, destroyed or lost as they were carted off to Cairo on donkeys. As Moran stressed, '... we will never know how many tablets may have been found and later lost or destroyed.'[16] We are actually extremely fortunate to have the sample that has been preserved. There may well have been other Mesopotamian rulers represented in the original archive; so we should not expect to find in the collection a complete record of the Kassites who had dealings with Akhenaten and his family. For all we know, King [*X*] might represent the tip of an iceberg of missing Babylonian correspondents. 'Taphonomy' will have played its part and given that the tablets were looted/removed in batches, the entire correspondence of some kings may have been lost selectively through certain processes (see e.g. Brosius 2003, 4–16).

The following order of Kassite rulers – either correspondents or mentioned retrospectively – is clear from the internal evidence of the letters: Karaindaš, Kurigalzu, Kadašman-Enlil and Burnaburiaš (II)[17], the last three apparently members of the same family or dynasty. How do the names and order of the sequence we have from the surviving Amarna letters fit with the evidence of native Babylonian sources? There is little to corroborate each step but the succession Kadašman-Enlil to Burnaburiaš seems confirmed by two fragmentary inscriptions on lapis lazuli blocks, one from

[14] See however Mahieu (2021b, 99–100) who argues for no overlap between the First Dynasty of Babylon and the Kassites – though she argues for a dual line within the Kassite Dynasty itself.

[15] Cf. the shambolic state of the records from Hattusa (Boğazköy) in central Anatolia – see **Hittite Synchronisms** below.

[16] Moran (1992, xiii); see in some detail Mynářová (2015).

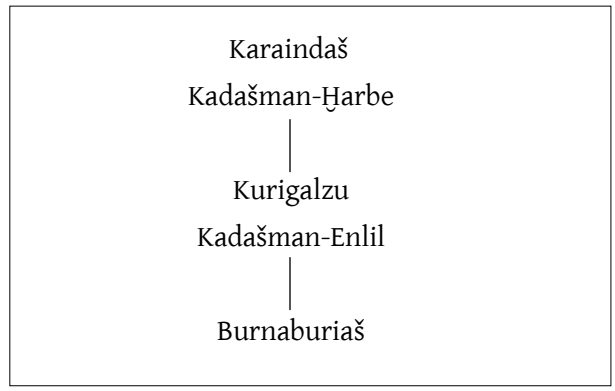
[17] This Burnaburiaš is referred to as the second to distinguish him from the first Burnaburiaš, who reigned much earlier in the Kassite Dynasty.

Susa, the other from Nippur.[18] This is about as far as we can go with contemporary Babylonian confirmation of the Kassite succession apparent from the letters.

As Karaindaš seems to be described as a remote predecessor of Burnaburiaš in EA 10, there would seem to be room in the sequence for a Kadašman-Ḫarbe, the father of Kurigalzu, known from a number of contemporary texts (as well as *Chronicle 21* – see below). *Nota bene,* I am far from being the first to argue this – it was once commonplace amongst scholars such as the eminent Assyriologists D. D. Luckenbill (1907, 281), A. T. Olmstead (1920, 131–132) and Petrus van der Meer (1947), as well as M. B. Rowton and C. J. Gadd in the *Cambridge Ancient History* no less.[19] Matters changed with what one might call the 'Brinkman revolution'. Through seminal publications such as his 1976 handbook *Materials and Sources for Kassite Study* I (= Brinkman 1976) and, presumably more widely turned to, the chronological tables for Assyria and Babylonia he provided for Leo Oppenheim's semi-popular *Ancient Mesopotamia: Portrait of a Dead Civilization* (first ed. 1964, revised 1977). There, Brinkman's king list excluded Kadašman-Ḫarbe as the father of Kurigalzu, relegating him to an earlier period (see below) and replacing him with a Kara-hardaš known only from Assyrian *Chronicle 21* (see below). The core of these issues is the notorious and tangled 'Kurigalzu problem' which Brinkman bravely attempted to resolve.[20]

I will, however, attempt by degrees to show how Brinkman's solution ultimately relies on the kind of circular arguments of which Hanfmann warned – to reverse the 'Brinkman revolution' and offer tentative beginnings towards a new model for the middle to late Kassite Dynasty.

The genealogical relationship between Kadašman-Ḫarbe and Kurigalzu of *Chronicle 22* is supported by five inscriptions: a clay prism and its duplicate mentioning a Kurigalzu son of Kadašman-Ḫarbe; two clay cones (with the same inscription) from the reign of a Kadašman-Enlil confirming a land grant made in the time of Kurigalzu son of Kadašman-Ḫarbe; and a legal text, possibly from the reign of Nazi-Maruttaš, also referring to Kurigalzu son of Kadašman-Ḫarbe.[21] There is no good reason to doubt that the same pair of kings is referred to in each instance. Regarding the clay cones of Kadašman-Enlil confirming a land grant made in the time of Kurigalzu son of Kadašman-Ḫarbe (Brinkman 1976, 136, J. 2.19), Brinkman noted that 'there is no compelling reason' here to chose between Kadašman-Enlil I and II – though the distance in time between Kurigalzu 'I' and Kadašman-Enlil II may make this option less likely. Allowing this, we can allow for the pre-Brinkman understanding and reasonably arrive at the following sequence[22] for the Amarna Kassite correspondents and their immediate predecessors:



Chronicle 21 vs. *Chronicle 22*

Beyond this we have to dive into the shark-infested waters of later sources to flesh out the picture. It is not a recommended strategy, and one I have strenuously avoided in reconstructing the history of the Egyptian Third Intermediate Period. But there are two good, in fact vital, reasons for looking into them with respect to Mesopotamia. First, the Chronicles are very different from the king lists which simply give name after name with little or no political information. By contrast, the Chronicles give narrative history, and it is legitimate to take clues from them and then see if they can be corroborated or contradicted by information from contemporary records. Second, and more importantly, the Neo-Assyrian *Chronicle 21* (also known as the *Synchronistic History*) *has been liberally used by scholars* (notably Brinkman)*, to reconstruct the currently popular history of this period and provide a fit with the Amarna letters*. This has been done at the expense of rejecting the information from *Chronicle 22* which gives a largely

[18] Susa: (4) *bur-na-b*[*u-ri-ia-aš*] (5) LUGAL [...] (6) DUMU *kad-aš-ma-an-*ᵀᵈ[EN.LÍL] (Frame 1987, 6, no. 3); Nippur: (6) [*bur-na-bu*]*-ri-ia-aš* ... (14) DUMU.SAG *kad-aš-ma-an-* (15) ᵈEN.LÍL (Brinkman 1976, 107, E.2.7 and n. 32). The latter inscription identifies Burnaburiaš as the eldest son of Kadašman-Enlil. While not entirely ruling out the possibility that the king's name here might be restored as [Šagarakti-Šu]riaš, Brinkman notes that this would have to be squeezed in the limited space available; further the qualification 'eldest son' would suggest a monarch who immediately succeeded his father, which would not favour Šagarakti-Šuriaš. See below where I suggest that Burnaburiaš could have been the eldest son of Kadašman-Enlil IIa.

[19] Rowton (1970, 205); Gadd (1975a, 29).

[20] Brinkman (1969, 320–327) sets out the 'Kurigalzu problem' clearly. A full discussion of all the literature relating to this would occupy another paper entirely. Nonetheless, it is clear that the intention of the scribe who drew up *Chronicle 22* was that the father of Kurigalzu, great-grandson of Aššur-uballiṭ, was Kadašman-Ḫarbe (see below).

[21] Brinkman (1976, 147, Kᵃ.3.1–3; Q.2.1.1, 2); Oshima (2012, 253).

[22] Recently Mahieu (2021b, 97) has come to the same conclusion that a Kadašman-Ḫarbe can be safely inserted into the Amarna sequence; we differ in that she thinks that he was the father of Kurigalzu I rather than 'II' as argued here.

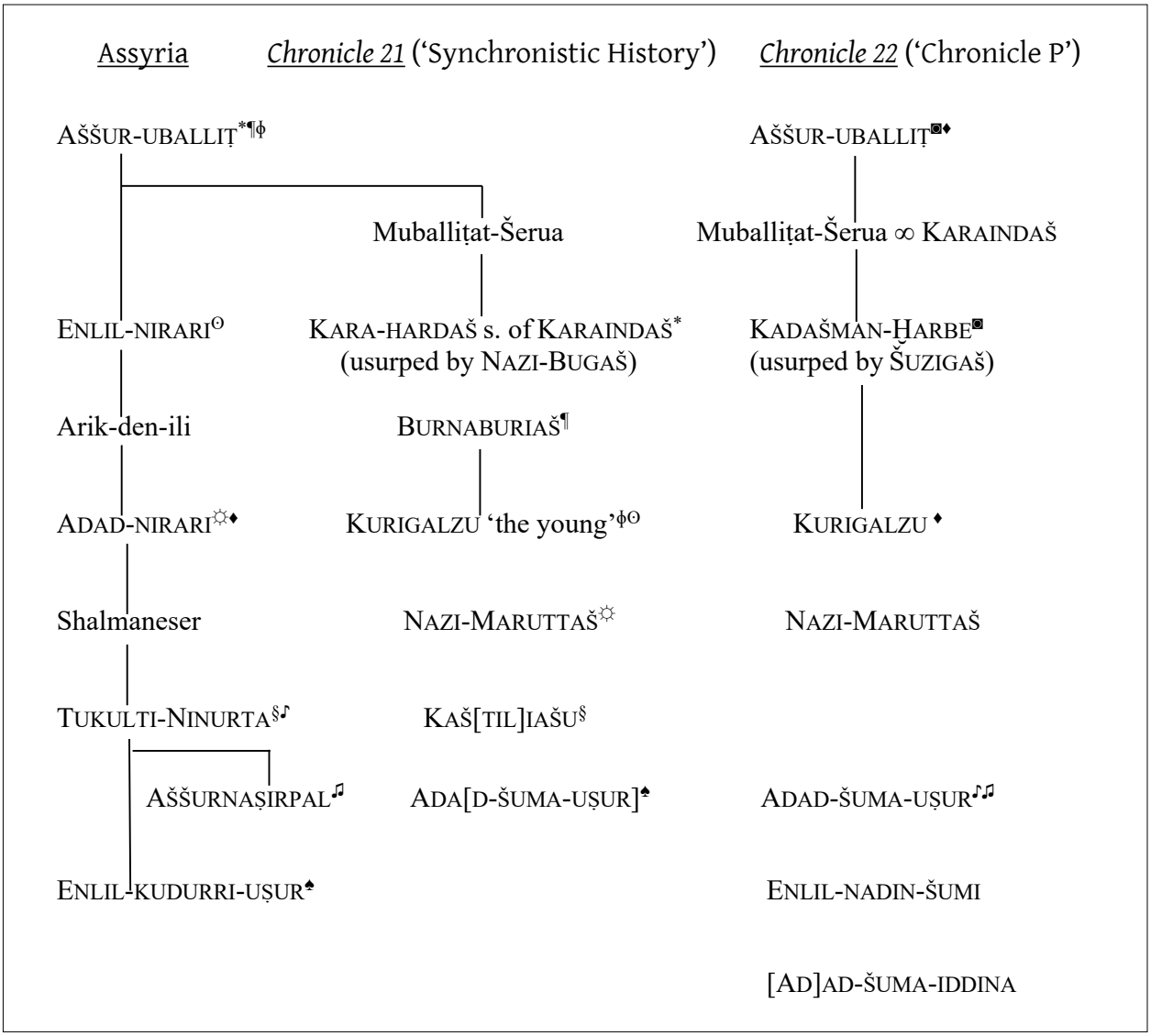


Figure 2. The Middle Kassite rulers according to Chronicles 21 *and 22. The lineage of Assyrian kings follows their contemporary inscriptions. Only kings mentioned in the* Chronicles *are given* (small caps)*; attested synchronisms are shown with superscript symbols:* Chronicle 21: * ¶ ϕ ʘ ☼ § ♠ ; Chronicle 22: ◙ ♦ ♪ ♫

parallel account, but which was written in Late Period Babylonia. One knows only too well how the history of one country can be distorted by that of a rival, with the intention of establishing dynastic or territorial claims as well as self-glorification. As a modern analogy, Grayson (1975, 50) cited the differing accounts of the nature and result of the war between the United States and the United Kingdom in AD 1812–1814.[23]

How does a comparison of the two documents pan out when examined through the lens of contemporary evidence? Tabulated above are *Chronicle 21* ('The Synchronistic History') and *Chronicle 22* ('Chronicle P'). Contemporary kings of Assyria are given in a column on the left.[24] The synchronisms given by the two *Chronicles* differ.

[23] Despite the claims of some historians, according to others the British technically won by burning down the President's Residence in Washington (now the White House) – a parting gift in return for Madison's invasion of Canada and declaration of war in favour of Napoleon.

[24] The tabulation given by Grayson (1975, 211) of these relationships is unsatisfactory. For *Chronicle 22* ('P') he tentatively makes Karaindaš the son of Muballiṭat-Šerua, which, by introducing another generation, would make Kurigalzu (unfeasibly) the great-great-grandson of Aššur-uballiṭ, rather than great-grandson. Line i 12 of *Chronicle 22* names Kadašman-Ḫarbe the son of Aššur-uballiṭ's daughter. This enables us to read clearly the earlier statement (i 5–6): '[Kadašman-Ḫar]be, son of Karaindaš, son of Muballiṭat-Šerua – [daughter] of Aššur-uballiṭ, king of Assyria', i.e. Kadašman-Ḫarbe was the son of Karaindaš and Muballiṭat-Šerua. The name Kurigalzu is restored in *Chronicle 22* (l. i 14′), as per most scholars including Grayson (1975, 172) and Glassner

Clearly, both versions cannot be right. But to which version should we give more credence? Grayson (1975, 58) summarised the arguments in favour of *Chronicle 22* ('Chronicle P'):

> In general Chronicle P is a more reliable document than the Synchronistic History [*Chronicle 21*]. The latter, as shown in the discussion of that text, is replete with errors of all kinds and is notorious for selecting or even distorting facts in such a way as to shed a more favourable light on Assyria. The Babylonian document, Chronicle P, on the other hand, contains no significant errors and far from omitting facts dishonourable to Babylonia, mentions four Babylonian setbacks. Thus as a general principle one should, in cases of disagreement, lend more credence to the version of Chronicle P.

Jean-Jacques Glassner (2004, 50) put it more bluntly:

> Space does not permit me to list all the errors and omissions in this chronicle [*Chronicle 21*], such as Nabû-šuma-ukīn being erroneously called Nabû-šuma-iškun, where a simple comparison with chronicle 45 [= Grayson *Chronicle 22*], which deals with the same period, is informative. I merely note the deliberate falsification of the facts to which the author did not hesitate to resort.

Thus the odds are heavily stacked against the Assyrian version (*Chronicle 21*) being correct. It is internally inconsistent, referring to the grandson of Aššur-uballiṭ as Kara-hardaš in one line and as Karaindaš in another. The reading Karaindaš (as in *Chronicle 22*) is highly preferable, as it is otherwise attested as a Kassite royal name: Kara-hardaš is not. Nor is there any inscriptional evidence for Kara-hardaš who I have argued elsewhere is a phantom who should be deleted from the modern king lists.[25] Further, *Chronicle 21* makes little sense when it relates that Aššur-uballiṭ invaded Babylonia to avenge his grandson Kara-hardaš/Karaindaš, and then states that he placed Kurigalzu, son of a hitherto unmentioned Burnaburiaš, on the throne. Though it is fragmentary, there are no internal inconsistencies in the *Chronicle 22* account: Aššur-uballiṭ invades to avenge his deposed grandson Kadašman-Ḫarbe and places the latter's son Kurigalzu (called 'the young' in *Chronicle 21*) on the throne. The whole point of the narrative, surely, was to show how Aššur-uballiṭ attempted to secure his line on the Babylonian throne by installing his blood-descendant Kurigalzu.[26]

Ian Mladjov (2016a, 2016b) published two valuable articles on the controversy in *Nouvelles Assyriologiques Brève et Utilitaires* (= *N.A.B.U.*). While strongly in favour of the probity of the Babylonian account (*Chronicle 22*), Mladjov ran into complications as soon as Burnaburiaš and the evidence of the Amarna letters entered the picture, and he had to reject a key detail in the *Chronicle 22* account – namely that Kurigalzu was the son of Kadašman-Ḫarbe. Mladjov instead argued that the lacuna in Chronicle P (l. i 14) containing the relationship between Kurigalzu II and Kadašman-Ḫarbe should be restored not as 'son' (*mār*) but rather as 'father's brother' (*aḫi abi*). This would allow Burnaburiaš to be the father of Kurigalzu (as per *Chronicle 21*). Mladjov's rather elaborate emendation is unlikely to gain much favour. Various considerations (summarised in n. 24 above) make it a near certainty that *Chronicle 22* considered Kurigalzu as the son of a Kadašman-Ḫarbe. Further, in Mladjov's model Burnaburiaš appears somewhat out of the blue, his origin and claim to the throne unexplained.

The Burnaburiaš question also loomed large when Brinkman (1976, 15) offered what he saw as a major point in favour of the Assyrian version (*Chronicle 21*). He argued that the standard placement of Burnaburiaš follows '... from the fact [sic] that he is known to have been the father of Kurigalzu II (king No. 22) as well as the approximate contemporary of Aššur-uballiṭ I of Assyria (who placed Kurigalzu II on the throne)', adding (n. 28) '[t]hey both wrote Amarna letters to Akhnaton (e.g. EA 11, 16).'

See also Brinkman's courageous attempt to cut through the tangle of the notorious 'Kurigalzu problem' (Brinkman 1969, 323–324), in which the 'chain of reasoning' takes as its starting point the position of Burnaburiaš known from the Amarna letters. Here, *we get close to the main reason why* Chronicle 21 *is preferred by some scholars for this period*. I.e. as we already 'know' from the Amarna letters that there was a Burnaburiaš ruling in Babylonia at the time of Aššur-uballiṭ, then the *Chronicle 21* tradition immediately looks more plausible than that of *Chronicle 22*, which omits such a Burnaburiaš.

Wider Ramifications

In the *Chronicle 22* sequence there is no Burnaburiaš during the time of Aššur-uballiṭ, but a Kadašman-

(2004, 278) as: [*i-duk* ^{I}*Ku-ri-gal-zu* DUMU ^{I}*Ka-dáš*]-*man-Ḫar-be*. This is justified to the point of certainty by a number of considerations: (1) though the patronymic is partly restored, the *-Ḫarbe* can only reflect the name Kadašman-Ḫarbe; (2) Kurigalzu is the next king mentioned in both *Chronicle 21* (ll. i 16′, 18′) and *Chronicle 22* (ll. ii 10ff.); and the relationship between Kadašman-Ḫarbe and Kurigalzu is attested in contemporary inscriptions. See further Grayson (1975, 212).

[25] See James (2020, 154, 156, n. 6); in agreement with Mladjov (2016a, 19–20).

[26] Nevertheless, Röllig (1967) and Brinkman (1969, esp. 323, n. 1; 1976, 418–423) have both defended the Assyrian version in accepting details such as making Kurigalzu II the son of Burnaburiaš (II). Röllig's arguments were analysed in detail by Grayson (1975, 58, n. 69) and found wanting: 'Certainly there are scribal errors in Chronicle P but there are also scribal errors in the *Synchronistic History* .... By itself, therefore, this point cannot decide the issue in favour of either document.'

Brinkman (1976/1977)	*Chronicle 22*	Assyria
*Karaindaš *c.* 1413 BC		
*Kadašman-Ḫarbe		
*Kurigalzu I		
Kadašman-Enlil (1374)–1360		
Burnaburiaš (II) 1359–1333		Aššur-uballiṭ I 1363–1328
Kara-hardaš 1333	*Karaindaš	Enlil-nirari 1327–1318
	*Kadašman-Ḫarbe	Arik-den-ili
Kurigalzu II 1332–1308	*Kurigalzu (‘II’)	Adad-nirari I 1307–1275
Nazi-Maruttaš 1307–1282	Nazi-Maruttaš	Shalmaneser I 1274–1244
	Adad-šuma-uṣur	Tukulti-Ninurta I 1243–1207

Figure 3. A comparison between the order of the Middle Kassite rulers per Brinkman (1976/1977) and Chronicle 22*.*

Ḫarbe instead. Nor is there room for a Burnaburiaš at this point in the narrative, where Karaindaš and Kadašman-Ḫarbe are given as the immediate forebears of Kurigalzu. In order to make room for the Amarna Burnaburiaš, Brinkman’s chronology had to relegate Karaindaš and Kadašman-Ḫarbe to a time *before* the reign of Aššur-uballiṭ I, with Karaindaš placed *c.* 1413 BC (Brinkman 1976, 31) and Kadašman-Ḫarbe identified as ‘the father of the pre-Amarna Kurigalzu’ (Brinkman 1976, 146), with the following result – compared to the sequence in *Chronicle 22* (see *Figure 3*).

Here we see a strange mirroring of two sequences of three identically named kings (asterisked). While homonyms were common in Kassite kingship, three in a row seems less likely, suggesting that there might be something fundamentally wrong here with Brinkman’s reconstruction.

The pre-Brinkman position was to take the evidence of the inscriptions (see above) as corroboration of relationship between Kadašman-Harbe and Kurigalzu ‘II’ as given in *Chronicle 22* (see above). As Petrus van der Meer (1947, 16) summarised:

> We thus possess two accounts of the same event, one from the Assyrian side in the synchronistic history and the other from the Babylonian side in chronicle P. They agree in substance with one another, but differ as regards the name .... Now since the Babylonian chronicle reproduces the Babylonian tradition and as regards the correctness of names is in a better position than the writer of the Assyrian synchronistic history, who reproduces the Assyrian point of view and was not so well informed as regards the names, since he had less easy access to the Babylonian archives, the Babylonian chronicle P inspires more confidence with respect to the correctness of the names. So we can reconstruct the run of events thus ... Kadasmanharbe. He in turn had a son, Kurigalzu, as is clear from the Babylonian chronicle .... The Assyrian synchroniser calls Kurigalzu a son of Burnaburiash, who was really his great-grandfather.

But why did *Chronicle 21* refer to Burnaburiaš as the father of Kurigalzu, if this is incorrect? Radau (1908, 64–67) argued that when *Chronicle 21* called this Kurigalzu the ‘son’ of Burnaburiaš it was to identify him as a descendant of that king and not as his immediate offspring. The motive would have been to avoid Kurigalzu’s connection to the unpopular and ill-fated Kadašman-Ḫarbe – who was usurped by Šuzigaš during an uprising of the Kassites (according to *Chronicle 22*). Mladjov (2016a, 19) remarked:

> Indeed something like this could be discerned in the inscriptions of later Assyrian kings, where Sîn-aḫḫē-erība (Sennacherib) avoided mention of his father Šarru-kīn (Sargon) II, who had been felled in battle.... In principle, Radau’s solution could resolve the discrepancy.[27]

An analogy closer in time was cited by Radau (1908, 65, n. 2): in one inscription the late Kassite king Meli-Šipak is called ‘son’ of Kurigalzu (BE 6376; Brinkman 1976, 254, S.2.3), whereas in another document

[27] However, Mladjov continued: ‘But if Kurigalzu II really was the son of the Babylonian grandson of Aššur-uballiṭ, it would have been very odd for the Assyrian text to omit that point, and to associate Kurigalzu II with a hitherto unmentioned more distant ancestor instead!’ This apparently strong point breaks down on closer analysis. While it was composed in Assyria, *Chronicle 21* is likely to have been based on a Babylonian original which would have included this detail. To assume that *Chronicle 21*, which is replete with inconsistencies, was thoroughly edited enough to iron out every detail expects too much. As Mladjov (2016a) himself notes: ‘Surely it is not difficult to suppose that Assyrian scribes, especially if supplying the information at a later time, had trouble with relatively unfamiliar Kassite names and with the details of Babylonian history.’

Figure 4. The remains of the ziggurat at 'Aqar-Qūf, site of the ancient Kassite city of Dur-Kurigalzu. While it is now generally assumed that the city was founded by (and named after) Kurigalzu I rather than Kurigalzu II, this remains a moot point (photography by Hanay, Wikipedia commons, free domain).

(*kudurru* BM 90827) he is called the son of Adad-šuma-uṣur.[28] Presumably Meli-Šipak wished to reinforce his authority by claiming descent from a more famous predecessor than his father. The same could apply to some of the inscriptions of Kurigalzu II which name his father as Burnaburiaš rather than Kadašman-Ḫarbe.[29] But rather than assuming that this Burnaburiaš was the grandfather (Radau) or great-grandfather of Kurigalzu (van der Meer), it seems more likely that he was a more remote ancestor – Burnaburiaš I, son of Agum, the first Kassite king to really rule over Babylonia, and the effective founder of the Dynasty.

As an interim conclusion, it would seem that it was the interposition of the Amarna Burnaburiaš (II) into the mid-Kassite sequence that has led to the preference (by some scholars) of the narrative of *Chronicle 21* over that of *Chronicle 22*. Given this, we should note that *Chronicle 22* does not place any of the four known Kassite kings from Amarna in the 'right' place for the standard chronology. In fact in *Chronicle 22* the first two Kassites known from the Amarna letters (Karaindaš and Kurigalzu) fall a century after their conventional placement.

A New Historical Link?

Crucial to the questions raised here is the identity of the Kurigalzu mentioned in the Chronicles. There is an important historical consideration which supports the identification of the Amarna Kurigalzu with Kurigalzu 'II' called *ṣeḫru*, 'young', in *Chronicle 21*.[30] *Chronicle 22* echoes lines from an epic poem about this Kurigalzu, lauding him as an energetic warrior. First he subdued a coastal country, most likely the 'Sealand' of southern Babylonia (Grayson 1975, 224), where the conquered people were said to exclaim: 'We did not know, Kurigalzu, that you had conquered all peoples.' Next he is said to have defeated king Hurpatila of Elam and subdued his country, to be followed by a battle at Sugaga on the Tigris where he trounced the Assyrian

[28] As Brinkman (1976, 104–105) notes, the patronymic Kurigalzu here should not lead us to the 'sheer implausibility' of adding an extra Meli-Šipak into the king list (see further, n. 27, above).

[29] Also suggested by Furlong (2010, 55): 'To account for the fact that these votive inscriptions [of which at least a dozen are known, ed.] describe Kurigalzu II as a "son" of Burnaburiash, as does the Synchronistic History, I would suggest that this genealogical relationship was stressed due to the inglorious fate of his half-Assyrian biological father, Kadashman-Harbe I, who was deposed and murdered by the usurper Nazibugash/Shuzigash.'

[30] The term is sometimes translated as 'the younger', as if to differentiate him from an earlier Kurigalzu. [Note, however, Akkadian does not distinguish between the comparative and simple form of the adjective (ed.).] The explanation of Radau (1908, 64) is more likely: that when he was placed on the throne by Aššur-uballiṭ he was still a child, 'seeing that his great-great-grandfather, the Assyrian king Ashshur-uballiṭ was still living'.

king Adad-nirari I (Grayson 1975, 175) – Sugaga, is only 5km from the capital Ashur.[31] Taken together these would be remarkable achievements which would have made Babylonia for a short while the pre-eminent military power in Mesopotamia, if not the Near East. (See later regarding the apparent political strength of Burnaburiaš.)

Compare this with the information about the recent past given in Amarna letter EA 9 written by Burnaburiaš (most likely to Tutankhamun). It states that 'all the Canaanites' had written to his father (*a-bi-ia*) Kurigalzu asking him to come to their border to support a rebellion against Egypt. Kurigalzu refused and threatened to plunder the Canaanites if they rebelled against Egypt and allied with anyone else. It would clearly have needed a king with an awesome military reputation to provoke such an appeal – and one with enough reach that the Canaanites could imagine that he could march through western Mesopotamia or the Syrian desert in order to help their rebellion. There is thus a tempting match here between Kurigalzu 'the young' of *Chronicle 22*, and the father of Burnaburiaš referred to in EA 9.[32]

Interestingly there is a reference to Dur-Kurigalzu in a toponym list of Amenhotep III from his mortuary temple at Kom el-Hetan. It appears next to a cartouche with the name 'Babylon' and opposite one with 'Sangar' (Babylonia).[33] About 30km west of Baghdad remains of the great ziggurat of Dur-Kurigalzu (at modern *'Aqar Quf*), dedicated to the god Enlil, are still standing (see *Figure 4*). It thrived as a city and possibly the capital through much of the Kassite period. As a reference to Dur-Kurigalzu has only been identified in Egyptian sources from the time of Amenhotep III, this matches perfectly with the fact that a Kurigalzu was on friendly terms with Egypt during the half-century prior to Amarna times. It would also echo the reputation of the Kurigalzu retrospectively described by Burnaburiaš and the military prowess of Kurigalzu 'the young' lauded in *Chronicle 21*.

I offer the identification of Kurigalzu *ṣeḫru* of the Chronicles with the Kurigalzu mentioned by Burnaburiaš in EA 9 (*scil.* Kurigalzu I *vis*-à-*vis* Kurigalzu II son of Burnaburiaš, well-known from the monuments) as a new synchronism produced by a shakeup of Kassite chronology, along with that of the 'vizier' Itti-Marduk-balaṭu with the Isin II king of that name (see above). And, like that, it would suggest that on the order of a century's shift in relative chronology between Kassite Babylonia and Middle Assyria is required.

Kurigalzu I or II?

As argued above, the already-ancient confusion between Kurigalzu 'I' and 'II' may be one of the root causes about the dispute over which of them founded Dur-Kurigalzu. Amongst others, Clayden (1996, 113) argues that it was Kurigalzu I that built the city, though allowing that Kurigalzu II may have refurbished it and that: 'Of the several dozen or so other building inscriptions found at Dur-Kurigalzu, none provides conclusive evidence as to which Kurigalzu founded the city.' And a key argument employed by Clayden (1996, 113; repeated in Bartelmus 2010, 148–149) for the priority of Kurigalzu I is equivocal. It is based on the evidence of an extispicy report from Nippur which refers to Dur-Kurigalzu:

> The crucial point is that the text is dated II-1-year 11 of Burna-Buriash (rev. 1–3; Brinkman 1976: 115: E.2.26). This cannot have been Burna-Buriash I, as he reigned before Kurigalzu I. Therefore it must refer to Burna-Buriash II (1359–1333 B.C.), the father of Kurigalzu II (1332–1308 B.C.), i.e. at least fifteen years after the death of Kurigalzu I in 1375 B.C. If Dur-Kurigalzu existed in the reign of Burna-Buriash II, then Kurigalzu I must have founded the city.

As it is argued here that the second Burnaburiaš did *not* precede the Amarna Kurigalzu but followed him, this would remove Clayden's point in favour of Kurigalzu I as being the founder of Dur-Kurigalzu. The earlier idea (see e.g. Olmstead 1920, 132) that it was Kurigalzu II who built the city should not be dismissed lightly. There is also a cylinder recording the building achievements of a Kurigalzu son of Kadašman-Ḫarbe which praises his work at Uruk, Ur and Eridu but with no mention of Dur-Kurigalzu.[34] Unless the text is from very early in his reign, perhaps the inscription is from a time before the founding of the latter and belongs to the first Kurigalzu rather than the second.

But all of this raises the question: who was the Kurigalzu *ṣeḫru* of the Chronicles? Usually assumed to be the second of that name there is also the possibility that he was the first.[35] This might resolve any arguments over who built Dur-Kurigalzu and raises some interesting chronological ramifications. Beyond the scope of the

[31] The *Chronicle 21* version names the Assyrian adversary at the battle as Enlil-nirari, and claims that it was a defeat for the Babylonians. But the fact that it also notes a subsequent treaty setting a boundary between the two states suggests at least a draw.

[32] The identity of the Kurigalzu who defeated Hurpatila of Elam has naturally become embroiled in the 'Kurigalzu problem'. The poetic rather than chronicle-like style of the entry in Chronicle P reveals it to be quoted from an earlier composition, and it has been argued (Vallat 2006) that it was taken out of context by the chronicler and originally referred to Kurigalzu I.

[33] See Görg (1989a, 1989b); van der Veen *et al.* (2010, 18); Clayden (2017, 430 and Fig. 16.09).

[34] Recollated by Oshima (2012, 253–257, 262–264, Pls. 5–10).

[35] I am grateful to Ronald Wallenfels for this suggestion (cf. note 30, above).

present essay, the aim of which is to sketch out the broader historical problems, I leave these open for further investigation.

The Elamite 'Berlin Letter'

Coming back to Burnaburiaš, further evidence of a king of that name *after* the Amarna Kurigalzu ('I'; presumed *ṣeḫru*) comes from a Neo-Babylonian or Achaemenid copy of a '12th century BC' Elamite royal letter to Babylonia, now in Berlin (VAT 17020).[36] It describes relations between Elam and Babylonia, concentrating on their dynastic marriages, including those of a Kurigalzu and a Burnaburiaš. The 'Berlin Letter' was composed in the name of an Elamite king, either Kutir-Nahhunte or perhaps more likely Shutruk-Nahhunte (Potts 2016, 224), who is proffering his candidacy to become king of Babylon during a time of dynastic troubles – on the grounds that he is the descendant of the daughter of 'mighty King Kurigalzu' (VAT 17020, rev. 12). The 'Letter' was presumably written before an Elamite invasion of Babylonia and despite its extremely fragmentary condition, it may offer potentially valuable evidence for the reconstruction of the Elamite Igihalkid Dynasty, as noted by Singer (2008, 392):

> Scholars have raised justifiable doubt about the historicity of these literary compositions (e.g. Brinkman 2004: 292) but one has to admit that the author of the 'Elamite Letter' had a good knowledge of the Babylonian line of succession, going back as early as Kurigalzu (I)... He must have had access to historical sources and the information provided him on three negative precedents in which the Babylonian throne was imprudently given to non-Elamite descendants may contain a kernel of truth.

The opening lines of the preserved text are as follows (after Roaf 2017, 182–183):

6′ Pihiranu-^{d}U [married the sister/eldest daughter (?)]
of the mighty king Kurigalzu.
Humban-immeni [married] his daughter;
she gave birth to Hundasha-^{d}GAL. Hunda[sha-^{d}GAL]
10′ mar[ried] the daughter of Burnaburiaš;
she gave birth to Kidinu-[hud]uru[dish]. Kidinu-[hudurudish]
marr[ied] the daughter of ^{m}[...]anduniash;
she gave birth to ^{d}[GAL-h]und[ash]. I, the son [of ...],
14′ have married the eldest daughter of Meli-Šipak.

I make no apology for not reviewing in detail all the voluminous literature on the identifications of the Elamite kings concerned and refer the reader to the recent and exhaustive study of Roaf (2017). The Pihiranu-^{d}U mentioned is agreed to be the Pahir-ishshan of other (Elamite) sources. He married the daughter or sister of a Babylonian king Kurigalzu. Their daughter married Humban-numena, whose son Untash-Napirisha married a daughter of Burnaburiaš.

The order in which the Kassite names Kurigalzu and Burnaburiaš occur is clear. Which Kurigalzu is mentioned in the Letter is a moot point, while it is the very reference to a Burnaburiaš that seems to have created the major problem in interpreting the genealogies in the Letter. In the standard reconstruction of the Kassite dynasty there was no Burnaburiaš after Kurigalzu II – accordingly the text is often understood as referring to Kurigalzu I and Burnaburiaš II. But this has automatically led to backdating the beginning of the Elamite Igihalkid dynasty by about a century (compared to the once-standard model, e.g. Labat 1975). As Goldberg (2004, esp. 35) explained, this produces some difficult fallout: notably, the Kidin-Hutran, grandson of Burnaburiaš has to be distinguished from the Kidin-Hutran who invaded Babylonia in the wake of Tukulti-Ninurta I's conquest (late 13th century). A similar chronological tension is produced for the last part of the genealogy, which would place only three generations between Burnaburiaš and Meli-Šipak, a Kassite king conventionally dated to the early 12th century. In short, the backdating 'generates suspiciously long gaps in the Berlin letter's sequence and the sequence of Elamite kings actually attested on the ground in Elam' (Goldberg 2004, 40).

Goldberg suggested reasonably that these and other difficulties can be resolved if we understand the 'mighty king Kurigalzu' of the text (l. 59) as being the second, rather than the first, ruler of that name. This makes good historical sense. As noted above (**A New Historical Link?**), *Chronicle 22* describes how Kurigalzu (the 'young' of *Chronicle 21*) subdued the Sealand and defeated and captured king Hurbatila of Elam.

As for the Burnaburiaš in the Letter, as it is usually assumed that Burnaburiaš II preceded rather than followed Kurigalzu, Goldberg had to argue that he was a prince rather than a king. While the existence of an unattested prince of this name is of course possible, as Roaf (2017, 187, n. 79) noted the 'prince' explanation is unlikely from the context, '...because, as Paulus (2013, 432) pointed out, if the Burna-Buriaš in the Berlin Letter was not a king of Babylon, mentioning him would not have improved the claim to the Babylonian throne by the author of the letter.' It would be easier to assume that the composer of the Berlin Letter understood that there was a Kassite *king* Burnaburiaš two generations after a Kurigalzu. This matches well with the sequence known from the Amarna letters: Kurigalzu – Kadašman-Enlil – Burnaburiaš.

Roaf reviewed a number of possible reconstructions of the relationships between the Elamite and Kassite royal

[36] Van Dijk (1986); for an English translation see Potts (2016, 199).

houses, finally considering the following one based on that of Goldberg, together with notional dates (2017, 188, Fig. 6.04) – see *Figure 5* below. Regarding the name fragment (l. 15) ... $^{\text{I}}$x-x-$^{\text{d}}$/*an-dun-iá-àš,* the restoration 'king (EŠ[ŠANA]) of Karduniaš' has been suggested, though Roaf notes S. Paulus' preference for the likes of van Dijk's (1986, 162) reading $^{\text{I.kur}}$*kar*-*an-dun-iá-àš*, translated 'a Babylonian', and her suggestion that an 'Elamite king had entered into a royal marriage that was not recognised in Babylonia in the turbulent times following the reign of Burnaburiaš II'.

Note that on this generational model a Burnaburiaš would fall almost a century later than the conventional date for the Amarna king of that name.[37]

> historically accurate document, with regret because the Berlin Letter is such an engaging text and tells such a wonderful story and with relief because there is then no need to attempt the impossible task of forcing the contents of this uncooperative text into conformity with the other sources.

By 'other sources' here Roaf effectively means documents dated by the standard king list chronologies, based on links with Egypt. There was simply no way that Roaf, despite his best efforts, could fit the information from the Berlin Letter into the conventional scheme. But with a restructuring of Kassite chronology, these problems disappear and the information from the Berlin Letter can come into its own – with the understanding that Burnaburiaš mentioned was a king rather than a prince and the Amarna correspondent of that name – placed correctly in succession to a Kurigalzu with a 'mighty' reputation.

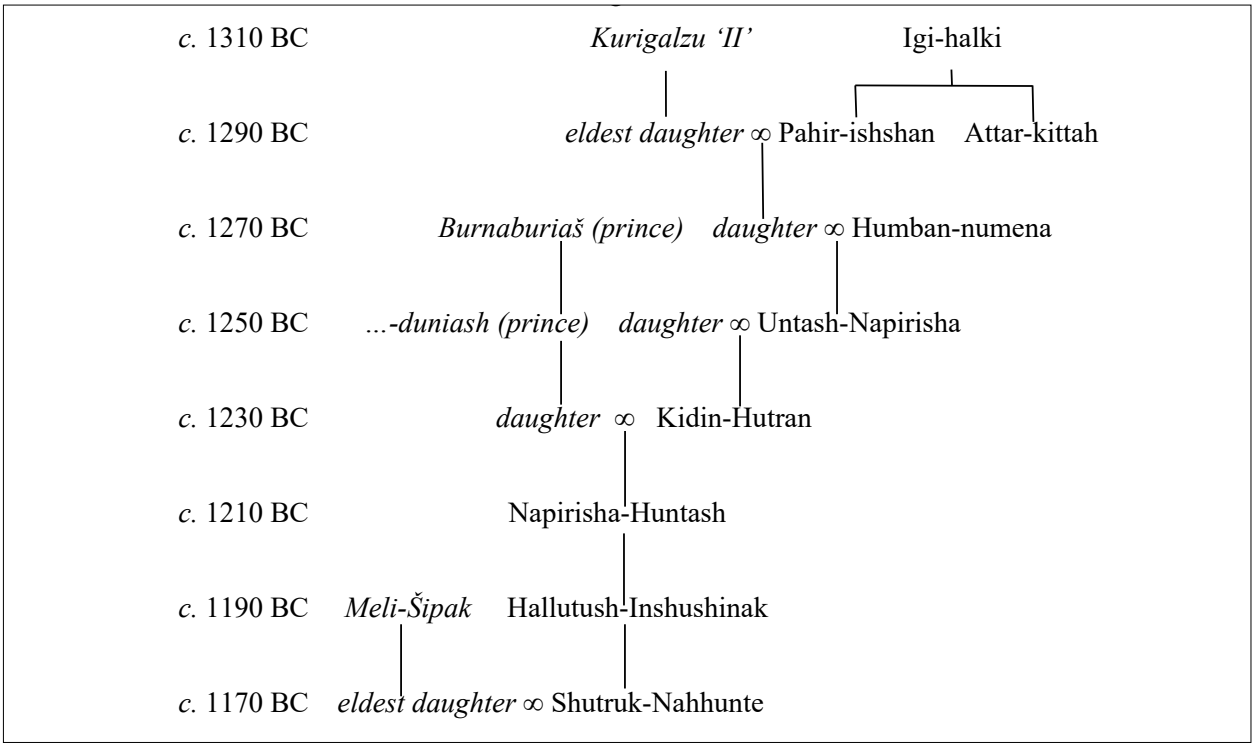


Figure 5: Goldberg's reconstruction of the Middle Kassite and Elamite dynastic relationships,[a] *with the notional dates as added by Roaf.*

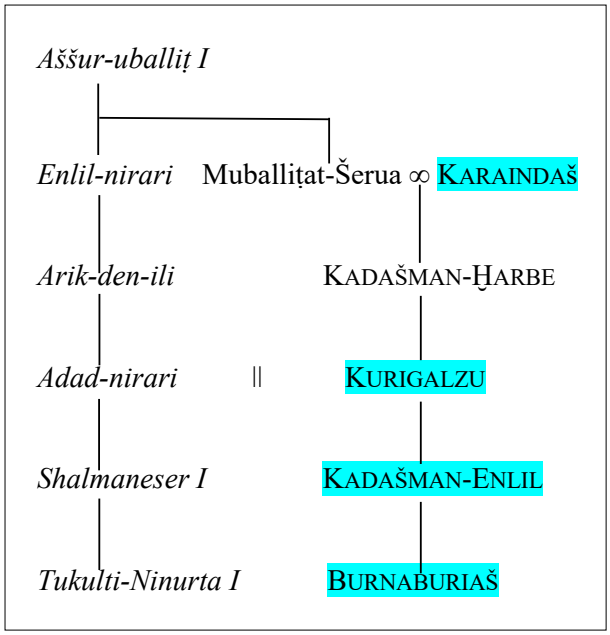
[a] With one correction: Goldberg (2004, Fig. 1) mistakenly gave Hutelutush-Inshushinak instead of Hallutush-Inshushinak – as noted by Roaf (2017, 187, n. 78).

After experimenting with this and other reconstructions, Roaf (2017, 195) decided to reject them all as unworkable, concluding (wistfully or gratefully?) that:

> It is with regret and relief that I come to the conclusion that the Berlin Letter is not a reliable,

Combining this with the information in *Chronicle 22*, we can reconstruct this simple stemma for the Middle Kassite kings, shown alongside their Assyrian contemporaries (left), as known from the AKL and the monuments, and Kassites known from the Amarna letters highlighted in blue:

[37] The overall dates are set to accommodate those generally accepted for Meli-Šipak (*c.* 1186–1172 BC) the *floruit* of whose 15-year reign would be better set at *c.* 1180 BC rather than Roaf's *c.* 1190 BC. On the revised model argued here this would need further lowering by some 110–120 years.



The upshot here would be a surprisingly different picture with regard to the placement of the Amarna period, which would no longer fall in the time of Aššur-uballiṭ I but during the reigns of Shalmaneser I and Tukulti-Ninurta I. Following the clues from *Chronicle 22* and the Berlin Letter the sequence of Amarna Kassites (highlighted in blue above) can thus be placed just as well in the 'late 14th–early 13th century BC' as they are in the early to mid '14th century BC' as usually reconstructed.

Support for this shift comes from *Chronicle 22*, which states that the battle of Sugaga (see above) was fought between Kurigalzu and Adad-nirari. This synchronism from *Chronicle 22* is often assumed to be incorrect. See e.g. Glassner (2004, 50):

> According to this account, Kurigalzu II, the predecessor of Nazi-Muruttaš, won the battle against his Assyrian adversary Adad-nārārī. The Babylonian chronicler is obviously open to doubt, since he made a mistake in transcribing one or other of the royal names. Since Kurigalzu II reigned before Adad-nārārī, and was a contemporary of Enlil-nārārī, it appears that he confused the theophoric elements in the Assyrian king's name.

There is also the possibility of two conflicts, one very early in Kurigalzu's reign, as allowed by Roaf (2017, 179):

> Because of the possible error of five years each way in the dates of the Kassite kings in relation to the Assyrian kings it is possible that the reign of Kurigalzu II overlapped with both those of Enlil-nārārī and Adad-nārārī, but it is possible that the better-known Adad-nārārī is a mistake for Enlil-nārārī.

That aside, the overall reconstruction set out above – *based on local records and not Egyptian input* – places Burnaburiaš, usually understood as a contemporary of Aššur-uballiṭ I, *about 120 years later* than usually thought – in fact as a contemporary of Tukulti-Ninurta I and/or his successors.

The A. 1998 Anomaly

Discussion of Kassite chronology was greatly complicated in 1982 when Donbaz published a clay tablet (A. 1998 = Bab 39031) in the Istanbul Museum dated year 12 Kadashman-Turgu. It refers to matters 'from the accession year of Kadashman-Enlil to the 12th year of Kadashman-Turgu, king'. These two kings were immediately assumed to be the '13th-century' kings of that name, yet the order given in A. 1998 flatly contradicts the order of the pair Kadašman-Turgu and Kadašman-Enlil as known from the letter of Hattusili (see above).

Brinkman (1983) analysed the problem in detail, weighing up two possible explanations:

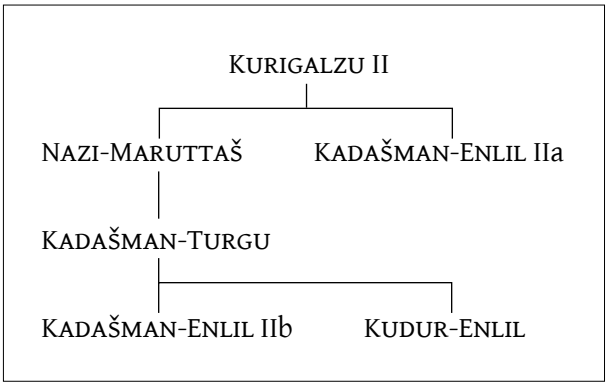
A. That there was another Kadašman-Enlil before Kadašman-Turgu.

B. That there was a second Kadašman-Turgu after Kadašman-Enlil II.

Working within a conventional model, solution A was accepted by Boese (2009) who argued from other evidence that there was a Kadašman-Enlil between Nazi-Maruttaš and Kadašman-Turgu. This 'new' Kadašman-Enlil (IIa) would have been a younger brother of Nazi-Maruttaš. The evidence comes from a cylinder seal used to seal eight tablets or envelopes from the year 4 of Nazi-Maruttaš. The seal belonged to a priest of Enlil, called [Be?]lanum son of a Kadašman-Enlil and grandson of King Kurigalzu.[38] Some have tried to relate this to Kurigalzu I but, as Boese notes, this would add seventy years or more to the career of [Be?]lanum, who must have reached a certain age already when he became a priest in the time of his grandfather: '... on the contrary, everything suggests that the aforementioned seal inscription concerns King Kurigalzu II. During his lifetime the seal would have been cut and used a few years later under his son and successor Nazi-Maruttaš.'[39] We have therefore evidence, at least, for a *prince* Kadašman-Enlil son of Kurigalzu II, whom Boese identified with the future king Kadašman-Enlil II. From the seal, A. 1998, and other data Boese offered the following *stemma*:

[38] Stiehler-Alegria Delgado (1996, 202, no. 193).

[39] Boese (2009, 87, trans. Joe Baker).



Discussion was revived more recently by Nahm (2016) in a short and ill-informed note in which he simply dismissed A.1998 as an ancient forgery, even descending to invoke a paranormal analogy:

> Occasionally young scientists are told the following story. An explorer comes home with a photo of a green tiger. One just about may discuss the existence of such an animal. If he shows a picture of a green deer in addition, one rather will discuss his equipment or his personality.

In this rather offensive tirade he did not name the authors (including Boese, Furlong and the present writer) whose personality he imputes.[40] Nahm also used mistaken assumptions to support his position – one of which (regarding the alleged year '10' for Kadašman-Ḫarbe II) was corrected by Brinkman (2016, emphasis added) in a more balanced response in the 'green tiger' discussion:

> Werner Nahm in *N.A.B.U.* 2016/1 (12) grouped together two tablets from the Babylon M8 archive which he labelled 'probable fakes' because of their questionable dates ('two texts allegedly dated by ancient kings'). These tablets are: (a) A. 1998 (=Bab 39031) from the 12th year of Kadašman-Turgu, (presumably immediate) successor of an otherwise unknown Kadašman-Enlil; and (b) Bab 39045 from the 10th year of Kadašman-Ḫarbe II. The existence of two such tablets with anomalous dates in the same archive was viewed as corroborating the interpretation of these texts as ancient forgeries.
>
> The data merit closer examination. Bab 39045, known at present only from the photos PhBab 1769–1770 2,6, was catalogued in Pedersén 2005 (p. 98, M8:18) as dating to Kadašman-Ḫarbe II, year '10?' – *the doubt as to the reading of the regnal-year number not mentioned by Nahm*. The relevant photo, PhBab 1769, shows after the MU in this line an almost vertical wedge in slight shadow which favors a reading MU.1.KAM—the slant of the '1' here is similar to that of the *-dáš-* in the following royal name and differs markedly in shape and angle of impression from the '10' sign in the day date in the preceding line. Many late Middle Babylonian tablets are written in a script heavily slanted downward toward the right; this is not surprising to those familiar with their paleography. A date in year 1 of Kadašman-Ḫarbe II is also attested in YBC 7652 – thus obviating the proposed reason for doubting the authenticity of this tablet.

Nevertheless, Brinkman felt that:

> A. 1998 (Bab 39031) presents a different set of problems, and I share Pedersén's and Nahm's qualms about the authenticity of this text. There is no other supporting evidence for a Kadašman-Enlil as immediate predecessor of Kadašman-Turgu. The title to real estate pledged as a security for debt in this document could furnish sufficient motive for forgery. And citation of a copper standard of value measurement here (lines 15, 19, 21, and 36) is anachronistic. This standard is attested in dated or datable Babylonian texts only in the twelfth century between 1175 (Meli-Šipak, year 12) and c. 1130 (Itti-Marduk-balāṭu, year not preserved), after political disturbances at the eastern end of the Mediterranean had shut off the flow of gold from Egypt. By contrast, in the early thirteenth century, a gold standard—or less commonly a silver standard—would have been cited. (Müller 1982; Brinkman 1987).

In a response (James 2020) I countered all the points raised by Nahm and Brinkman. To summarise regarding the more substantive arguments in the exchanges:

(1) Nahm: that the relationship of A. 1998 to Bab 39045 (with a date of the tenth year of Kadašman-Ḫarbe II) makes Pedersén's suspicion 'that the text may be an ancient fake... the most likely possibility'. The idea that Bab 39045 is 'anomalous' relies on the assumption that the Kadašman-Ḫarbe mentioned was the second king of this name who is only attested as having a reign of six months (according to Babylonian King List A). See however, Brinkman's response above showing that the figure on Bab 39045 is more likely a '1', 'obviating the reason for doubting the authenticity of this tablet.'

(2) Brinkman: 'There is no other supporting evidence for a Kadašman-Enlil as immediate predecessor of Kadašman-Turgu'. But this is merely invoking the

[40] Nahm basically works in science (in nuclear physics), something one feels should have made him more on the ball when he suggested that 'efforts to identify scribes on the basis of 3D recordings of cuneiform texts can use the two probable fakes and the tablets written by Itti-Ezida-lummir as a test case.' He had not first checked whether the tablets in question were even available for analysis. Of the 100 tablets in the M8 archive, only 55 have been located in the Berlin and Istanbul museums; the other 45 are currently missing and – with one exception – are known only from excavation photos and/or transcriptions.

accepted dating in order to reject a possible anomaly. There are 'factoids' in the Kassite period that are based on far more slender evidence, and Boese indeed produced evidence for the existence of a Kadašman-Enlil before Kadašman-Turgu (see above).

(3) Brinkman: 'The title to real estate pledged as a security for debt in this document could furnish sufficient motive for forgery.' I fail to understand this point: if one presented fake land deeds to a magistrate with the name of an imaginary monarch written on them, no-one would be convinced. Are we to assume that people in the Kassite period had no memories of recent kings or public records by which to authenticate claims?

(4) Brinkman: '... citation of a copper standard of value measurement here (ll. 15, 19, 21, and 36) is anachronistic.' Maybe such a thing could be said *if* the internal chronology of the Kassite period was a settled matter. Note also that Brinkman is talking about *a* copper standard not that copper was *the* standard referred to in the tablet. Four lines of the text refer to copper as a standard, while *another five lines* (13, 20–21, 34–35) actually refer to a gold standard in their totals, and another (36) both gold and copper, showing that there was a mixed system at the time.

In short there are no good reasons to reject the probity of the document and it is hard to resist the conclusion that it is really the 'anomalous date' on A. 1998 which is the real reason for its rejection as a genuine document.

Wider Fallout

On the principle of taking an ancient text as innocent until proven guilty, one can continue to use A. 1998 in a chronology-building experiment, to see how the information it provides can mesh with that of *Chronicle 22*. As seen above, Boese argued that Nazi-Maruttaš had a younger brother Kadašman-Enlil. (His paper is overlooked by both Nahm and Brinkman.) The family relationships are set here against Assyria (using the synchronism between Adad-nirari I and Kurigalzu 'the young' given in *Chronicle 22*):

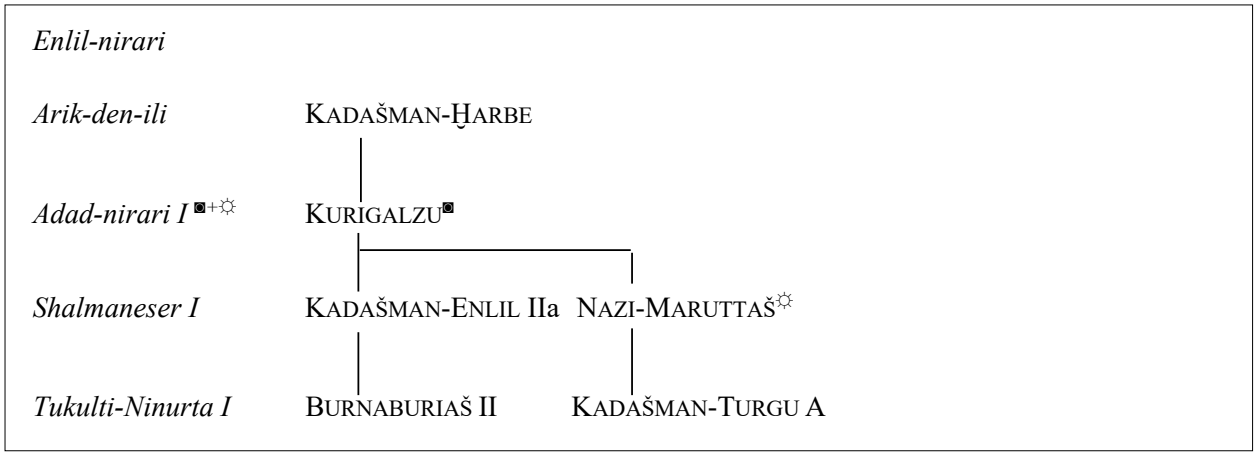
We see here two lines of descendants from Kurigalzu. The senior line would have begun with Nazi-Maruttaš and his son Kadašman-Turgu A.[41] After their deaths Kadašman-Enlil IIa would have taken the throne, to be followed by Burnaburiaš – as per the Amarna letters.

There is nothing from Assyrian records to contradict these placements. For Adad-nirari I, *Chronicle 22* states that he defeated Nazi-Maruttaš in battle.[42] But claims that Adad-nirari was also a contemporary of Kadašman-Turgu are unconvincing. These are based on the highly damaged VAT 15420 (Frahm 2009, 128), which has been held to provide a synchronism as Kadašman-Turgu is mentioned four times (twice as king of Karduniash) and '[Adad-nā]rārī' in l. 11. Near the beginning of the extant text, after the name Kadašman-Turgu, reference is made to the cleansing of the sins of someone's son (l. 5), 'sein(en) Sohn, reinigte er von der Sünde' (Frahm). What this may have to do with the restored personage '[Adad-nā]rārī' is unclear but the composition appears to be referring to some issue that occurred in the past.[43] As to its date, it is interesting that the last preserved line mentions one Babu-aha-iddin[a] the name of a well-known vizier from the time of Shalmaneser I (Donbaz 1997) and Tukulti-Ninurta I (Mora and Giorgieri 2004, 168–170: KUB 23.103 = KUB 23.92). If this reflects the actual date of the composition, perhaps the references to Kadašman-Turgu may well apply to a later reign than

[41] As argued in Furlong (2010). It always remains possible however, that the two reigned concurrently in a power-sharing agreement.

[42] There is one fragmentary text from his annals which describes his war against Karduniash (Grayson 1987, 157 [A.0.76.21]) in which the name [Nazi-Maruttaš] has to be fully restored.

[43] Devecchi (2017, 113). VAT 14440 is a tiny fragment of a clay tablet in which the name of Adad-nirari occurs together with the mention of a king of Karduniash, whose name is regrettably lost. Frahm (2009, 127 [65]) hypothesized that this fragment might belong to the same composition, or even to the same tablet, as VAT 15420, where Adad-nirari and Kadašman-Turgu are repeatedly referred to. The poor state of preservation of the text(s) does not allow any reconstruction of its historical background.



Adad-nirari. [Ed. The author had planned to develop his argument further here, but was unable to, due to his untimely passing.]

Hence the statement of the *CAH* stands: 'There is no record that he [Adad-nirari] was in hostilities with Kadašman-Turgu (1297–1280) or Kadašman-Enlil (1279–1265).'[44] For the thirty-year reign of Shalmaneser I (*c.* 1274–1245 BC), Assyrian records are completely silent with respect to Babylonia:

> There is no reference in his extant inscriptions to his relations with his Babylonian contemporaries, Kadashman-Enlil II (1279–1265), Kudur-Enlil (1263–1255) and Shagarakti-Shuriash (1255–1243), and such information as there is from other sources throws only a partial light on their nature.[45]

Names of Babylonian opponents are also missing from both the early and later reign of his successor Tukulti-Ninurta I (*c.* 1244–1208 BC); his final conquest of Babylon and capture of Kaštiliašu may have taken place as late as his year 19 (for discussion see Yamada 2011, 216; cf. Bloch 2010a, 2010b, 2023).[46] There would hence be no obstacle to fitting the Amarna Kadashman-Enlil (IIa) within the mid-'thirteenth-century' slot. After Tukulti-Ninurta's conquest of Babylon there was a revolt against him both there and in Assyria. Assyria went into a recession, with at least three claimants to the throne: Aššur-nadin-apli, Aššurnaṣirpal and Enlil-kudurri-uṣur (see Wallenfels, 'Middle Assyrian History and Culture …' in these Proceedings, p. 65). The fragmentary and conflicting nature of the sources makes this a particularly confusing period to understand. Enough to say that there is no obstacle to Burnaburiaš being a contemporary of the late reign of Tukulti-Ninurta and the '*epigoni*' that followed him.

As for the kings Nazi-Marrutaš and Kadašman-Turgu A, interposed here between Burnaburiaš and his father Kurigalzu, their apparent absence from the Amarna Letters is reasonably explainable by gaps we have in the surviving sample of texts – though one or other might well be the King *X* mentioned above.

Aššur-uballiṭ: New Possibilities

This brings us to the so-called 'Aššur-uballiṭ problem'. If the proposed reconstruction is correct, then it should be clear from the above tables that the Aššur-uballiṭ I of the Assyrian King List reigned several generations before the Amarna Kassite Burnaburiaš. Another candidate must then be sought for the Amarna correspondent of this name. I pondered on the curious phrase used by Aššur-uballiṭ in Amarna Letter EA 16 about his equivalence to the king of Hanigalbat, usually assumed to be the failing state of Mitanni in north-eastern Syria. The passage is rather cryptic: 'When the king of Ḫanigalbat wrote to your father in Egypt, he sent 20 talents of gold to him. Now I am the [equ]al of the king of Ḫanigalbat, but you sent [me … of go]ld, and it is not enough for the pay of my messengers on the journey to and back.'[47] But I wondered whether this Aššur-uballiṭ was actually based in Hanigalbat. In the 'mid 13th century' the kings of Assyria appointed a line of viziers (sing. *sukkallu rabiu*) or local kings (sing. *šar Ḫanigalbat*) over their north-western province of Ḫanigalbat, seized from the once great power of Mitanni.[48] In the words of Machinist (1982, 16): '… this post of "king" must have been created after – and as a deliberate variation on – the unsuccessful attempt by Adad-nīrārī I to govern Ḫanigalbat through a native vassal king.' Aššur-uballiṭ's diplomatic grumble about the lack of gold from Egypt might reflect the transition from the rule of north-western Mesopotamia by an independent 'Great King' of Mitanni (a.k.a. Ḫanigalbat) to one by a local Assyrian king. Indeed, the *CAH* describes Aššur-uballiṭ's 'reference to a "Khanigalbatian king" as, in a sense, his own predecessor' (Gadd 1975a, 27).

All the same, this 'solution' to the Aššur-uballiṭ question depends on the difficult understanding of the largely restored word 'I am the [equivalent]' ([*meḫrē*]⌜*ku*⌝), which may be an intractable problem due to the condition of the tablet (see the paper by Johannes Dams in these Proceedings, p. 108). And unfortunately tablet EA 16, housed in Cairo, has not been available for clay analysis, which might tell us whether it comes from Nineveh, the Hanigalbatian region, or was even a locally made Egyptian copy. Such is our ignorance in the matter. Attempts were made by the Israeli project doing petrographic analysis of the tablets but

[44] Gadd (1975b, 275).

[45] Gadd (1975b, 282; cf. Devecchi 2017, 113): 'Neither the chronicles nor the royal inscriptions refer to any common event in the history of the two kingdoms during the reign of Shalmaneser I.'

[46] Cf. Miller (2017, 101): 'The conquest and capture of Kaštiliašu seem to have been effected in at least two stages during the latter half of the second decade of Tukultī-Ninurta's reign, though the exact dates are still disputed.'

[47] As restored and translated by Moran (1992, 39); for other translations see Artzi (1997); Rainey (2015, 132–133).

[48] Machinist (1982, 14, 15–16): 'In the thirteenth century, however, we see a concerted effort at permanent control of provincial territory on a wide scale …. Only states like Hanigalbat, which were perceived as close enough and vital enough, economically and militarily, to the Assyrian heartland, underwent the whole evolution from raiding through vassalage to province … in the course of the evolution already noted a special officer appeared, charged with the overall supervision of the area. He was … the *sukallu rabû*, perhaps the highest Assyrian officer after the king; and the fact that as supervisor of *Ḫanigalbat* he carried the title *šar Ḫanigalbat* suggests that at least one intent of the whole effort was to preserve for the area a vassal-like status in order to counter the strong nativist sentiment within it.'

received no reply (pers. comm. Israel Finkelstein). One can only describe the attitude of the Cairo Museum as lamentable.

While I would not rule out entirely the Ḫanigalbat possibility, Ronald Wallenfels has drawn attention to the language of Aššur-uballiṭ's two letters, which are in distinctly different dialects of Akkadian: EA 15 was written in Assyrian and EA 16 in Hurro-Akkadian (Moran 1992, xix, n. 35). Moreover there are distinct differences in the content of the two letters. Whereas EA 15 claims that this is the first time that Assyria and Egypt had communicated, EA 16 states that Aššur-uballiṭ's 'father' (*a-bi*) Aššur-nadin-ahhe had received gifts from Egypt upon sending an embassy there. These he has suggested may have been amongst the various upstarts who claimed the Assyrian throne in the chaos that ensued after the murder of Tukulti-Ninurta I, perhaps aliases of the two or three claimants that we already know of (Aššur-nadin-apli and Aššurnaṣirpal), assuming the mantle of Aššur-uballiṭ I, for its grand associations as he was effective founder of the Middle Assyrian kingdom (see Wallenfels, 'Middle Assyrian History and Culture …' in these Proceedings, p. 70). This would match perfectly with the alignment suggested above, that the later Amarna period belongs to the end of the reign of Tukulti-Ninurta I and his successors. It would also explain the otherwise seemingly overblown claim of Burnaburiaš when he complained to the Pharaoh (Tutankhamun) that his 'Assyrian subjects' had no right to be communicating with Egypt (EA 9, 31–35, trans. Moran 1992, 18), to which Wallenfels has drawn attention:

> Now, as for the Assyrians, my vassals (*dāgil pānīya)*, it was not I (who) sent (them) to you. Why on their own initiative have they come to your country? If you love me, they will conduct no business whatsoever. Send them off to me empty-handed.

To add to this are three rather overlooked references to the Kassite king in the letters of Rib-Hadda, the vassal ruler of Byblos. The first (EA 76:14–15) accuses Abdi-Aširta of Amurru of empire-building by seizing nearby cities, characterising his hubris in these words: 'Is he the king of Mitanni or the king of Kaššu, that [h]e strives to take the land of the king for himself?' (trans. Moran 1992, 146). EA 104:20–21 repeats this with respect to the sons of Abdi-Aširta, while reversing the order of the two major powers invoked (Kaššu and Mitanni). EA 116:70–71 adds to the refrain the king of Hatti. In the standard dating one might expect these letters to have made a comparison with Assyria, since Aššur-uballiṭ I is thought to have subdued Mitanni and, as we know from the Chronicles discussed above, dominated Kassite Babylonia for a while, altering the succession there. Two of the letters date to after the death of Abdi-Aširta, while the mention of Hatti can only refer to Suppiluliuma I, a relative latecomer to the Amarna scene (see below) who was threatening the territories jointly claimed by Egypt and Mitanni and was entering the 'Great Kings' network. These clues put the letters in question near the end of the Amarna period, matching the date of EA 9 from Burnaburiaš. If we date these letters in the period after the revolt against Tukulti-Ninurta I, then the comparisons with the king of the Kassites makes sense and support the idea that Burnaburiaš's claim over Assyria was no empty boast. In the standard model all these references seem inexplicable. In a discussion of the status of Babylonia internationally, Brinkman (2017, 24 and n. 210) pondered 'whether Babylonia's ranking as a major power especially in the thirteenth century was based more on tradition than military achievement or even military potential', adding: '[a]n impression perhaps reinforced by the impotence of Burna-Buriaš reflected in EA 9:31–33, where he complains that the Assyrian king, his inferior in rank (*dāgil pāniya* 'my subject'), was communicating directly with the Egyptian court.' This perceived 'impotence' was certainly not in the mind of Rib-Hadda when on three occasions he listed the Kassite king as being on a par with those of Mitanni and Hatti and with a conspicuous absence of reference to Assyria.

Hittite Synchronisms

Suppiluliuma I is the only ruler of Hatti known from the Amarna correspondence: EA 41 (and probably also EA 42–43 – see Devecchi 2012) was addressed by him to Ḫuriy[a], most likely Tutankhamun.[49] Consequently, on the revised model argued here his contemporaries in Assyria would have been Tukulti-Ninurta I and his immediate successors rather than Aššur-uballiṭ I.

Other synchronisms between Egypt, Assyria and Hatti can be drawn from the information in the tablets found at Hattusa (Boğazköy). Unfortunately, when the royal archive was discovered it was in a shambolic state. Only a few of the tablets were found together in a relatively secure context. Some were found on the Acropolis or nearby, but many others were found in fill used for levelling floors in later Phrygian buildings. Many were smashed in antiquity either for this purpose or deliberately by Hittite scribes discarding old tablets (Hoffner 2009, 41–42). Thus ordering the tablets in any sequence from their context is impossible: this can

[49] We also know that from the *Deeds of Suppiluliuma I* (Güterbock 1956, 94 [A iii 7–15]) that Suppiluliuma was asked for one of his sons to marry an Egyptian queen, the widow of Pharaoh Nibkhururiya, as her husband had died with no heir. Nibkhururiya is either Neferkheperure Akhenaten (e.g. Krauss 1978, 36–40; Gabolde 1998, 194–212; Parker 2002; Miller 2017, 99–100) or Nebkheperure Tutankhamun (e.g. Bryce 1990; Murnane 1990, 23 n. 114, 133 and n. 120; Dodson 2009, 89).

only be done using philological means but mainly by utilising the names of the kings mentioned who can be placed in time from a secure reconstruction of the royal line for the Hittite New Kingdom (Beckman 2000, 25–26).

Two tablets with correspondence between Assyria and Hatti, are of particular importance here: KUB 26.70 (Bo 4979) and KUB 3.74 (Bo 8383). The first is to an unnamed Hittite correspondent from a Tukulti-Ninurta (not named as a king) who states that his father had previously had some dealings with Urhi-Teshub, the predecessor of Hattusili III. The Hittite correspondent must have been one of the last rulers of the Hittite Empire, Tudhaliya IV being a likely candidate. The second letter provides the best synchronism between Hatti and Assyria, sent by a king Tudhaliya to a king Tukulti-Ninurta. This Tukulti-Ninurta has to have been the first king of that name, as Tukulti-Ninurta II (890–884 BC) existed beyond the general time frame of the letters.

Were we to combine the evidence of the two letters (as I mistakenly did in James *et al*. 1991a, 306, 341–342), then the Hittite writer of KUB 3.74 must have been Tudhaliya IV – but that would seem to raise a problem for a revised chronology, as it would appear to confirm the dating of Tudhaliya IV to the time of Tukulti-Ninurta I (late 13th century BC, conventional dating). On closer examination there is every reason to think that the Tukulti-Ninurta addressed in KUB 26.70 was not a king but a prince, as he is not addressed with a royal title – though admittedly the tablet is fragmentary. According to Mora and Giorgieri (2004, 200) it is not even clear that Tukulti-Ninurta was the addressee of the letter. Given this, the evidence of this letter should be discounted for chronological purposes.

KUB 3.74 provides much firmer evidence: the Assyrian monarch Tukulti-Ninurta who was addressed by a Tudhaliya has to have been the first king of that name. Another letter from a Tudhaliya to a king Shalmaneser announced that he had recently taken the throne, making him a younger contemporary, perhaps placing him towards the end of Shalmaneser's reign.[50] (Again, this could only be the first ruler of that name and not Shalmaneser II, *c.* 1030–1019 BC). The realignment of Egyptian chronology relative to the Assyrian argued here would mean that the Hittite correspondent would not be Tudhaliya IV but rather Tudhaliya III, the father of Suppiluliuma I. Tudhaliya III began the recovery of the Hittite Empire after its disastrous but short-lived collapse due to the 'concentric attack' from surrounding enemies. The following realignment can be offered with triple synchronisms including Assyria and Egypt:

Egypt		Assyria		Hatti
Amenhotep III	||	Shalmaneser I	||	Tudhaliya III
Akhenaten	||	Tukulti-Ninurta I	||	Tudhaliya III/ Suppiluliuma I
Tutankhamun	||	multiple kings	||	Suppiluliuma I

A problem that this realignment may face is that the handwriting of the letters usually ascribed to Tudhaliya IV (in the above model, Tudhaliya 'III') is usually thought to be largely in keeping with that (*ductus* IIIc) from the late New Kingdom (see Mora and Giorgieri 2004, 140). Further analysis, beyond my area of expertise, is required here into whether such stylistic dating is completely certain for the letters in question. But one only needs to recall how Hittitologists have often had to rethink such matters. The redating of a number of texts such as the *Indictment of Madduwatta* from the late 13th century (Hittite New Kingdom) to the late 15th/early 14th century BC (Hittite Middle Kingdom) has now long been generally accepted.[51] There seems to have been some overkill here, as Stephano de Martino (2005) has reconsidered the backdating of some tablets to the Hittite Middle Kingdom. Notably he identifies from prosopographic and geographical details that some texts should be reassigned from the time of Tudhaliya I/II to the time of Tudhaliya III, who reigned at the cusp of the Middle and New Hittite kingdoms.

Another issue is that many letters and other documents (as they have survived) were later copies. Some years ago, Ünal (1991, 18, 28) argued that a fragmentary royal edict (KUB 23.13), 'one of the most exhaustively studied historical fragments in the field of Hittitology', and hitherto tentatively assigned to the reign of Tudhaliya IV,[52] belonged to the time of Tudhaliya III. Ünal's hunch seemed to be confirmed by the evidence from

[50] KUB 23.99 (Mora and Giorgieri 2004, 175–178). Comparison has been made with the contents of a letter from Ugarit (RS 34.65), which relates the defeat of King Tudhaliya by an Assyrian whose name has been heavily restored as Shalmaneser (Singer 1985, 100–101) – see however Wallenfels ('Middle Assyrian History …' in these Proceedings, p. 71, who suggests that the name could plausibly be read as (the patronymic) [Aššur]-˹rēš˺-[iši] (I). The ruler of Ugarit mentioned is [Ibira]na which would mean that the Hittite ruler concerned would be Tudhaliya IV, providing a new synchronism for a *CoD*-style revised chronology between that king and Aššur-reš-iši's son Tiglath-pileser I.

[51] For discussion and bibliography on the redating, see Houwink ten Cate (1970); Bryce (2005, 129–130); Beckman (2000: 20–21).

[52] For the standard dating to the reign of Tudhaliya IV, see Güterbock (1992).

the archive at Ortaköy, where the family of Tudhaliya III took temporary residence during the 'concentric attack' from surrounding enemies. Soysal (2012, 174) adds a discussion of the prosopography, leaning towards the idea that the text is indeed of (Late-)Middle Hittite dating, though 'a heavily "modernized" late copy'. Were other documents such as royal correspondence (if still needed for reference by the court) similarly updated in terms of their *ductus* and grammar? In this climate of continual reassessment, it does not seem beyond the pale to wonder whether KUB 3.74 could belong to the time of Tudhaliya III rather than IV – hence resolving an apparent problem with the synchronisations offered by the *CoD* chronology. As it happens, Mora and Giorgieri (2004, 26) note that KUB 3.74 may be a copy, though they do not specify when that might have been made: but it may explain the 'late' style of the letter.

Conclusion

It is argued here that there is no independent support for the conventional Egyptian chronology from Mesopotamia. A reconstruction of Kassite chronology for this period, based on native sources with no input from Egyptian dating, can produce a consistent picture though one very different from the standard pattern of synchronisms assumed for this period. It also means that there will no longer be good reason to reject the probity of ancillary documents such as the contemporary text A. 1998 and later texts such as *Chronicle 21* and the Berlin Letter. The reasons for rejecting their information can now be seen as doctrinaire. In the model offered here the Assyrian *Chronicle 21* would be challenged in that it seems to have mistakenly referred to Burnaburiaš as the father of Kurigalzu II rather than his ancestor; not forgetting, of course, that *Chronicle 21* is contradicted on this very point by the native Babylonian *Chronicle 22*.

A downwards shift of Babylonian chronology by a century or more – relative to both the Egyptian and Assyrian – *provides just as many synchronisms as the standard model, and arguably three more*: if we include the new identifications of Itti-Marduk-balaṭu the vizier with the like-named king of the Second Dynasty of Isin; Kurigalzu I with the earlier king of that name mentioned in the Amarna correspondence of Burnaburiaš; and the Burnaburiaš of the Berlin Letter as the Amarna correspondent.

Moving to the Hittite lands and the Levant, we also need to consider the other new synchronisms offered by the *CoD* revision of chronology – such as the identification of Ini-Teshub I of Carchemish with Ini-Teshub II (see James *et al.* 1991a, 134–135, Table 6:3, 303, 362–363), and now the Tarhuntassan kings Hartapu I and II (see James 2021–2022 and Kelder, 'Dating Hartapu ...' in these Proceedings, pp. 133–139), kings Taita I and Taita II of the Hamath region (see Wallenfels, 'Middle Assyrian History and Culture ...' in these Proceedings, p. 65), and the Šipiṭ-Baᶜal I of the Byblite inscriptions with the like-named king of Byblos (Šipiṭ-Baᶜal II) named by Tiglath-pileser III in 738 BC (see Wallenfels 1983; James *et al.* 1991a, 249–251). In both Mesopotamia and the West it would seem that numerous kings have been reduplicated by the procrustean bed of the conventional chronology.[53]

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[53] See further Wallenfels and James, 'Discussion 1: The North Syrian Ḫurrian Kingdom and Dark Age Chronology', in these Proceedings, pp. 29–36).

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Middle Assyrian History and Culture and Dark Age Assyrian Chronology[1]

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In 2019 I published in the Journal of the American Oriental Society *a review article entitled, 'Solomon and Shishak: A Disambiguation' (Wallenfels 2019b), focusing on P. James and P. van der Veen's 2015 edited collection of essays,* Solomon and Shishak: Current Perspectives from Archaeology, Epigraphy, History and Chronology*, a record of the proceedings of the Third BICANE Colloquium held in 2011. The presenters argued variously both for and against the central postulates underlying the revised chronology put forward 20 years earlier in P. James* et al.*'s 1991 volume,* Centuries of Darkness*. At the conclusion of that review article I noted that if this underlying* CoD *ultra-short chronological model for Egypt and the Levant is correct – and I believe in principle that is – then, in light of numerous well-known synchronisms of varying certainty between New Kingdom Egyptian kings and kings of Kassite Babylonia and of the neighbouring Middle Assyrian kingdom(s), the chronologies for the corresponding periods in Mesopotamia would need to be adjusted significantly downward, too. In the following, several case studies will be presented testing the validity of the proposed revisions.*

Introduction

Four clear cornerstones have emerged for this writer that appear to be foundational to the *Centuries of Darkness* (James *et al.* 1991), or *CoD*, revised chronology: (1) the 10th century biblical Egyptian king Shishak is to be identified with the historical Egyptian king Ramesses III, not Shoshenq I[2] who is himself to be relocated to the later 9th century in a truncated and telescoped Third Intermediate Period (TIP);[3] (2) a much-abbreviated Iron Age I period represents the break-up of the United Monarchy, not the initial Israelite settlement;[4] (3) the invasion of Judah by Zerah the Kushite in the reign of King Asa, *c.* 900 BCE, reflects the Levantine military expedition of Userhau, Overseer of (Northern) Foreign Lands in year 5 Ramesses IV (van der Veen and James 2015); and (4) the Iron Age IIA period is 'Omride', not 'Solomonic'.[5] In this light, assuming yr. 32 Ramesses III = yr. 1 Ramesses IV, and, for the moment, taking the chronological notice in 2 Chron. 13:23b literally,[6] I propose, as a starting point, lowering the dates of Ramesses IV, and by extension the 20th Dynasty as a whole, by exactly 248 years (see *Figure 1*). As a direct consequence of this arrangement, Ramesses III would have besieged Jerusalem in his 11th regnal year (cf. James 2017).

[1] The following remarks were first presented at the *Seminar für Biblische Archäologie bei Schwäbisch Gmünd* in September 2019 at the kind invitation of that meeting's organizer, Peter van der Veen. All conventional dates not directly linked to the Julian calendar are marked herein with an asterisk (*c.* *1200 BCE); all *CoD* chronology-based dates are marked with an exclamation mark (*c.* 950! BCE). For the convenience of the reader Assyrian king's names herein are numbered per their position in the Assyrian King List (AKL), e.g. (78) Tukulti-Ninurta I. Cuneiform texts are cited herein in conformity with the *Chicago Assyrian Dictionary* (*CAD*) 20, *U and W* (2010).

[2] First proposed by J.-F. Champollion in 1828 (Champollion 1868, 81).

[3] Kitchen (1996, 1997), notwithstanding; for a general statement of the issues, see James *et al.* (1991, 231–259); see further, e.g, Thijs (2005); Furlong (2010, 143–193); James and van der Veen (2015, *passim*).

[4] Bimson (2015); for the entry of the Israelites into Canaan at the end of MB IIB–C, see Bimson (1978); see further James *et al.* (2022).

[5] As first argued by Kenyon in 1960 (1979, 258–285) and more recently by Finkelstein (1995, 1996); see further Chapman (2015; cf. Wallenfels 2019b, 492).

[6] '. . . his (viz. Abijam's) son Asa succeeded him. The land was untroubled for ten years.' Galil (1996, 18) would place the war of Zerah closer to yr. 15 Asa when the Judaean king conducted a triumphal assembly at Jerusalem, seemingly following a substantial military victory. Should such a lower date prove to be more likely the case, then the dating of yr. 5 Ramesses IV would have to be lowered appropriately.

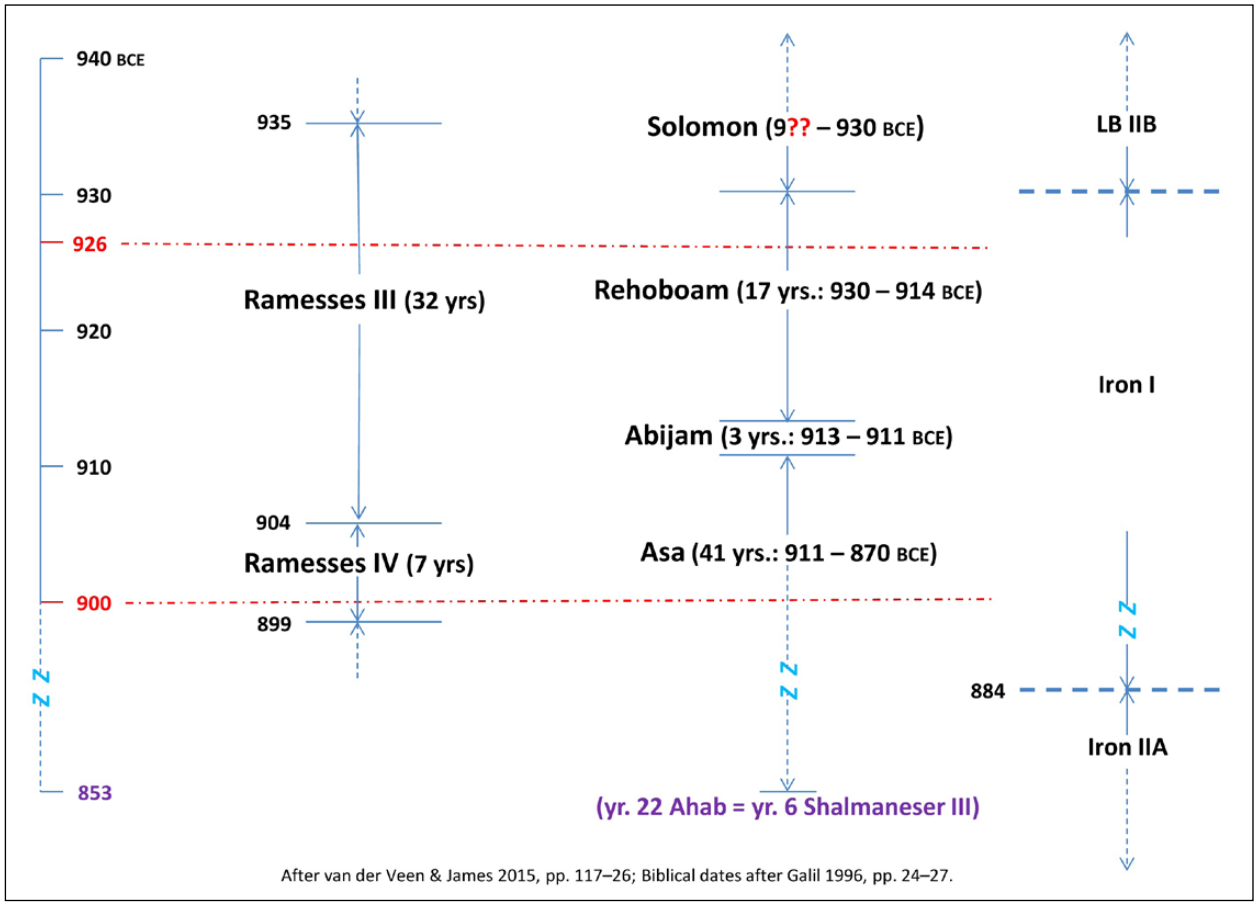

Figure 1. A CoD-based chronology of Egypt and Judah: the invasion of 'Zerah the Kushite', yr. 11 Asa = c. 900 BCE = Levantine expedition of Userhau, Overseer of the (Northern) Foreign Lands, yr. 5 Ramesses IV: Δ = -248 yrs (after van der Veen and James 2015, 117–126; biblical dates after Galil 1996, 24–27).

At the conclusion of my review of 'Solomon and Shishak' (Wallenfels 2019b), I noted that *if* this underlying *CoD* ultra-short chronological model for Egypt and the Levant is basically correct, as a direct consequence the chronologies for the corresponding periods in Mesopotamia would need to be adjusted significantly downward, too. There are, after all, numerous well-known synchronisms of varying certainty between New Kingdom Egyptian kings and kings of Kassite Babylonia and of the neighbouring Middle Assyrian kingdom(s). Putting aside the numerous difficulties associated with Kassite chronology for now,[7] I would like to focus here on the chronology of the Middle Assyrian kings, for they alone have the potential to be firmly linked to the Assyro-Babylonian chronological backbone (determined by Ptolemy's Royal Canon) and thereby extend it back before its current 910 BCE cap (imposed

[7] The conventional order of the Kassite kings is largely determined by Babylonian King List A (BKL A) which is broken in the middle of its enumeration of the dynasty. Known only from a single Late Babylonian copy (BM 33332), it summarizes that 36 kings ruled 576 years, however only the names of kings nos. 1–6 and 24–36 are at least partially if not fully preserved; nos. 7–10 are restored from the late Neo-Assyrian copy of the Synchronistic King List from Ashur (14616c); nos. 20–21 are restored from the Neo-Assyrian-period copies of the Synchronistic History (Chron. 21) whose reliability is, in general, to be questioned (cf. Babylonian Chron. P = Chron. 20B); nos. 29–31 are likely shadow kings during the occupation of Babylonia by Tukulti-Ninurta I. The remainder are named and ordered by modern scholars from the exclusively single-generation genealogies of the later kings and their attested/assumed Assyrian and/or Egyptian contemporaries (see further Mahieu 2021, 93). Given the unequal down-dating of the New Kingdom Egyptian and Middle Assyrian chronologies to be discussed herein, the current ordering of the Kassite dynasty will necessarily be subject to substantial revision and reinterpretation (see James, pp. 37–62, elsewhere in these Proceedings). Note further, that Ea-gamil, the eleventh and final king of the preceding Sealand I dynasty was in fact a contemporary of Ulambur(ar)iash, presumed to be the brother of Kashtiliashu III(?) and son of Burnabur(ar)iash I, the tenth Kassite king; given further that the first six Sealand I Dynasty kings were contemporary with the last five Old Babylonian kings (see Wallenfels, p. 16, elsewhere in these Proceedings), consideration must be given to the possibility of additional such overlapping of dynasties elsewhere in BKL A, if not in the corpus of Assyrian and Babylonian king-lists and chronicles at large.

by the limitations of the preserved Eponym or *līmu* lists) when the first Neo-Assyrian kings emerge from the mists of the preceding Dark Age.

'Overlapping' Royal Lines

James *et al.* (1991, 303) had postulated that in the aftermath of the murder of the Middle Assyrian-period king (78) Tukulti-Ninurta I two 'overlapping' lines of Assyrian kings emerged: one, the initially dominant line of the 'Ilī-iḫaddâ'[8] dynasty, and the second, the obscure predecessors of (98) Ashur-dan II, father of (99) Adad-nirari II, himself the first Assyrian king to be securely dated to the Julian calendar (see *Figure 4* in Wallenfels, p. 19, above).[9] The result of two such overlapping royal lines could, in James *et al.*'s view, result in the shortening of the later Middle Assyrian chronology by some 110 (James *et al.* 1991, 303) to 125 (James 2015, 4–5) years although no specific basis for any number was offered. Nonetheless, in apparent confirmation of such a chronological foreshortening, James *et al.* (1991, 303) proposed that the floruit of the Imperial Hittite Viceroy of North Syria, Ini-Teshub I, resident at Carchemish, is now coincident with Ini-Teshub II, the king of Carchemish who paid tribute to the Middle Assyrian king (87) Tiglath-pileser I. The implications of the identification of Ini-Teshub I with Ini-Teshub II and their impact on Luwian studies are manifest, but again largely beyond the scope of the present discussion.[10]

The regnal dates of the Middle Assyrian kings are believed, by and large, to be independently determined primarily by the data contained in the so-called Assyrian King List (AKL). The Assyrian King List is known today from three largely complete and two fragmentary exemplars.[11] All are similar – but not identical – first millennium Neo-Assyrian copies (see, e.g. Hagens 2005, 27–28). AKL presents the pious fiction of a single continuous line of kings, going back to the Amorite founders of the Old Assyrian Dynasty in what is conventionally the early 2nd millennium BCE. This no doubt reflects the much older southern Mesopotamian tradition exemplified by the 'Sumerian King List', wherein kingship was idealised to have circulated variously over the centuries among the Sumerian city-states in a single continuous unbroken line (Glassner 2004, 55–56, 117–126). Additional factors to take into consideration when evaluating a list such as AKL were enumerated by K. Ryholt (1997, 10–19) in his analysis of the credibility of the Egyptian Turin King-list and include the possibility of omissions, incorrect ordering, incorrect regnal-lengths, incorrect names, the inclusion of fictitious kings, and, as exemplified by his own reconstruction of the Second Intermediate Period (p. 6), the existence of contemporary parallel dynasties spanning significant periods of time.

Untangling the Lines

When A. Poebel (1943) first published his edition of the Assyrian King List from Khorsabad (KhKL), ancient Nineveh, he in fact proposed that four successor-lines arose in parallel in the immediate aftermath of the murder of (78) Tukulti-Ninurta I.[12] Three of these lines were led by his sons (79) Ashur-nadin-apli, Ashurnaṣirpal,[13] and (81) Enlil-kudurri-uṣur. A fourth

[8] The variant ^{I}DINGIR-*i-ḫad/pa-da* in AKL B and C (Grayson 1980–1983, 111), suggests a case of crasis wherein in pronunciation and spelling the *–ī* of the preceding pronominal suffix was assimilated to the following *i-* prefix of the predicate yielding the vocalized form /Ilîḫaddâ/, or the like; cf. III R 4 no. 5 (K. 3045):5 DINGIR-*ḫad-da* a [. . .]. The earlier reading ^{d}PA-DA= Nabu-le'i was rejected by Ungnad (1938, 448).

[9] Year 1 (99) Adad-nirari II = 910 BCE (Millard 1994, 23, rejecting without comment the restoration of the eponymate of Ashur-raim-nisheshu for 903 BCE as proposed by Forrer 1929, 212–213, followed by, e.g. Ungnad 1938, 418; cf. Weidner 1939–1941, 313 [r. (89) Ashur-bel-kala]). For the purposes of internal consistency, all Middle Assyrian regnal dates herein, prior to (99) Adad-nirari II, follow without prejudice Brinkman (1977, 345–346), less one calendar year (see further, below).

[10] See further, James *et al.* (1991, 132–137), esp. Tab. 6:3. Note now, that with the proposed lowering of the date of (87) Tiglath-pileser I by 110–125 years, his contemporary, Taita I, king of Hamath, becomes contemporary with and thus identical with, king Taita II of Hamath. Therefore, Hawkins' observation on the Hamath dynasty may be rephrased to read: 'The combined attestations of Palastin/Walastin suggest the existence in the ~~11th and early~~ 10th cent. of a powerful kingdom centered on the Amuq, controlling under Taita ł Ain Dara and Aleppo, ~~perhaps also Karkamiš~~, and ~~under Taita II, perhaps his grandson~~, as far south as Meharde-Sheizar. This kingdom would have formed in the "dark age" following the dissolution of the Hittite Empire in the territory of its vassal states such as Mukiš, Nuhašše, Halab, Niya and Tunip. By the 9th cent. BC it would have split into the Neo-Hittite ruled states of Unqi and Hamath and the Aramean Bit-Agusi (later Arpad). Thus this group of inscriptions of Palastin/Walastin is gradually revealing the political vicissitudes of north Syria between the Hittite Empire of the Late Bronze Age and the Neo-Hittite and Aramean states of the Early Iron Age' (Hawkins 2016, 190, strike-throughs added). Further, the seemingly inexplicable gap of more than a century noted by Hawkins between the conventional dates for the GÜRÜN and KÖTÜKALE inscriptions on the one hand and the KARAHÖYÜK inscription on the other evaporates in a *CoD*-like chronology (cf. Hawkins 1988, 102, 105–106).

[11] (A) Nassouhi (1927; 'NaKL'); (B) Gelb (1954; 'KhKL'); (C) Gelb (1954; 'SDAS'); (D) Schroeder (1920; KAV 15); (E) Millard (1970, 174–176). For the possible origins of AKL in the reign of Ashur-uballit I, see Valk (2019); cf. Yamada (1994).

[12] Chronicle P (Chron. 22) iv 8–11 (Grayson 1975, 176).

[13] Whether or not Ashur-nadin-apli and Ashurnaṣirpal were two distinct individuals and, if so, who usurped his father's throne and who was responsible for his father's murder has occasioned much controversy. Glassner (2021) has compiled evidence that both figures, in fact, existed (cf. Yamada 1998; Pedersén 1999; Frahm 2009, 119–122;). Note Ashur Stele no. 10 is readily restored to identify one '... ⌜Aššurnaṣirpal⌝ (⌜*aš-šur*⌝-PAP-⌜A⌝) [... son of Tukulti-Ninurta (I)

line was led by one Ilihadda, who was, in fact, the viceroy (*sukallu rabiu*) of Assyria under (78) Tukulti-Ninurta I and the king of Hanigalbat, or better Hani-rabbat,[14] Assyria's at times semi-autonomous western province(s) located across northern Syria, its capital founded by (77) Shalmaneser I at Tell Sheikh Hamad (ancient Dur-Katlimmu) on the Habur. Ilihadda was a member of a cadet royal line descended from an earlier Middle Assyrian king, (76) Adad-nirari I, whose reign is conventionally dated to *c.* the late *14th–early *13th centuries BCE (Szuchman 2007). That at least two 'kings' of Assyria did at some point 'reign' simultaneously is confirmed by a 1st millennium BCE copy of a letter[15] from the Kassite Babylonian king Adad-shuma-usur in which he dismissively addressed together (80) Ashur-nirari III, grandson of (78) Tukulti-Ninurta I,[16] and the aforementioned Ilihadda as the 'kings of Assyria'.[17] Ilihadda, however is not listed as a ruling king in any copy of AKL, although he is acknowledged there as the father of (82) Ninurta-apil-Ekur, whose own direct descendants would continue to rule Assyria for much of the next two centuries.

Removing Spurious Kings

P. Furlong (2010), in his own formulation of an ultra-low Near Eastern chronology, suggested a novel approach to removing some eight-and-a-half decades from the late Middle Assyrian chronology. He proposed that the early Neo-Assyrian king (98) Ashur-dan II, father of (99) Adad-nirari II (910–891 BCE), overthrew his now-suggested-to-be contemporary, the last Middle Assyrian king (94) Ashur-nirari IV. Presumably at some remove thereafter an Assyrian scribe inserted (98) Ashur-dan II's three immediately preceding generations, (97) Tiglath-pileser II, (96) Ashur-resh-ishi II and (95) Ashur-rabi II, into the Assyrian King List.[18] Their deletion from an otherwise conventional AKL-based chronology, labelled 'AC' by Furlong, with some additional minor alterations, would reduce the dates of the reign of, e.g. (87) Tiglath-pileser I from *c.* *1113–*1075 BCE to *c.* 1030–992 AC.[19]

Furlong (2010, 143–170) also argued for a dramatically foreshortened Egyptian Third Intermediate Period, thus lowering the dates of the preceding New Kingdom (Dynasties 18–20). Specifically, he proposed that the dating of the Egyptian 18th Dynasty, as per the High, or Memphite, Chronology, be lowered by exactly 200 years wherein yr. 24 Thutmose III, the year that king received substantial greeting gifts during his Second Campaign from an unnamed Assyrian king,[20] corresponds with the accession year of (73) Ashur-uballit I, in what Furlong terms, the year 1281 AC (Furlong 2010, 24). But this part of his argument is beset by special-pleading (p. 21), and thus has far less to recommend it; on (73) Ashur-uballit I and his dating, see further, below.[21]

Reconnecting Lines

Additional flexibility in the chronological relationship between the two proposed Middle Assyrian royal lines, whether as postulated by James *et al.* (1991) or by Furlong (2010), may be achieved by reassigning (95) Ashur-rabi II as the son of Ashurnaṣirpal 'A', the son of (78) Tukulti-Ninurta I, rather than the son of (92) Ashurnaṣirpal I, son of (91) Shamshi-Adad IV, as is generally assumed (AKL only; cf. Yamada 2003), but for which there is no confirmation in the monuments. This notion was first proposed by B. Newgrosh (1999), along with other alterations which are again beyond the scope of this paper;[22] however, because Newgrosh was operating within the chronological paradigm postulated by D. Rohl (1995), wherein Shishak is identified with Ramesses II rather than Ramesses III, Newgrosh's revised chronology would remove an overly generous 150 years from the Middle Assyrian chronology.

...], son of [Shal]maneser (I), king of the wo[rld, king of] Assur, son of Adad-[nirari (I)], king of Assur' (Andrae 1913, 18–19). Although this stele has been taken to be royal since Andrae's identification of it (p. 5), Glassner (2021, 185) suggests rather that it represents an eponym (cf. Frahm 2009, 121–122).

[14] Valério (2011); cf. the writing *ḫa-bi*-GAL in BKL A iv:12′, 1[4′(?)], 16′, in seventh-century BCE contexts (Fales 2014).

[15] K.1109+K.3045 = ABL 924:(1) [...] ^{1d}*aš-šur-na-ra-ra ù* ^{1}DINGIR-*ḫad-d*[*a*...(2)...] LUGAL.MEŠ ^{kur}*aš-šur*^{ki} ... .

[16] Son of (79) Ashur-nadin-apli (NaKL); son of Ashurnaṣirpal (KhKL, SDAS); cf. n. 13, above

[17] See further, Fales (2014, 224–230).

[18] No royal inscriptions are known for (94) Ashur-nirari IV nor for (95) Ashur-rabi II (Grayson 1991, 125). Cf. James *et al.* (1991, 303).

[19] In possible support of Furlong's notion that a king (or his successors) might have inserted into the king list his (non-canonical?) forebear(s), consider the case of (94) Shalmaneser II: he is acknowledged as the father of (94) Ashur-nirari IV in all three exemplars of AKL, however he is listed as a ruling monarch only in KhKL but is omitted as such in NaKL, while SDAS is broken at this point. But given that Assur Stele no. 14 is inscribed for (93) Shalmaneser (II), son of (92) Ashurnaṣirpal (I), son of (91) Shamshi-Adad (IV), (son of Tiglath-pileser I) and where each is called a 'king of Assur', the NaKL manuscript would appear to be defective here.

[20] Lepsius, *Denkmäler* III, 32, ll. 32–39 = Brugsch, *Thesaurus* 1166-68, ll. 21–28 (Breasted 1906, II, §§ 446, 449).

[21] Cf. e.g., Hagens (2002), who argues for lowering the chronologies of both the Egyptian New Kingdom and the Middle Assyrian kingdom prior to Ashur-dan II by some 80 years.

[22] Newgrosh (1999, 100, Tab. 9), reassigns (80) Ashur-nirari III as the son of Ashurnaṣirpal, son of (78) Tukulti-Ninurta I and the line of (91) Shamshi-Adad IV, (92) Ashurnaṣirpal I and (93) Shalmaneser II as descendents of (97) Tiglath-pileser II, rather than (87) Tiglath-pileser I. (94) Ashur-nirari IV is deemed 'a figment of AKL' (Newgrosh 2007, 249).

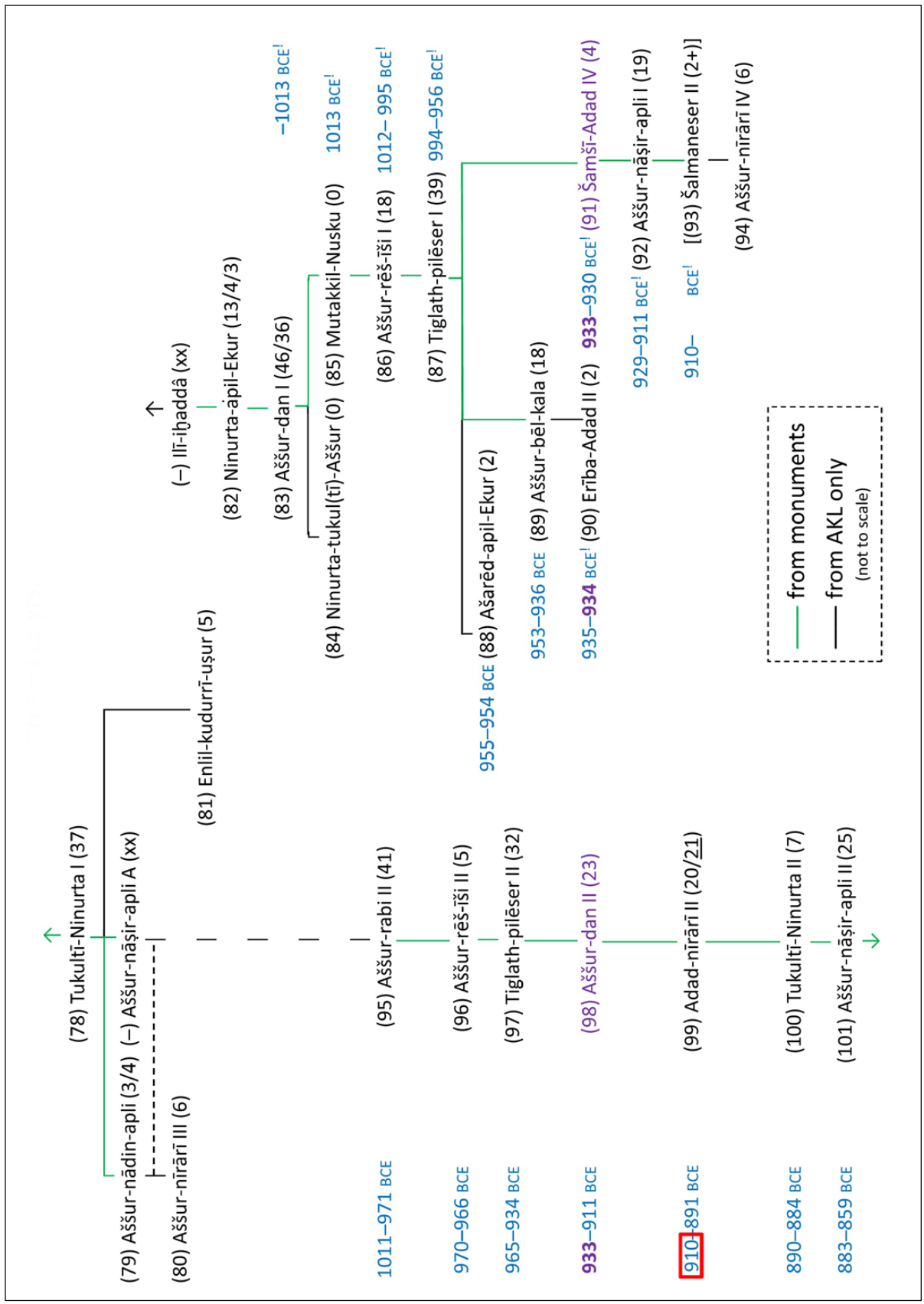

*Figure 2. An alternative restructuring of the late Middle- and early-Neo-Assyrian chronology: yr. 1 (98) Ashur-dan II = 933 BCE = yr. 1 (91) Shamshi-Adad IV (*1052 BCE): Δ = -119 yrs.*

A Working Hypothesis

In light of the foregoing, I would like to propose my own working hypothesis in order to create a baseline from which to proceed further (see *Figure 2*): (1) that, following Poebel (1943), there were four initially more-or-less contemporary successor lines in the aftermath of the death of (78) Tukulti-Ninurta I; (2) that, as per Furlong (2010), (106) Ashur-dan II, or a close successor, did insert Ashur-dan's three immediately preceding male ancestors into the Assyrian King List; (3) that, as per Newgrosh (2007), (95) Ashur-rabi II was a direct successor of, if not the immediate son of, Ashurnaṣirpal 'A', son of (78) Tukulti-Ninurta I, rather than the son of (92) Ashurnaṣirpal I, son of (91) Shamshi-Adad IV;[23] (4) that (98) Ashur-dan II came to power in 933 BCE, not by his overthrow of his presumed contemporary, (94) Ashur-nirari IV, as per Furlong (2010), but rather, again as proposed by Newgrosh (2007, 231–232), in the wake of (91) Shamshi-Adad IV's overthrowing of his nephew, (90) Eriba-Adad II, the previous year;[24] and (5) that the Nassoui redaction of the Assyrian King List was composed during the reign of (99) Adad-nirari II, if not later, rather than during the reign of (98) Ashur-dan II, as is usually assumed (cf. e.g. Brinkman 1973, 314), in order that the now-cadet-line of (91) Shamshi-Adad IV and his successors be included in that, the earliest extant copy of AKL.

This scenario would result in the removal of 119[25] years from the regnal dates of Ilihadda's line of successors and is well within James' proposed 110–125-year reduction in the later Middle Assyrian chronology. Since there are significant discrepancies among the various copies of AKL in the recording of the regnal lengths of several of the kings prior to (86) Ashur-resh-ishi I, (78) Tukulti-Ninurta I's regnal dates, and those of his predecessors, are less certain, potentially fluctuating up or down, at this point, by as much as 21 years (cf. Boese and Wilhelm 1979).[26] In addition, as a result of the use of a purely lunar calendar of 354 days prior to Tiglath-pileser I's conquest of Babylonia and the Assyrian adoption of the Babylonian year of 365 days (Bloch 2012a; 2012b, 334–388; see further Freydank 1991; Cancik-Kirschbaum and Johnson 2011–2012; Jeffers 2017), one year every 33 years must be subtracted from the chronology prior to the reign of Tiglath-pileser I, further complicating the assigning of exact Julian-year regnal dates.

The *Stelenreihen*

A not unreasonable counterargument to creating a break at this point in AKL's recitation of kings might be raised on the basis of the so-called *Stelenreihen*, some 140 stone stelae, many bearing cuneiform inscriptions. These were found at Ashur inside the double ring of fortification walls built by the ninth-century Neo-Assyrian king (102) Shalmaneser III. The excavator, W. Andrae (1913), imagined that these stelae, dated between the conventional *14th and historical 7th centuries BCE, were arranged in two rows: A northern row comprising a list of kings and a southern row with a corresponding list of eponyms, together forming a monumental calendar (viz. Andrae 1938, 53, Fig. 27). P. Miglus (1984), however, after re-examining the published stratigraphy, concluded that the stelae of the eponym officials, of which only 100 of an anticipated 700 were recovered,[27] were not all found at the same level, with the Neo-Assyrian stelae overlying the Middle Assyrian. On the other hand, all of the kings' stelae, again representing only a small fraction of the anticipated full number, were found in the same Neo-Assyrian level; but of these, eight are certainly attributable to specific Middle Assyrian kings,[28] just one to a Neo-Assyrian king[29] and two to Neo-Assyrian royal women.[30] Among the royal stelae, nos. 27 and 15–19 span the stretch of AKL in question, which as a group name some of the otherwise most poorly-attested Assyrian kings. Note that (90) Eriba-Adad II's stele no. 27 is displaced some 60m to the east of stelae nos. 15–19, while stele no. 10, inscribed for Ashurnaṣirpal son of (78) Tukulti-Ninurta I, and who is not named in any king list, is displaced some 50m to the west of the group of those kings with the closest chronological affinity, stelae nos. 24, (76) Adad-nirari I and 23, (77) Shalmaneser I. Crucially, at the very transition point we are examining, only a small, empty, uninscribed stele base fragment,

[23] AKL (95) Ashur-rabi II 'son of' (92) Ashurnaṣirpal I is not supported by the monuments. Compare Richardson (2005, 285, n. 60), commenting on Poebel's (1947) observation that the Neo-Babylonian scribe of Babylonian King List B (BM 38122: Pinches, *PSBA* 3 [1881], 21–22), which lists the eleven Old Babylonian kings on the obverse and the eleven Sealand I kings on the reverse, made systematic mathematical textual restorations, and may likewise have made similar (unwarranted) restorations to the paternities in the previous columns. For the contemporaneity of the last five Old Babylonian kings and the first six Sealand I kings, see n. 7, above.

[24] (91) Shamshi-Adad IV son of (87) Tiglath-pileser I came up from Karduniash to drive off (90) Eriba-Adad II son of (89) Ashur-bel-kala (Assyr. Royal Chron. = Glassner 2004, 143).

[25] Δ = –119 years predicated on yr. 1 Adad-nirari II = 910 BCE (see n. 9, above).

[26] (79) Ashur-nadin-apli (3/4 yrs); (82) Ninurta-apil-Ekur (3/4/13 yrs); (83) Ashur-dan I (36/46 yrs); (84) Ninurta-tukulti-Ashur and (85) Mutakkil-Nusku (0/1 yr.).

[27] For the identification of specific eponyms among the stelae, see Naim (2012; 2021).

[28] Stele no. 24 (76) Adad-nirari I; no. 23 (77) Shalmaneser I; no. 10 Ashurnasirpal 'A'; no. 27 (90) Eriba-Adad II; no. 15 (91) Shamshi-Adad IV; no. 14 (93) Shalmaneser II; no. 12 (96) Ashur-resh-ishi II; no. 11 (97) Tiglath-pileser II.

[29] Stele no. 6 (101) Ashurnaṣirpal II.

[30] Stele no. 5 Semiramus; no. 1 a 'palace-woman' of (113) Ashurbanipal.

no. 13, might potentially represent either (94) Ashur-nirari IV, the last Middle Assyrian king of the Ilihadda dynasty, or (95) Ashur-rabi II, great-grand-father of the first Neo-Assyrian king proper, (98) Ashur-dan II. In this light, Miglus concluded, without hesitation, that the current order of these stelae does not reflect their original placement, but rather their re-placement following the construction of (102) Shalmaneser III's walls in the 9th century BCE, at the earliest.[31] It would appear then that the Neo-Assyrian builders rearranged those stelae on hand in an order vaguely reflecting, at best, the order of the kings as found in Neo-Assyrian-period copies of AKL. The fact that the same string of kings' names occurs in the same order in the Eponym Canon (KAV 21) and in the Synchronistic King List from Assur (14616c), should not suggest the inerrancy of these lists, but rather that all of them were levelled by the historical later 10th–mid-9th centuries BCE to a common arrangement maintaining the pious myth of a single continuous line of kings of Assyria. It is just in this light that the so-called *Distanzangabe* offered in first millennium records involving Assyrian rulers prior to 910 BCE should be considered suspect (cf. e.g. Brinkman 1968, 83–84; Eder 2004; Mahieu 2021).

Case Studies

I shall now present several brief case-studies selected to demonstrate the improvements a *CoD*-like chronology centred on the proposed unequal lowering of the conventional dates of the kings of Egypt (Δ = −248 years) vis-à-vis the Middle Assyrian kings (Δ = −119 years), and those dated by them, respectively, can make in our understanding of details of Middle Assyrian philology and art history.

Anachronisms and Middle Assyrian Scribal Practices

Over the years several distinct features associated with Middle Assyrian cuneiform scribal traditions and the date and means of their spread – especially to the west – have appeared to several Assyriologists to be anachronistic. W. von Soden (1986) was baffled by such Assyrianisms among the letters and documents from Ugarit and in the North Syrian Amarna letters, where he expected to see rather grammatical features associated with the so-called *Randdialekt*, or Peripheral Dialect, of Babylonian Akkadian (p. 106). The problem was made all the more noticeable to von Soden by the absence of such Assyrianisms in the earlier Nuzi texts, conventionally dated to the latter half of the *c.* *15th century BCE, when both Nuzi and Assur, barely 100km apart, were still under the domination of the Hurrian kingdom of Mitanni.

Similarly, W. Moran (1975) noted how the writing and language of the Amarna correspondence of Abdi-Heba of Jerusalem (EA 285–290) most closely parallel those same northern Assyrianising traditions (p. 154) that so perplexed von Soden. Moran no doubt expected to see, rather, the West Semitic-Akkadian *Mischsprache* seen in the Amarna letters of Jerusalem's Canaanite neighbours in the south (p. 146). But Moran, too, was at a loss to account for what appeared to be the most obvious conclusion at which he himself arrived: that '[t]he Assyrian component of these [...] documents [...] is probably to be explained as ultimately reflecting the expansion of Assyrian political power.' And so he could not help but add that 'at the time of the Jerusalem letters, *however late we may date them within the* (conventional) *possible limits of the Amarna archive, such an explanation is improbable*' (emphasis added).

And in yet a third case, A. K. Grayson (1987, 307), in assigning the Shamshi-Belu dedicatory inscription (A.0.83.1002) to the reign of (83) Ashur-dan I rather than (98) Ashur-dan II commented on the occurrence there of the logogram A.BA = *ṭupšarru*, 'cuneiform scribe'. Grayson noted K. Deller's (1982, 151–152) observation that the earliest use of this logogram in Assyria proper occurs only in the time of Tiglath-pileser I, conventionally the late *12th–early *11th century BCE, despite being attested at Ugarit, *c.* *1400 BCE. Grayson added somewhat defensively that this writing is thus 'not unexpected' in a conventional *twelfth-century BCE text such as this.

In our proposed *CoD*-like chronological model, however, the Amarna period is now largely parallel with the reign of (78) Tukulti-Ninurta I, while the following 19th Dynasty and the contemporary kingdom of Ugarit now fall parallel with the reigns of (86) Ashur-resh-ishi I and (87) Tiglath-pileser I. Both are periods of intense Assyrian activity in the west when such Assyrianisms would not be unexpected, thus resolving these seeming Assyriological dilemmas. Although it remains unclear why Abdi-Heba chose to employ an Assyrian-trained scribe at Jerusalem, now at least in the proposed revised chronology the presence of such a scribe is not anachronistic. In the same vein, there is no reason at all now to be surprised to find Assyrian scribes active at Ugarit and Emar.[32]

[31] '[T]here never existed two correlated rows of stelae. We may refer only to different groups of stelae' (Miglus 1984, 137); see further, Millard (1994, 12). For a 7th century date for the final arrangement of the stelae, see Porter (2002, 2005) who argues that Stelae nos. 15–17 – actually reused stone columns – were likely added at that time.

[32] Ugarit: 'Aššur-reš-išši' (reading with Cohen 2017, 278–279), Nahish-shalmu (Cohen 2017, 281 and n. 37); Emar: Mar-Sheru'a (Cohen 2017, 281 and n. 38). For a summary of Assyrian features present among Akkadian cuneiform texts from Ugarit, see Huehnergard (1989, 276–289).

EA 15–16

Just two Middle Assyrian cuneiform letters, EA 15 and 16, each written by an Assyrian king named Ashur-uballit, were apparently recovered in the late eighteenth-Dynasty royal archive in Amarna, Egypt. Numerous commentators have noted that EA 15 is written largely in the contemporary dialect of Assyrian, whereas EA 16 displays features of the more common Babylonian-derived Hurro-Akkadian dialect, von Soden's *Randdialekt*.[33] Neither tablet has been subjected to petrographic analysis (cf. Goren *et al.* 2004), thus it cannot be determined whether one or both originated in Assyria proper or in provincial Syria (Hani-rabbat) or were locally made Egyptian copies.

In EA 15:9–10, Ashur-uballit states, reading with Moran (1992, 38), that 'up to now, my predecessors (*ab-ba-ú-ia*) have not written (to the king of Egypt); today I write you.'[34] However, in EA 16[35] clear references are made to previous contacts between the Egyptians and the Assyrian king's father or ancestor (*a-bi*), one Ashur-nadin-ahhe (l. 19), and to an unnamed 'king of Hani-rabbat' (ll. 22–23), presumably in provincial Syria. Since the Assyrian royal monuments and AKL agree that (73) Ashur-uballit I was the son of (72) Eriba-Adad I, the reference here in EA 16 to Ashur-nadin-ahhe is a curiosity, perhaps a lapse of the stylus. Certainly, significant scribal errors can be identified in the numerous, often contradictory, variations in royal genealogies and regnal lengths throughout the relevant documentation (Brinkman 1973, 311–312). Nonetheless, two Middle Assyrian kings named Ashur-nadin-ahhe are known from AKL, both of whom occupy seemingly equally unimportant side branches off the main royal line without known issue. However, in one of his inscriptions from Assur, (73) Ashur-uballit I son of (72) Eriba-Adad I does make mention of his improvement of a construction project initiated by a prior Assyrian ruler described as the 'choicest among my forefathers, a king who preceded me', one Ashur-nadin-ahhe.[36] Several stamped bricks (Grayson 1987, A.0.71.1) and a fragment of a clay cone (Grayson 1987, A.0.71.2) from Ashur confirm the building activities there of a king by that name. This name also occurs in later references to earlier building projects at Ashur among the inscriptions of (76) Adad-nirari I,[37] (87) Tiglath-pileser I,[38] and (89) Ashur-bel-kala.[39] So it might well be asked: (1) Are all of these references pointing to the same king Ashur-nadin-ahhe, whether the first or the second?; (2) With which Egyptian king, per EA 16, did whichever Ashur-nadin-ahhe correspond?; and, (3) Why did the Ashur-uballit who surely must have dictated EA 15 either not know about or otherwise deliberately ignore that earlier Assyrian-Egyptian correspondence cited in EA 16?

I admit in all candour that I have no ready answers to any of these questions. However, note that in a *CoD*-like timeframe (73) Ashur-uballit I son of (72) Eriba-Adad I, the Assyrian king who installed Kurigalzu I on the Babylonian throne,[40] was long dead by the Amarna period. Thus, it is an open question as to who in fact did write EA 15 and 16 during what is now the period surrounding the murder of (78) Tukulti-Ninurta I when any number of claimants to the Assyrian throne – beyond those already known – might have arisen? Given the obvious discrepancies noted above between these two letters, it is not beyond the realm of possibility that two different claimants to the Assyrian throne each adopted the throne name of Ashur-uballit, the late de facto founder of the entire Middle Assyrian line, as his *nom de guerre*.[41] For now, I shall refer to the writers of EA 15 and 16 when necessary as Ashur-uballit 'A' and Ashur-uballit 'B', respectively, to distinguish them from each other and from (73) Ashur-uballit I, son of (72) Eriba-Adad I and (117) Ashur-uballit II, the last king of Assyria who disappeared from history in 609 BCE (Radner 2019). It remains unclear to which, if either, Ashur-uballit, A or B, Burnaburiash, in his correspondence with Tutankhamun, may have been referring as his 'subject' (*dāgil pānīya*) unauthorized to communicate directly with the Egyptians (EA 9:31–35).

[33] E.g., Artzi (1978, 26 and n. 2); Moran (1992, xix–xx); de Ridder (2018, 27). Surprisingly, contrast Rainey (2015): EA 15 'is linguistically influenced by the peripheral Akkadian dialect of Mitanni' (p. 1347), whereas EA 16 is the only Amarna tablet written in a 'pure (Middle) Assyrian dialect' (p. 1348). For a critical edition of EA 16, see Artzi (1997).

[34] Similarly, Rainey (2015, 129); contrast Grayson (1972, 48), 'Today, I have sent you agreements (*adû*) which my forefathers never sent.' Moran (1984, 298) countered that the term *adû* is an Aramaic loanword 'that entered the language considerably after the time of (73) Ashur-uballit I'; cf. *CAD* A/2, p. 131, s.v. adû A, which gives only first millennium Neo-Assyrian and Neo-Babylonian citations.

[35] Addressed to Akhenaten, Tutankhamen, or Ay (Moran 1992, 39, n. 1).

[36] Certainly taken that way by Grayson (1987, 112) who normalizes the writing $^{\text{I}}$*d-šur-na-din-a-hi* as 'Aššur-nādin-ahhê (II?)' without further comment; similarly, see n. 37. (Grayson 1987, A.0.73.3:15 *-a-ḫi*; note the 'defective' syllabic writing here – indistinguishable from the singular – of the plural for the expected *-aḫ-ḫi* or logographic -ŠEŠ.MEŠ)

[37] Grayson (1987, A.0.76.16:33), where he is also described as 'the choicest among my forefathers'.

[38] Grayson (1991, A.0.87.3:37; 4:54).

[39] (5) *... šá* $^{\text{I}}$*d-šur*-SUM-ŠEŠ.MEŠ MAN KUR $^{\text{d}}$*a-šur* (6) *e-pu-šu* 'which Aššur-nādin-ahhē, king of Assyria, had built' (Grayson 1991, A.0.89.7:v 5).

[40] Cf. Chron. 22 (Chron. P) i 5–14 (Grayson 1975, 171–172); contrast Chron. 21 (SH) i 8′–17′ (Grayson 1975, 159).

[41] Compare the two successive claimants to the Babylonian throne many centuries later following the death of Cambyses, Nidintu-Bel and Arahu, each of whom in their turn took the throne name Nebuchadnezzar, labelled today III and IV, respectively (Lorenz 2008).

A cuneiform-inscribed clay cone fragment from Ashur mentioning an Ashur-uballit, without preserved patronymic,[42] also mentions, just as in EA 16:15, a previous king Ashur-nadin-ahhe (I/II?) and the (re) building of a (re)new(ed) palace at Ashur; the similarities are such to suggest that they may represent the same non-canonical Ashur-uballit 'B', although note that Ashur-uballit I son of Eriba-Adad I also makes mention of the same earlier king and his building activities at Ashur (see n. 37, above). Note also the fragmentary mace head inscribed 'palace of Ashur-uballit' excavated in Hasanlu (HAS 74-318) (reference courtesy Megan Cifarelli, pers. comm.).

A tablet copy of an inscription on a silver drinking-vessel rededicated during the reign of (100) Tukulti-Ninurta II (KAL 3 no. 22), recalls the vessel's original dedication by (78) Tukulti-Ninurta I, and its subsequent restoration by one Ashur-uballit (ll. 7–8), who unlike the other royal names given in this text, lacks patronymic or any accompanying royal or other official titles.[43] Whether this Ashur-uballit was intended to be included in Tukulti-Ninurta II's reference to his ancestors' inscriptions (l. 14) on the vessel remains an open question. Whereas the editor Frahm (2009, 56), operating in the conventional chronology, was at a loss to identify this Ashur-uballit as other than a private individual or the earlier king Ashur-uballit I, in the *CoD*-like chronology presented here, this may well be a reference to a non-canonical king of this name – hence the absence of any patronymic – active towards the end of or just after the reign and murder of Tukulti-Ninurta I, perhaps either Ashur-uballit A or B, as identified above.

RS 34.165

The Akkadian cuneiform letter RS 34.165 (Dietrich 2003, 110–111) was recovered in Ras Shamra, ancient Ugarit, located on the north Syrian coast opposite Cyprus. The main tablet fragment does not retain the names of either the addresser or addressee but does mention in the body of the letter a king Tudhuliya of Hatti and his ultimate defeat at the hands of the Assyrians at the Battle of Nihriya. The letter's address formula is heavily reconstructed from an unassociated flake ordinarily restored to read, '[Thus (says) Shalman]eser (I) king of the land of [Assur], [say to Ibira]na king of the land of U[garit]' (e.g. Dietrich 2003, 112; cf. Schwemer 2006, 254–256).[44] However, this reconstruction should be considered suspect for two reasons. First, the form of the invocation as reconstructed, although compatible with Egyptian and Hittite epistolary practices, is at complete odds with all other known Middle Assyrian letters which are invariably rendered in the exact opposite order, 'Say to PN, thus (says) PN_2'.[45] Second, in the light of our proposed chronological reconstruction, the most likely author of the letter would have been Tiglath-pileser I, or perhaps, his father, Ashur-resh-ishi I, whose name could plausibly be restored in line obv.1′ of the flake.[46] Beyond the scope of the present paper are the associated issues of whether the city of Nihriya (uru*ni-iḫ-ri-ia*) named in this letter and the Na'iri lands ((kur(.kur))*na-i-ri*) of the Assyrian royal inscriptions are but graphical variants of one and the same locale and if so where that might have been.[47]

Anachronisms and Middle Assyrian Glyptic

I would like to turn now to a brief discussion of Middle Assyrian glyptic where the unequal lowering of the dates of the Middle Assyrian kings and of the Egyptian and Kassite kings associated with them, has a most pronounced effect. The establishment and development by (72) Eriba-Adad I of an independent Assyrian state free of Mitannian interference, followed by Assyria's further expansion under his son (73) Ashur-uballit I is believed to be reflected in the new, simpler, bold designs seen in impressions of their respective royal seals.[48] Nonetheless, the depictions of demonic creatures on these seals, according to E. Porada (1979, 7), still echo their Mitannian heritage. But the most dramatic developments in Assyrian glyptic occurred during the thirty-two-year long reign of (76) Adad-nirari I, a period also marked by significant developments in the narration of military events in the now suddenly-numerous cuneiform royal inscriptions,[49] describing

[42] RIMA 1, A.0.73.1002.

[43] Reference courtesy Robert Porter (pers. comm.) who first suggested the text's significance.

[44] (o.1′) [*um-ma-a* $^{\text{I}}$d*šùl-ma-na*]-˹SAG˺ (bl.) LUGAL ˹KUR˺ d˹[*a-šur*$^{\text{ki}}$] (o.2′) [*a-na ...(?)* $^{\text{I}}$*i-bi-ra*]-*na* (bl.) LUGAL KUR ˹*ú*˺-[*ga-ri-it*$^{\text{ki}}$ *qí-bí-ma*].

[45] Cancik-Kirschbaum (1996, 60–61 and n. 60); Llop (2012).

[46] (o.1′) [. . . A(?) $^{\text{I}}$d*aš-šur*]-˹SAG˺-[*i-ši*] LUGAL ˹KUR˺ d˹[*a-šur*$^{\text{ki}}$].

[47] 'The localization of this region is a complex problem. First, we must mention two current equations with Naïri: the first, with Egyptian Nhrn (i.e. Mitani [*sic*]) and Hebrew Aram Naharaim, and the second with Nihriya of the Mesopotamian, Hittite and Urartean sources. The second equation is virtually certain, especially in view of the variant *Ni-i-ri*' (Singer 1985, 105). For the disambiguation of these terms, see now Miller (2012). On the extent of the Na'iri lands, see most recently Summers (2022, 412).

[48] VAT 9009 (Eriba-Adad I) and VAT 8995 (Ashur-uballit I); see conveniently Beran (1957, 145, Fig. 2 and 151, Fig. 17), respectively.

[49] Pongratz-Leisten (2015, 175) notes that beginning in the reign of Adad-nirari I, the cuneiform-sign MAN, which may also refer to the sun-god Shamash (d20), came to be used to write the word 'king' (*šarru*), adding that during 'Adad-nīrārī I's expansion to the west into the region of the Balīh river and the Jazira ... this writing parallels the use of the winged disk as a visual signifier for "king" in Hittite culture.' On the date of the introduction of the winged disc with volutes into North Syrian glyptic at the beginning of the first millennium, see Collon (2001, 35).

not only his large-scale renovation of temples, the ziggurat, the palace, and walls within and around Ashur (Grayson 1972, 128), but more significantly from an annalistic perspective, his wars with Babylonia to the south and with Hani-rabbat to the west (Grayson 1987, 128).The extent of Adad-nirari I's westward expansion is suggested by a tablet copy of a label originally affixed to cedar pillars, booty from Nahur, near Harran (Grayson 1987, A.0.76.25). Note also the mace head inscribed 'Palace of Ashuru-ballit' from Hasanlu (HAS 74-318; M. Cifarelli, pers. comm.).

Middle Assyrian seal composition has been noted for its realistic tendencies and its treatment of landscape (Porada 1948, 67). D. Bonatz (2019, 106) has summarized the scholarly consensus describing Middle Assyrian glyptic as 'vital, original, well-balanced, self-perpetuating, naturalistic, "triangular" in format, and having a high quality.' D. Matthews (1990, 91) observed that '[n]o glyptic style in Mesopotamian history lavished so much imagination on trees as the Middle Assyrian', which are largely restricted to the conventional *13th century BCE. Among these, the most distinctive is a type of tree with twisted trunk and 'globular' crown usually depicted atop a hill typically composed of stacked individual scale-like forms resembling those in the archaic cuneiform-sign KUR which may be translated 'mountain, land', but also 'underworld' (http://psd.museum.upenn.edu/epsd-frame.html, s.v. KUR).

Perhaps the finest rendering of this subject occurs in an incomplete seal impression on the upper edge of a cuneiform tablet in the Louvre (TCL 13 182; Moore 1935, 186–189, no. 182), a letter-order from the *šatammu* 'chief administrator' of the E-ana temple in Uruk in southern Babylonia, dated 520 BCE, early in the reign of the Persian king Darius I. The original seal was in fact a Middle Assyrian heirloom,[50] carved in the so-called Adad-nirari I-style (Matthews 1990, 91–98), with a complex but incompletely known object biography suggesting a close connection between this seal and members of elite families on a generational scale (Wallenfels 2019a). Readily reconstructed on the basis of obvious comparanda (see *Figure 3*),[51] the original seal scene likely depicted a diademed royal archer, bare-chested and wearing a knee-length skirt belted at the waist. Down on one knee, he fully draws his bow and arrow and threatens a facing rampant goat resting its right front hoof on a limb of a 'globular' tree with twisted trunk that visually separates the archer from his prey. From each of some half-dozen main limbs branching off the central twisted trunk of the tree, there are some three dozen individual branches, each terminating with a single slender leaf. No trace of the expected hill beneath the tree is preserved in the impression, but the comparanda suggest that it was composed of four tiers of semi-circular 'scale'-like mountains, stacked one on top of two on top of three on top of four; the bottom of the tree-trunk was likely fused with the upper-most single mountain-scale. The lower halves of two incompletely impressed rayed astral symbols, one encircled, occur side-by-side near the upper edge of the impression.

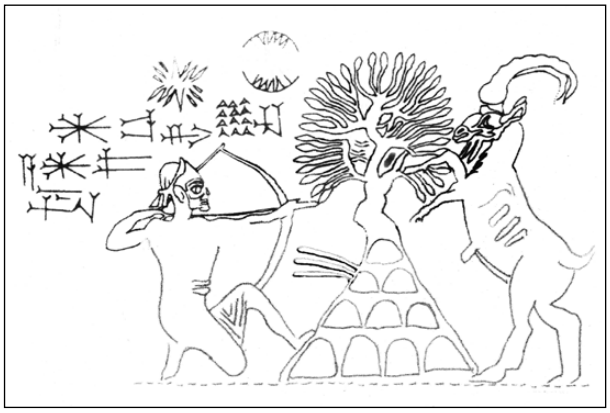

Figure 3. Artist's reconstruction of the impression of a re-inscribed Adad-nirari I-style Middle Assyrian seal as reused in the Persian period (drawing by C. M. Herriges and R. Wallenfels, after E. Ehrenberg 1999, Pl. 28, no. 217).

The upper half of the impression is incompletely preserved to a height of some 16mm (Ehrenberg 1999, 102) – the expected upper horizontal terminal line is not preserved, suggesting the original seal must have stood at least 32mm tall, putting it on par with other Middle Assyrian seals of similar style and related motif. The dimensions of the impression, which was rolled out through one and two-thirds rotations on the tablet's upper edge, are given as 47×16mm (Ehrenberg 1999, 102, no. 217). Although the tablet's thickness is given as 28mm, because the tablet edge is convex in cross-section, the full width of the edge is only flat enough towards its midline to accommodate, in this case, most of the upper half of the seal where all the formal interest lies. The exquisite modelling of the original seal is still evident in the impression and it thus may well serve as the example *par excellence* of the Adad-nirari I seal style.

Perhaps within a century or so after the original seal was manufactured, a horizontal cuneiform inscription cut in reverse to be read in the impression was added in the once-open area between the opposed backs of the archer and goat (Wallenfels 2019a). It identifies one Shamash-aha-iddina, son of Ilihadda, the latter likely the *sukallu*

[50] Delaporte (1923, 175, no. A.795 A) did not note its non-Neo-Babylonian origins; Matthews (1990, 10, n. 81, no. 312) certainly recognized its Middle Assyrian pedigree, but it was Ehrenberg (1999, 102, no. 217) who first suggested that the inscription was a later addition, although she assumed it to be Neo-Babylonian; for the identification and earlier dating of the added inscription, see Wallenfels (2019a).

[51] E.g., de Clerq (1888, no. 311); Moortgat (1942, 59, Fig. 16); Porada (1948, nos. 600, 603).

rabiu 'grand vizier', king of Hani-rabbat, a descendent of (76) Adad-nirari I, at the time of the murder of (78) Tukulti-Ninurta I, and the father of the eventual solitary successor to the Assyrian throne, (82) Ninurta-apil-Ekur (Szuchman 2007). It is not known who owned the seal prior to Shamash-aha-iddina, although it was likely a high-ranking official in the court of Adad-nirari I, if not the king himself; nor is it known how Shamash-aha-iddina came into its possession, nor how the original Assyrian seal came centuries later to be in the hands of the *šatammu* of E-ana in Uruk in southern Babylonia.

The diademed half-kneeling archer on Shamash-aha-iddina's seal, and a similar, but standing diademed archer on a seal owned by one otherwise unknown Adad-ishmeshu (BN 367 = Matthews 1990, no. 308) might be intended to be idealizing images of (76) Adad-nirari I himself. But it is the tree in the Louvre seal impression that I wish to focus on here first. Previous commentators have observed that this sort of tree already existed in what is conventionally *c.* *14th century BCE Babylonia, as depicted in the seals of two *ša-rēši*-officials, Kidin-Marduk, son of Sha-iluma-damqa (Porada 1981, no. 26) and Kidin-Marduk's son, Adad-ushabshi (Muscarella 1981, no. 77), both of whom served a Kassite king Burnaburiash (cf. Brinkman 1968, 310). However, Ehrenberg (1999, 30) observed that in these Second Kassite-style seals, the tree's branches radiate from a central stylized twisted and branched trunk and lie within a fully circular crown whose perimeter, in the case of the Adad-ushabshi seal, is actually drawn. These are quite unlike the Adad-nirari I-type where the more naturalistic tree branches and stems are modelled individually and the crown is more hemispheroidal; versions of fully circular globular tree crowns do occur in Middle Assyrian seals, but mostly later, as in, e.g. Porada (1948, no. 599), dated to the reign of (77) Shalmaneser I in the conventional *13th century BCE, where the tree's now-fully circular crown has been reduced to a series of radially-arrayed slashes of a cutting wheel.[52] In the conventional chronology, the Burnaburiash mentioned in these seal inscriptions is assumed to be the king of the same name in the Amarna correspondence (EA 6–12, 14) of the conventional mid- to late-*14th century BCE, thus Burnaburiash II,[53] while (76) Adad-nirari I is conventionally dated later to the late *14th through early *13th centuries BCE. However, in a *CoD*-like chronology, (76) Adad-nirari I is redated to the first half of the 12th! century BCE, about a half-century *earlier* than the Amarna period which is itself redated to what is now the end of the 12th! and earlier 11th! centuries BCE.[54] Thus, the distinctive tree with twisted trunk and globular crown on a stylized mountain is not an invention of the Second Kassite style, but rather the invention of (76) Adad-nirari I's artisans at least a half century earlier. In this light, the globular trees on the Babylonian seals of Kidin-Marduk and his son, both early examples of the new Second Kassite style,[55] absent the Assyrian original's 'naturalness', betray their derivativeness by the very features noted earlier by Ehrenberg (1999, 30), including their schematized twisted trunk and fully circular crown with radially arrayed leaves and twigs, only some of which are attached to inner limbs. Just such simplifications are beginning to occur within the Adad-nirari I-style seals as well as in the succeeding Shalmaneser I style (e.g. Porada 1948, nos. 599–603). That there is a connection between the Second Kassite style and the Adad-nirari I style is long recognized, but which style influenced which is now subject to significant re-interpretation.

The other feature of note here, the half-kneeling archer drawing on a prey animal, now, in the revised chronology, also appears first in Middle Assyrian art. Matthews (1990, 22–23) cogently described the archer's posture as 'the one most specifically Assyrian' and is never seen in Mitannian[56] or First Kassite seals but does occur in 'sub-Assyrian' styles, such as Second and Third Kassite styles and Levantine, Egyptianizing and Cypriote seals. The obviously mythological scene of the royal hunter *par excellence* in this singular landscape recalls the question Marduk asks of Erra in the *Poem* of the same name (I 150–153):

> 150- Where is the *mēsu*-tree (giš$_{MES}$), the flesh of the gods, the ornament of the king of the universe,

[52] Compare Bonatz (2021, 63, no. M.As 38; Tell Fekheriye), with tree twisted trunk and circular crown surrounding leaves without stems.

[53] Brinkman (1976, 100) has noted that '[i]t is uncertain how many Kassite kings there were named Burna-Buriaš. Besides the well-known Burna-Buriaš of Amarna times, there was at least one earlier king of the same name, and possibly two.'

[54] To put these events into proper perspective, in the *CoD*-like chronology used herein Tukulti-Ninurta I (r. *c.* 1123!–*c.* 1087! BCE) and Akhenaten (reign. *c.* 1104!–*c.* 1088! BCE) appear to have died within one year of each other (subject, of course, to the assumptions and approximations already made).

[55] Kidin-Marduk also owned a seal in the older traditional First Kassite style (Moortgat 1940, no. 554), as did his father, Sha-iluma-damqa (Porada and Collon 2016, no. IK20).

[56] Not noted by Matthews is the half-kneeling archer drawing directly on a reversed lion in a fragmentary seal impression on a Nuzi tablet dated to the second generation of the Tehip-tilla family archive of the latter-half of the conventional *15th century BCE (Porada 1947, no. 914); Porada (p. 86) noted especially close similarities between her no. 914 and an Assur tablet seal impression dated to the eponomy of Ishtar-erish (Moortgat 1944, p. 58, Fig. 12, "wahrscheinlich zur Zeit Tukulti-Ninurtas I"), now datable to the reign of (77) Shalmaneser I (Freydank 1991, p. 145, s.v.) in the conventional *13th century BCE. Because the history of Mitanni is dependent upon both Egyptian (and associated Hittite) and Assyrian sources, which are unequally down-dated in the present *CoD*-like model, a significant revision of Mitanni's history is indicated (see Wallenfels and James elsewhere in these Proceedings, pp. 29–36).

> 151- That pure tree (*iṣu*), that 'august stripling (*eṭlu*)' suited to supremacy,
>
> 152- Whose roots reached as deep down as the bottom of the underworld – a hundred double-hours through the vast sea waters,
>
> 153- Whose top reached as high as the sky of [Anu]?
>
> (trans. after Cagni 1977, 32; cf. Foster 1993, 779).

The pairing of the half-kneeling archer and the globular tree with twisted trunk on a hill rendered to suggest the cuneiform sign KUR, Sumerian for both 'mountain' and 'underworld', is the visual embodiment of the pun inherent in line 150 between Akk. *mēsu* a (native) tree (*CAD* M/2, s.v.) and Sum. MES 'tree; young man' (http://psd.museum.upenn.edu/epsd-frame.html, s.v.), made explicit in the following line by paralleling Akk. *iṣu* 'tree' with Akk. *eṭlu* 'young man' (Foster 1993, 779, n. 5). In the third line (l. 152), the scribe chose the poetic Akk. *arallû* for 'underworld' (*CAD* A/2, s.v.), itself a loan-word from Sumerian where it is a synonym for KUR. Scholarly suggestions, ancient and modern, for the date of composition of this 'song' by a scribe – most unusually known by name, Kabti-ilani-Marduk son of Dabibi – range from the reign of Ibbi-Sin at the end of the Ur III period, in what is conventionally the end of the third millennium, to the reign of (111) Sennacherib (704–681 BCE; Frahm 2010, 7). A. George (2013, 47) has suggested that its 'historical background is a long period of weak rule in Babylonia punctuated by violent disorder', specifically that period during the Aramaean incursions beginning in the conventional *11th century BCE and continuing into the historical 8th century BCE (see further, below). Copies of the composition have been recovered in sites across Syria, Assyria and Babylonia, none dated prior to the 1st millennium BCE (Cagni 1977, 5).

The Spread of Middle Assyrian Glyptic Innovations

The motif of the half-kneeling archer and prey is widely distributed among cylinder seals dated to the conventional mid-*13th through early *12th centuries BCE. These include Pseudo-Kassite seals from both Babylonia and Elam (e.g. Matthews 1990, nos. 259–265), which post-date the Kassite Second Style (Matthews 1990, 69), and may now be redated to the late 12th! to mid-11th! century BCE, and Middle Elamite cylinders from Choga Zanbil in south-west Iran,[57] dated conventionally to the reign of Untash-Napirisha in the latter half of the *14th century BCE (Potts 2016, 197–223), here redated to *circa* the first half of the 11th! century BCE, thus parallel with the latter half of the reign of (78) Tukulti-Ninurta I through the reign of (82) Ninurta-apil-Ekur. In the west, the half-kneeling archer motif occurs among cylinder seals from Ras Shamra, ancient Ugarit (Amiet 1992, nos. 168, 316, 318, 320–323) and Meskene, ancient Emar (Beyer 2001, E43a, E78, G6), conventionally dated to the *13th century BCE; these too are to be redated to the late 11th! or early 10th! centuries BCE. Most significantly, in a *CoD*-like short chronology, *all* of the comparanda are to be dated later than the (76) Adad-nirari I originals.

There is now less than a century separating early historical ninth-century Neo-Assyrian linear-style cylinder seals depicting a half-kneeling archer and prey (e.g. Collon 2001, nos. 14–26) from their so-called Late Bronze Age antecedents. This is exactly the time period when climatologists tell us the prolonged drought of the so-called 3.2 ka BP climate event besieged the ancient Near East. On the evidence of the unequally down-dated Egyptian and Middle Assyrian chronologies as just outlined, the disastrous consequences of this event – dam building, massive grain shipments to affected areas, famine, population displacement, even cannibalism – appear to have peaked during what is now the first half of the 10th! century rather than spread out more broadly over several centuries (Neumann and Parpola 1987; Divon 2008, 101–109; Kaniewski *et al.* 2015).

Conclusions

The purpose of these case studies has been to determine, for a given object or class of objects, here Middle Assyrian cuneiform texts and cylinder seals, whether a *Centuries of Darkness*-like revised dating scheme for the ancient Near East is productive? I believe I can say in all honesty that in each case presented here and in others to be included in future studies, there are innumerable improvements to be had in our understanding, which in some cases are admittedly small, but in others, quite significant, even revolutionary. Further, the newly determined solutions give every indication that additional improvements overall are to be had for the asking.

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[57] Porada (1970, 39–41, Group IV, nos. 33–36, 38). On the further distinctions between the Pseudo-Kassite and Elamite styles, see Amiet (1986), Matthews (1990, 66–70).

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Two Parallel Lines of Kings during the Late Middle and Early Neo-Assyrian Periods

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Though the conventional Assyrian chronology has been reconstructed with much competence and profound knowledge of the ancient sources, it also faces some shortcomings, in particular the supposed 'Dark Age' that lasted for the second half of the Middle Assyrian period. It is proposed here to remove this poorly attested period by presuming that the ill-attested Middle Assyrian kings ruled contemporaneously with the well-attested kings and that there was not one successive line of rulers, contrary to the apparent presentation in the Assyrian King List*, on which the conventional chronology is based. In order to trace possible parallelisms between kings, the available data on the sequences of the kings and on the historical contexts should be reconsidered. This will help to establish the relationship between the ruling branches and to fix their respective locations. The data suggest that, beginning from the death of Tukulti-Ninurta I until the accession of Shalmaneser III, two lines were ruling in Assyria, residing in Assur and Hanigalbat (and later in Assur and Nineveh).*[1]

1. The Sequences of the Kings

To work out the hypothesis that some Assyrian kings ruled contemporaneously, the true, historical sequence of the kings should be established. The *Assyrian King List* (henceforth *AKL*) gives the impression that all the kings reigned in one, successive line, but possible parallel reigns might be concealed. References to kings in documents outside of the *AKL* help to evaluate the sequential data found in the *AKL*.[2] Two types of information are particularly relevant: (1) references to activities by former kings and (2) filiations.[3]

1.1. References to activities by former kings

Old Assyrian period

- Shamshi-Adad I rebuilds a temple built by Erishum I, the son of Ilushuma (RIMA 1 A.0.39.1, ll. 18–19).
- Shamshi-Adad I mentions a temple built by Ikunum, the son of Erishum I (RIMA 1 A.0.39.3, obv. 5–6).
- Puzur-Ashur III mentions a temple built by Ilushuma and restored by Sargon I, the son of Ikunum (RIMA 1 A.0.61.2, ll. 6–9).
- Enlil-nasir I mentions towers in relation to Ishme-Dagan I, the son of Shamshi-Adad I (RIMA 1 A.0.62.1, ll. 7–8).
- Ashur-bel-nisheshu mentions a wall built by Puzur-Ashur (III?, RIMA 1 A.0.69.1, l. 6).
- Ashur-rim-nisheshu mentions a wall built by Kikkia, Ikunum, Sargon I, Puzur-Ashur II and Ashur-nirari I, the son of Ishme-Dagan II (RIMA 1 A.0.70.1, ll. 5–7).

Middle Assyrian period

- Ashur-uballit I mentions a canal dug (RIMA 1 A.0.73.3, l. 15) and a wall built (RIMA 1 A.0.73.1002, l. 5′) by Ashur-nadin-ahhe (II?).
- Ashur-uballit I follows Ashur-nadin-ahhe (II?) in time (EA 16, l. 19).[4]

[1] I would like to thank Peter James†, Robert Porter and Ronald Wallenfels for their suggestions.

[2] The study numbers the columns and lines of the *AKL* according to the Khorsabad recension (IM 60017; henceforth Khors.). Sometimes the Nassouhi recension (Istanbul A. 116 [Assur 8836]; henceforth Nass.) and the Seventh Day Adventist Seminary recension (IM 60484; henceforth SDAS) are referenced.

[3] The present survey ends with Ashur-nirari V, the last king recorded in the Khors. *AKL* (iv.31–32).

[4] Knudtzon (1908, 128): ^{d}*a-šur-na-din-aḫē a-bi*. Kühne (1987, 258 n. 157): '*abu* "Vater" bedeutet hier "Vorgänger"'; cf. the translation by Moran (1992, 39): 'Aššur-nadin-aḫḫe, my ancestor' (though Rainey 2015, 131: 'my father Aššur-nadin-aḫḫē'). Ashur-uballit I (Khors. *AKL* iii.13–14) is the second successor of Ashur-nadin-ahhe II (iii.9–10), but he is only distantly related to him (Ashur-uballit I is the son of Eriba-Adad I, the son of Ashur-bel-nisheshu, iii.11–14 and see §1.2). Neither does Ashur-uballit I belong to the lineage of Ashur-nadin-ahhe I (ii.47).

- Arik-den-ili refers back to Eriba-Adad I (RIMA 1 A.0.75.1, l. 46).
- Adad-nirari I mentions a temple built by Ilushuma, the son of Shalim-ahum, restored by Sargon I, the son of Ikunum, and restored by Puzur-Ashur III, the son of Ashur-nirari I (RIMA 1 A.0.76.15, ll. 7–14).
- Adad-nirari I mentions a wall built by a king called Puzur-Ashur (III?, RIMA 1 A.0.76.10, l. 37) and altered by Ashur-bel-nisheshu and Eriba-Adad I (RIMA 1 A.0.76.13, ll. 33, 35).
- Adad-nirari I mentions a palace built by Ashur-nadin-ahhe II (RIMA 1 A.0.76.16, l. 33).
- Adad-nirari I mentions a wall built by Ashur-uballit I (RIMA 1 A.0.76.11, ll. 3′, 25′, 28′).
- Shalmaneser I mentions a temple built by Ilushuma, the son of Shalim-ahum; restored by Sargon I, the son of Ikunum; restored by Puzur-Ashur III, the son of Ashur-nirari I; and restored by Adad-nirari I (RIMA 1 A.0.77.6, ll. 6–15).
- Shalmaneser I mentions a palace complex built by Ashur-nirari I, the son of Ishme-Dagan II (RIMA 1 A.0.77.10, ll. 6–7).
- Shalmaneser I mentions a temple restored by Ashur-uballit I (RIMA 1 A.0.77.17, ll. 7, 9–10).
- Ashur-resh-ishi I refers to destructions caused by earthquakes during the reigns of Shalmaneser I and Ashur-dan I (RIMA 1 A.0.86.1, ll. 9–10).[5]
- Tiglath-pileser I mentions a gate built by Puzur-Ashur III, the son of Ashur-nirari I (RIMA 2 A.0.89.3, l. 10′).[6]
- Tiglath-pileser I mentions a wall (RIMA 2 A.0.87.3, l. 37) and a house of the *labūnu* (RIMA 2 A.0.87.4, l. 54) built by Ashur-nadin-ahhe, and a house of the *šaḫūru* built by Tukulti-Ninurta I (RIMA 2 A.0.87.4, l. 52).
- Tiglath-pileser I mentions constructions by Eriba-Adad I, Ashur-nadin-ahhe, Ashur-dan I and Adad-nirari I (RIMA 2 A.0.89.7, v.4–5, 20, 25).[7]

Neo-Assyrian period

- Ashur-dan II mentions Shalmaneser (I?) and Ashur-rabi II (RIMA 2 A.0.98.1, ll. 16, 24).
- Ashur-dan II refers back to the Craftsman's Gate built by Tiglath-pileser I, the son of Ashur-resh-ishi I, the son of Mutakkil-Nusku (RIMA 2 A.0.98.3, ll. 6–7).
- Adad-nirari II mentions a temple built by Adad-nirari I, the son of Arik-den-ili (RIMA 2 A.0.99.1, rev. 11′).
- Adad-nirari II refers back to Tiglath-pileser (I?), the son of Ashur-resh-ishi (I?), and to Tukulti-Ninurta I, the son of Shalmaneser I (RIMA 2 A.0.99.2, ll. 52–53, 84–85 = 52–53, 128).
- Tukulti-Ninurta II mentions a wall rebuilt by Ash[ur-uba]llit I (RIMA 2 A.0.100.5, l. 137).[8]
- Ashurnasirpal II refers back to Shalmaneser (I?, RIMA 2 A.0.101.1, i.102; A.0.101.17, v.1; A.0.101.26, l. 46; A.0.101.28, v.1).
- Ashurnasirpal II mentions a temple built by Shamshi-Adad (I or IV?, RIMA 2 A.0.101.40, l. 31).
- Shalmaneser III mentions a temple built by Tukulti-Ninurta I, the son of Shalmaneser I (RIMA 3 A.0.102.49, l. 3).[9]
- Shalmaneser III mentions a temple built by Tiglath-pileser I, the son of Mutakkil-Nusku (RIMA 3 A.0.102.39, ll. 7–8).
- Shalmaneser III refers back to Tiglath-pileser I (RIMA 3 A.0.102.6, i.60), and to Tiglath-pileser I and Ashur-rabi II (RIMA 3 A.0.102.2, ii.37).
- Shalmaneser III mentions a gate built by Ashur-dan II, the son of Tiglath-pileser II (RIMA 3 A.0.102.46, ll. 5–6).[10]
- Shalmaneser III refers back to a wall built by Puzur-Ashur III, the son of Ashur-nirari I; and subsequently built by Adad-nirari I, the son of Arik-den-ili; by Tukulti-Ninurta I, the son of Shalmaneser I; and by Tiglath-pileser I, the son of Ashur-resh-ishi I (RIMA 3 A.0.102.10, iv.41–45).[11]

1.2. Indications of filiations

The manner in which kings affiliate themselves in their inscriptions provides important data on the sequence of the kings.[12]

Old Assyrian period

- Shalim-ahum is the son of Puzur-Ashur I.
- Ilushuma is the son of Shalim-ahum, the son of Puzur-Ashur I.

[5] Cf. RIMA 1 A.0.86.2, l. 5 and A.0.86.3, l. 4′: an earthquake at the time of Ashur-dan I mentioned by Ashur-resh-ishi I.

[6] For the possible attribution of the inscription to Tiglath-pileser I rather than to Ashur-bel-kala (the conventional proposal), see Mahieu (2018, 85–86), and n. 76 below.

[7] The inscription is conventionally attributed to Ashur-bel-kala; see Mahieu (2018, 79–85); Shibata (2022).

[8] Cf. RIMA 2 A.0.100.3 rev. 8′ (in a lacuna). The earlier reading of the royal name as Ash[ur-uba]llit I is preferable to the later restoration as Ash[ur-bel-k]ala: Mahieu (2020c).

[9] Cf. RIMA 3 A.0.102.50, l. 4; A.0.102.51, l. 4.

[10] Cf. RIMA 3 A.0.102.47, l. 5.

[11] Cf. RIMA 3 A.0.102.26, ll. 19–20; A.0.102.27, l. 7.

[12] For the editions of these inscriptions, see RIMA 1, 2 and 3.

- Erishum I is the son of Ilushuma , the son of Shalim-ahum, the son of Puzur-Ashur I.
- Ikunum is the son of Erishum I, the son of Ilushuma, the son of Shalim-ahum, the son of Puzur-Ashur I.
- Sargon I is the son of Ikunum.
- Shamshi-Adad I is the son of Ilu-kabkabi.
- Shamshi-Adad III is the son of Ishme-Dagan II.
- Ashur-nirari I is the son of Ishme-Dagan II.
- Puzur-Ashur III is the son of Ashur-nirari I.
- Enlil-nasir I is the son of Puzur-Ashur III.
- Ashur-rabi I is the son of Enlil-nasir I.
- Ashur-bel-nisheshu is the son of Ashur-nirari II.
- Ashur-rim-nisheshu is the son of Ashur-nirari II, the son of Ashur-rabi I.
- Eriba-Adad I is the son of Ashur-bel-nisheshu, the son of Ashur-nirari II, the son of Ashur-rabi I.

Middle Assyrian period

- Ashur-uballit I is the son of Eriba-Adad I, the son of Ashur-bel-nisheshu, the son of Ashur-nirari II, the son of Ashur-rabi I, the son of Enlil-nasir I, the son of Puzur-Ashur III.
- Enlil-nirari is the son of Ashur-uballit I, the son of Eriba-Adad I.
- Arik-den-ili is the son of Enlil-nirari, the son of Ashur-uballit I.
- Adad-nirari I is the son of Arik-den-ili, the son of Enlil-nirari, the son of Ashur-uballit I.
- Shalmaneser I is the son of Adad-nirari I, the son of Arik-den-ili, the son of Enlil-nirari, the son of Ashur-uballit I.
- Tukulti-Ninurta I is the son of Shalmaneser I, the son of Adad-nirari I.
- Ashur-nadin-apli is the son of Tukulti-Ninurta I, the son of Shalmaneser I.
- Ninurta-apil-Ekur is the son of Ilipada.
- Ashur-dan I is the son of Ninurta-apil-Ekur.
- Ashur-resh-ishi I is the son of Mutakkil-Nusku, the son of Ashur-dan I.
- Tiglath-pileser I is the son of Ashur-resh-ishi I, the son of Mutakkil-Nusku, the son of Ashur-dan I, the son of Ninurta-apil-Ekur.
- Ashur-bel-kala is the son of Tiglath-pileser I, the son of Ashur-resh-ishi I.
- Eriba-Adad II is the son of Ashur-bel-kala, the son of Tiglath-pileser I.
- Shamshi-Adad IV is the son of Tiglath-pileser I, the son of Ashur-resh-ishi I.
- Ashurnasirpal I is the son of Shamshi-Adad IV.
- Shalmaneser II is the son of Ashurnasirpal I, the son of Shamshi-Adad IV.
- Ashur-resh-ishi II is the son of Ashur-rabi II.
- Tiglath-pileser II is the son of Ashur-resh-ishi II.

Neo-Assyrian period

- Ashur-dan II is the son of Tiglath-pileser II, the son of Ashur-resh-ishi II, the son of Ashur-rabi II.
- Adad-nirari II is the son of Ashur-dan II, the son of Tiglath-pileser II, the son of Ashur-resh-ishi II.
- Tukulti-Ninurta II is the son of Adad-nirari II, the son of Ashur-dan II.
- Ashurnasirpal II is the son of Tukulti-Ninurta II, the son of Adad-nirari II, the son of Ashur-dan II.
- Shalmaneser III is the son of Ashurnasirpal II, the son of Tukulti-Ninurta II, the son of Adad-nirari II.
- Shamshi-Adad V is the son of Shalmaneser III, the son of Ashurnasirpal II.
- Adad-nirari III is the son of Shamshi-Adad V, the son of Shalmaneser III, the son of Ashurnasirpal II, the offspring of Adad-nirari II, the offspring of Tukulti-Ninurta I, the offspring of Shalmaneser I, the offspring of Ilu-kabkabi (Enlil-kapkapi), a predecessor from before the sovereignty of Sulili (see §3.3).
- Shalmaneser IV is the son of Adad-nirari III, the son of Shamshi-Adad V.
- Ashur-dan III is the son of Adad-nirari III, the son of Shamshi-Adad V.
- Ashur-nirari V has no epigraphical data regarding his filiation.

Both the references to activities by former kings and the indications of filiations accord with the sequence of the kings found in the *AKL*. This implies that the order in the *AKL* should nowhere be inverted. It must be questioned, however, if somewhere breaks in the sequence occur.

2. The Historical Contexts of the Two Lines

A previous study has shown that a (short) double line of rulers is concealed in the *AKL* for the Old Assyrian period: the 40 years of Ishme-Dagan I (Khors. *AKL* ii.2–3) apparently ran parallel to the reigns of Ashur-dugul

(Khors. *AKL* ii.4–5) and his successors, whereas the *AKL* records Ishme-Dagan I and Ashur-dugul successively.[13] A similar, though longer, double line might exist for the Middle Assyrian period, the period on which the present article will focus.

2.1. The context of the introduction of the two lines

If the Middle Assyrian period should be reduced in time, somewhere during this period kings must have been contemporaneous. From Eriba-Adad I (Khors. *AKL* iii.11–12) until Enlil-kudurri-usur, the son of Tukulti-Ninurta I (iii.25–26), the lineage is unbreakable. Hence, any interruption must occur later. The context of Tukulti-Ninurta I's death offers a possible setting for the institution of parallel lines. Just as the death of the important Old Assyrian king Shamshi-Adad I caused the introduction of two lines of Assyrian rulers (headed by Ishme-Dagan I and Ashur-dugul; see n. 13 above), so the death of the important Middle Assyrian king Tukulti-Ninurta I might have instigated parallel lines of rulers. Tukulti-Ninurta I had ruled a vast empire (including Babylonia, for seven years: *Chronicle P* iv.7–8), and turmoil occurred at the end of his reign, resulting in his murder (*Chronicle P* iv.10–11). During these troubles, several pretenders may have made a claim to the throne.[14] Ashur-nadin-apli, the successor of Tukulti-Ninurta I (see §2.3), bore the rare[15] title 'king of kings' (RIMA 1 A.0.79.1, l. 4), which might indicate that he held some kind of suzerainty over small Assyrian kingdoms or principalities and that the Assyrian Empire was divided.[16] A study on the probability of a parallel line of rulers for the entire Kassite period has similarly shown that the immediate successors of Tukulti-Ninurta I in Assyria seemingly ruled in a parallel line.[17]

In the period following Tukulti-Ninurta I, there are two points where breaks in the sequence of the kings are possible: (1) the accession of Ninurta-apil-Ekur and (2) the accession of Ashur-rabi II. It will be argued below that the first case inaugurated a new dynastic line that ran parallel to the descent of Tukulti-Ninurta I, while the second introduced a new line of rulers within the descent of Tukulti-Ninurta I.

2.2. Line 2: A new line inaugurated by Ninurta-apil-Ekur

Tiglath-pileser I traced his ancestry back to Ninurta-apil-Ekur (RIMA 2 A.0.87.1, vii.36–59). The fact that Tiglath-pileser I (and the other kings; see §1.2) did not trace his ancestry any further back than Ninurta-apil-Ekur might imply that Ninurta-apil-Ekur founded a new dynastic line.[18] Moreover, Ninurta-apil-Ekur used the uncommon title 'designate of Enlil and Ninurta' in both of his two sole inscriptions (RIMA 1 A.0.82.1–2). His appointment by the gods would indicate a new reign, unconnected to the preceding kings.[19] Similarly, the daughter of Ninurta-apil-Ekur, Muballita[t-...], was a high priestess (*ēntu*) of an unspecified god/dess.[20] Since this is the only Middle Assyrian attestation of a high priestess,[21] it might reflect the inauguration of a new line, with a particular cult, as well.

The background of that line might be found in the kingdom of Hanigalbat. When Shalmaneser I, the father of Tukulti-Ninurta I, conquered the area of Hanigalbat,[22] he instituted the Assyrian viceroyalty of Hanigalbat.[23] The last grand vizier (viceroy), Ilipada[24] (attested with both the titles 'king of Hanigalbat' and 'grand vizier'), was the son of the 'grand vizier' Ashur-iddin and the grandson of the 'king of Hanigalbat' and

[13] Mahieu (2020a).

[14] James (1991, 303) proposes two parallel dynastic lines for the period following Tukulti-Ninurta I's death. Poebel (1943, 56–57) assumes that, after the death of Tukulti-Ninurta I, Assyria was split into four kingdoms: the kingdoms of Ashur-nadin-apli, Ashurnasirpal (a son of Tukulti-Ninurta I; see §2.3), Enlil-kudurri-usur (a further son of Tukulti-Ninurta I; Khors. *AKL* iii.25–26) and Ilipada (the viceroy of Hanigalbat; see immediately below). Enlil-kudurri-usur would ultimately recover the whole of Assyria.

[15] Poebel (1943, 57): '[...] "king of kings," a title quite rare in that period, found before him [i.e. Ashur-nadin-apli] to date only in the inscription of Tukulti-Ninurta I [i.e. RIMA 1 A.0.78.16].' See RIMA 1 A.0.78.13, l. 4; A.0.78.16, l. 5; A.0.78.39, l. 3.

[16] Poebel (1943, 56–57); James (1991, 302). Hagens (2005, 39 [no. 2]) argues that the title 'king of kings' points to a rival in another capital.

[17] Mahieu (2021b, 125).

[18] Grayson (2001, 524); Newgrosh (2007, 196). Similar, long Middle Assyrian genealogies exist for Ashur-uballit I (who traces his ancestry back to Puzur-Ashur III: RIMA 1 A.0.73.2, ll. 1–12; A.0.73.4, obv. 1–11; A.0.73.5, ll. 1–12) and Shalmaneser I (back to Ashur-uballit I: RIMA 1 A.0.77.8, ll. 1–6).

[19] Newgrosh (2007, 239). Cf. Quintana (1996, 93): 'L'appel aux dieux, on le sait, est le refuge des rois usurpateurs du pouvoir'; Potts (1999, 206): 'The invocation of deities who have granted the writer of a royal inscription his kingship has a long history in the ancient Near East and usually implies that the writer was not descended immediately from his predecessor on the throne (see Quintana 1996). In other words, divine intervention is invoked to legitimize a *coup d'état.*'

[20] Weidner (1959, 51 [no. 49]); Grayson (1972, vol. 1, 140 [no. 914]).

[21] Menzel (1981, vol. 1, 249); Sallaberger and Huber Vulliet (2005, 627).

[22] von Weiher (1973, 106–107).

[23] Harrak (1987, 199–201). Shalmaneser I's predecessor, Adad-nirari I, had reinstalled Shattuara, the king of Hanigalbat, as a vassal king upon his conquest of Hanigalbat: Harrak (1987, 205).

[24] The name is rendered as either Ilipada (e.g. Cancik-Kirschbaum 1999, 216–217) or Ilihadda (e.g. Glassner 2004, 143). Ilipada was eponym during the reign of Tukulti-Ninurta I. For a survey of scholars' opinions on the position of Ilipada's eponymate, see Mahieu (2020b, 258–259).

'grand vizier' Qibi-Ashur.[25] In a letter of the Kassite king Adad-shuma-usur (K. 3045), Ilipada and Ashur-nirari III are called 'kings of Assyria'.[26] This testifies to the existence of two lines of Assyrian rulers after Tukulti-Ninurta I's death: the house of Ili-pada and the house of Ashur-nirari III.[27]

The arrival of Ninurta-apil-Ekur can be set in that context. Ninurta-apil-Ekur was a son of Ilipada. After the death of Tukulti-Ninurta I, Ilipada continued reigning at Hanigalbat with the title king of Hanigalbat, while his son Ninurta-apil-Ekur took the title king of Ashur. To sustain Ninurta-apil-Ekur's right to the Assyrian throne, Ninurta-apil-Ekur is given the filiation 'Ninurta-apil-Ekur, son (DUMU) of Ilipada, descendant (*lìb-lìb-bi*) of Eriba-Adad' in the *AKL* (Khors. iii.27–28).[28] Ninurta-apil-Ekur did not descend from Tukulti-Ninurta I, but he could make a claim to the Assyrian throne via his descent from Adad-nirari I (iii.17–18, the great-great-grandson of Eriba-Adad I, iii.11–12), who was the father of both Shalmaneser I (iii.19) and Ibashi-ili (Ninurta-apil-Ekur's ancestor).[29]

The numerous decrees issued by Ninurta-apil-Ekur might reflect his difficulty in establishing power.[30] One decree testifies to tension with a son of Tukulti-Ninurta I.[31] The contemporaneity of this son with Ninurta-apil-Ekur shows that Ninurta-apil-Ekur reigned in parallel to Tukulti-Ninurta's sons.[32] The parallel reign apparently began immediately after Tukulti-Ninurta I's death. The *AKL* might indicate the contemporaneity of Ashur-nadin-apli (Tukulti-Ninurta I's son and successor) and Ninurta-apil-Ekur in assigning the same variant of three years to both rulers: Ashur-nadin-apli reigned either four years (Nass. *AKL*) or three years (Khors. and SDAS *AKL*; see *Figure 3* n. a), and Ninurta-apil-Ekur reigned either 13 years (Nass. *AKL*) or three years (Khors. and SDAS *AKL*; see *Figure 3* n. b). Since Ninurta-apil-Ekur has more than three eponyms attested, he reigned 13 years rather than three years.[33] The variant of three years for both Ashur-nadin-apli and Ninurta-apil-Ekur might represent the parallel. Both kings also receive similar entries in the *AKL* (trans. Glassner):

- Khors. *AKL* iii.21–22: 'During the lifetime of Tukultī-Ninurta, Aššur-nādin-apli, his son, took the throne. He reigned 3 years.'
- Khors. *AKL* iii.27–30: 'Ninurta-apil-Ekur, son of Ili-ḫadda, descendant of Erība-Adad, w[ent] to Karduniaš. He went up from Karduniaš (and) took the throne. He reigned 3 years.'

The *AKL* records regular enthronements by the expression 'PN reigned' (LUGAL^{ta} DÙ^{uš} or MAN^{ta} DÙ^{uš}). The expression 'took the throne' marks irregular accessions. In the other cases of usurpation of the Assyrian throne (the accessions of Shamshi-Adad I, Ashur-rabi I, Enlil-nasir II and Shamshi-Adad IV), the expression 'he drove PN from the throne' (GIŠGU.ZA *lu-šat-bi/ú-šat-bi*) precedes the expression 'he took the throne' (GIŠGU.ZA *iṣ-bat*, Khors. *AKL* i.47–ii.1; ii.45–46; iv.3–4; cf. iii.1).[34] The exceptional, identical wording for Ashur-nadin-apli and Ninurta-apil-Ekur – without 'he drove PN from the throne' – suggests that both kings acquired power in the same context. Ninurta-apil-Ekur did not depose someone. He rather founded a parallel line when seizing the throne.[35] Ninurta-apil-Ekur's sojourn in Karduniash might have influenced this policy.[36]

[25] Freydank (1991, 59–60); Cancik-Kirschbaum (1999, 219–221). For the succession of the grand viziers, see Bloch (2010, 3–9).

[26] Grayson (1972, vol. 1, 137–138 [no. 890]).

[27] Fales (2014, 228–230); Younger (2017, 197).

[28] RIMA 1 A.0.82.1, l. 3, renders the name of Ninurta-apil-Ekur's father as ^{m}GIŠ-*pád-du*, which is likely a miswriting for ^{m}DINGIR-*pád-du*: Grayson (1987, 303); Bloch (2012, 158 n. 22).

[29] For a family tree from Adad-nirari I until Ninurta-apil-Ekur, see Freydank (1991, 60); Cancik-Kirschbaum (1996, 22); Fales (2014, 227).

[30] Weidner (1954–1956, 278); Grayson (1972, vol. 1, 139 [no. 905]): 'The exceptionally large number (nine) of palace decrees (see §§304–306) for this king are an indication, as Weidner has pointed out, of the need for strict measures by this member of an obscure branch of the Assyrian royal line.'

[31] Weidner (1954–1956, 283 [no. 17, ll. 78–79]): '[*Gesetzt, eine Frau*] des Palastes hat [...] verflucht *oder ein Sohn* des Tukulti-Ninurta [... *das*] königliche [*Ha*]*us des Bet*[*tes* ...] des Schemels [...], *der* (sich) unter ihr (befindet), verflucht sie in böser Absicht [...] zum *na-ša-en-ni*, so soll man der Frau des [*Palastes* die Na]se durchbohren, [*30 Stockschläge*] soll man ihr versetzen' (italics by Weidner). Cf. Grayson (1972, vol. 1, 139–140 [no. 905]): 'The decree is directed "to his palace" and, if Weidner's interpretation is correct, provides for a "*son* of Tukulti-Ninurta" instigating palace women to pronounce forbidden curses. Also there seems to be provision for curses against royal furniture (bed and tool are mentioned). The guilty woman will have her nose pierced and be beaten with rods' (italics by Grayson).

[32] Hagens (2005, 40 [no. 6]) considers the relevant decree to be proof of the contemporaneity of Enlil-kudurri-usur and Ninurta-apil-Ekur.

[33] Cancik-Kirschbaum (1999, 215–217); Pruzsinszky (2002–2005, 25); Freydank (2016, 31). The 60 years from Ashur-dan I until (year 4 or 6 of) Tiglath-pileser I in an inscription of Tiglath-pileser I (RIMA 2 A.0.87.1, vii.66–70) imply that Ashur-dan I ruled 36 years rather than 46 years (see Mahieu 2021a, 74).

[34] In the case of Enlil-nasir II (Khors. *AKL* iii.1), only the expression 'he [drove] his brother from the throne' is given, not 'he took the throne'.

[35] For Ashur-nadin-apli's accession, the circumstances are expressed by the phrase 'during the lifetime of Tukultī-Ninurta' (trans. Glassner). SDAS *AKL* iii.11 reads *a-da-ri*, while Khors. *AKL* iii.21 has *da-a-ri* (cf. Nass. *AKL* iii.29: *da-a-*^{r}*ri*$^{?}$); neither reading is clear (Gelb 1954, 211). Gelb (1954, 227) translates as 'While Tukulti-Ninurta (I) was alive?'. The expression apparently indicates that the takeover took place when Tukulti-Ninurta I was still alive and that the latter was killed: Fales (2014, 228 n. 140). See also §2.3.

[36] Ninurta-apil-Ekur's connection with Babylonia is clear

Babylonia might have been ruled by two lines of Kassite rulers at that time,[37] and Ninurta-apil-Ekur would have brought this Kassite model of parallel reign to Assyria.

2.3. Line 1b: A new line inaugurated by Ashur-rabi II

In addition to the line of Ninurta-apil-Ekur, a further line seems to have emerged in the context of Tukulti-Ninurta I's death, the line of Ashur-rabi II. Beginning with Ashur-rabi II, no king affiliates himself with a king preceding Ashur-rabi II (see §1.2). This shows that Ashur-rabi II might have inaugurated a new line.

The *AKL* calls Ashur-rabi II the son of Ashurnasirpal (Khors. *AKL* iv.9). This Ashurnasirpal is generally identified with Ashurnasirpal I (iv.5), who occurs three entries before Ashur-rabi II in the *AKL*. However, had Ashur-rabi II been the son of Ashurnasirpal I, he would have outlived his brother Shalmaneser II by 47 years, which seems unlikely.[38] Ashur-rabi II's father might rather be another person called Ashurnasirpal, possibly the prince Ashurnasirpal who was a son of Tukulti-Ninurta I and who killed Tukulti-Ninurta I.[39] The existence of this extra Ashurnasirpal is indicated by the stela Ass. 15711 (erected at Assur in the row of stelae with the names of Assyrian kings, not in the row of stelae with Assyrian officials).[40] Ass. 15711 mentions Ashurnasirpal (l. 1), *a gap* (ll. 2–3), the son of [...]-MAŠ (l. 4), the son of Adad-nirari (l. 6). The filiation can be reconstructed as:

- either Ashurnasirpal II, the son of [Tukulti]-Ninurta II (*tukúl-ti*-^{d}MAŠ), the son of Adad-nirari II;[41]
- or Ashurnasirpal, [the son of Tukulti-Ninurta I], the son of [Shalman]eser I (^{d}*Šùl-ma-nu*-MAŠ), the son of Adad-nirari I.[42]

The first option faces the inconvenience that Ashurnasirpal II would possess two stelae at Assur (Ass. 15711 and Ass. 15714).[43] The second option would confirm the existence of a son of Tukulti-Ninurta I called Ashurnasirpal. His existence is further reflected in the confusion that the *AKL* manifests for the filiations of Ashur-nadin-apli and Ashur-nirari III:

- Nass. *AKL* iii.30–33 mentions Ashur-nadin-apli as the son and successor of Tukulti-Ninurta I, and subsequently Ashur-nirari III as the son and successor of Ashur-nadin-apli.
- Khors. *AKL* iii.21–24 has Ashur-nadin-apli as the son and successor of Tukulti-Ninurta I, and Ashur-nirari III as the son of Ashurnasirpal and the successor of Ashur-nadin-apli.
- SDAS *AKL* iii.11–13 has Ashurnasirpal as the son and successor of Tukulti-Ninurta I, and Ashur-nirari III as the son and successor of Ashurnasirpal.

Given that Ashur-nadin-apli, the son of Tukulti-Ninurta I, is attested as king in inscriptions (RIMA 1 A.0.79.1–2), his kingship cannot be questioned. The succession of Tukulti-Ninurta I by Ashur-nadin-apli is assured by Khors. *AKL* iii.21–22 as well (see n. 35 above). Nass. and Khors. are thus correct in calling Ashur-nadin-apli the successor of Tukulti-Ninurta I, and SDAS errs in this matter. With regard to the fatherhood of Ashur-nirari III, Khors. and SDAS might err in calling Ashur-nirari III's father Ashurnasirpal (contrary to the reading Ashur-nadin-apli in Nass.): since Ashur-nadin-apli (the successor of Tukulti-Ninurta I) put his brother Ashurnasirpal (the murderer of Tukulti-Ninurta I) aside in the succession, Ashur-nadin-apli would unlikely have been succeeded by a son of Ashurnasirpal. Hence, Nass. seems to identify Ashur-nirari III's father correctly with Ashur-nadin-apli. Though Khors. and SDAS apparently err in the identification of Ashur-nirari III's father, the two recensions still have the merit of showing that a

from his name, 'Ninurta is the heir of the Ekur', the temple at Nippur (Cancik-Kirschbaum 1999, 222). Another son of Ilipada is called Mardukiya (Andrae 1913, 85 [no. 129]; Cancik-Kirschbaum 1999, 219), after the Babylonian god Marduk.

[37] Mahieu (2021b, 99–125).

[38] Cf. Hagens (2002, 64): 'Suspicion about the long duration of Ashur-rabi's reign arises from the information in the AKL that he did not directly succeed to the throne after the death of his father, but only after his brother Shalmaneser II (12 years) and his nephew Ashur-nirari IV (6 years) had reigned. The likelihood of a late-born son succeeding his father, brother and nephew, after an interval of 18 years, and then reigning for a further 41 years (a total of 59 very stressful years) must surely be quite low.'

[39] Newgrosh (1999, 98, 103; 2007, 205). See *Chronicle P* iv.10–11: 'Aššur-nāṣir-apli, his son, and the Assyrian officials revolted against Tukultī-Ninurta, who for evil had laid [hands] on Babylon, deposed him from his throne, locked him in a room in Kār-Tukultī-Ninurta, and put him to death' (trans. Glassner).

[40] Andrae (1913, vi).

[41] Weidner (1959, 42 [note to ll. 10–11]).

[42] Andrae (1913, 18–20 [no. 10]).

[43] Ass. 15714 belongs to Ashurnasirpal II: Andrae (1913, 12–13 [no. 6]). Newgrosh (2007, 187): 'This [i.e. Ass. 15711] cannot be the stele of Ashur-nasirpal II, son of Tukulti-Ninurta II, because he already has one stele and would have held the office of eponym only the once. Nor can it belong to Ashur-nasirpal I, the possessor of a rather grand stele, because he was a son of Shamshi-Adad IV. Orthographically, *Stelenreihen* number 10 is of a style rather later than the period of Tukulti-Ninurta I, indicative of either a later ruler (and this we can exclude) or a later reinstatement of its owner'; cf. Grayson (1972, vol. 1, 135 n. 261): 'Weidner [1959, 42 (note to ll. 10–11)] has argued that orthographically this [i.e. Ass. 15711] must be later than Tukulti-Ninurta I's time but this leaves two steles for Ashur-nasir-apli II, despite the fact that Ashur-nasir-apli II held the office of eponym only once.'

royal person called Ashurnasirpal lived at the time of Tukulti-Ninurta I's death.[44]

That Ashurnasirpal should be distinguished from Ashur-nadin-apli is also clear from the latter's inscriptions. Ashur-nadin-apli calls Tukulti-Ninurta I 'appointee of the god Enlil, vice-regent of the god Aššur' and 'king of the universe' (RIMA 1 A.0.79.1, ll. 12–13; A.0.79.2, l. 3). Had Ashur-nadin-apli killed his father Tukulti-Ninurta I, he would not have mentioned Tukulti-Ninurta I with such honorific titles. Moreover, the *AKL* (Khors. iii.21–22) exceptionally does not say that Ashur-nadin-apli 'drove Tukulti-Ninurta I from the throne', though 'he took the throne' (see n. 35 above). That Ashur-nadin-apli did not depose his father is explained by the fact that Tukulti-Ninurta I was deposed by Ashurnasirpal.

In conclusion, the descent of Tukulti-Ninurta I can be summarised as follows:

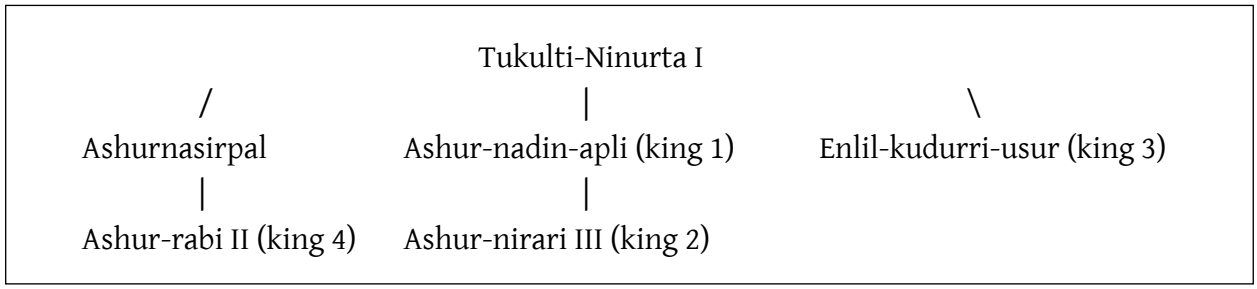

Figure 1: The descent of Tukulti-Ninurta I.

As can be seen, the lineage inaugurated by Ashur-rabi II was not a new dynastic line since Ashur-rabi II descended from Tukulti-Ninurta I.

2.4. The context of the inauguration of Line 1b by Ashur-rabi II

Though Ashurnasirpal headed the revolt that led to Tukulti-Ninurta's murder, he did not become king and was put aside.[45] Ashur-nadin-apli and his son Ashur-nirari III ascended the throne in Line 1a. Ashur-nirari III, in turn, was succeeded by his uncle Enlil-kudurri-usur, a further son of Tukulti-Ninurta I. Ashur-rabi II, the son of Ashurnasirpal, became king next. He might have wanted to avenge the fact that his father had not become king. To take the throne, he apparently sided with Line 2. Tension existed between Line 1a (the lineage of Ashur-nadin-apli) and Line 2 (the lineage of Ninurta-apil-Ekur). The *Synchronistic History* (ii.B3′–8′) describes an encounter between Enlil-kudurri-usur (the third and last ruler of Line 1a) and Ninurta-apil-Ekur (the first ruler of Line 2): 'King Enlil-kudurrī-uṣur of Assyria (and) [King Adad-šuma-uṣur (?) of Karduniaš fou]ght. As Enlil-kudurrī-uṣur and Adad-[šuma-uṣur (.?.)] were engaged in conflict, Ninurta-apil-E[kur ...] returned to his country. [He assembled] his numer[ous] troops and marched on Libbi-āli (= Aššur) in order to take it. [But (?) ...] arrived unexpectedly. He retreated and [went back to his country]' (trans. Glassner).[46]

The circumstances of the extermination of Line 1a are enlightened by the letter K. 212+4448 [+] Sm 2116+BM 104727 (with BM 55498+55499 as a – better preserved – duplicate of K. 212+),[47] written by an unnamed king. The letter is generally situated in the context of the struggle between Ninurta-tukulti-Ashur and Mutakkil-Nusku for the succession of Ashur-dan I (Khors. *AKL*

[44] On the basis of the *AKL* recensions, Weidner (1959, 42 [note to ll. 10–11]) argues that Tukulti-Ninurta I had a son called Ashurnasirpal and a son called Ashur-nadin-apli. Yamada (1998) considers the reading Ashurnasirpal in *Chronicle P*, Khors. and SDAS to be a writing error for Ashur-nadin-apli. Pedersén (1999, 371–373; cf. Chen 2020, 58–59) argues that, in addition to the standard reading PAP = *naṣāru*, PAP can also denote *nadānu*. All the versions would intend Ashur-nadin-apli.

[45] Cf. Wiseman (1975, 449–450): 'It may be that his [i.e. Ashurnasirpal's] name was deliberately erased from a royal stela (Andrae 1913, 18–20 [no. 10]) after he had been acclaimed by only a few followers who were soon crushed, or, less likely, that his name is a scribal error for another son, Ashur-nādin-apli, who may have been the murderer.'

[46] Cf. the earlier translations by Tadmor (1958a, 131): 'Enlil-kudur-uṣur, king of Assyria and A[dad-šum-uṣur, king of Babylonia] battled. While Enlil-kudur-uṣur (and) Adad-šum-uṣur [...] were engaged in battle and Ninurta-apil-E[kur the son of Ili-iḫadda, a descendant of Erība-Adad], returned to his land (Aššur). He [summoned] his big army and came to Aššur in order to conquer it. [A fire] broke out in the camp of [Adad-šum-uṣur]; he turned and [went back to his country],' and by Grayson (1975, 161–162): 'Enlil-kudur-usur, king of Assyria, (and) [*Adad-shuma-usur* king of Karduniash,] did battle [with one another]. Enlil-kudur-usur (and) *Ada*[*d-shuma-usur* ...] were engaged in battle and Ninurta-apil-e[kur ...] went home. [*He mustered*] his num[erous] troops [and] mar[ched] to conquer Libbi-ali (Ashur). [...] in his fortress he/it struck/fell. He turned and [*went home*].'

[47] Facsimile, transliteration and translation by Llop and George (2001–2002, 2–8, 20–23).

iii.32–36).[48] In the anonymous letter, a Babylonian king tries to reinstate Ninurta-tukulti-Ashur in Assyria.[49]

In this context, he refers back to a precedent: the Assyrian officials (GAL^{meš} *šá* ^{kur}*aš-šur*) had delivered the Assyrian king Enlil-kudurri-usur to the Kassite king Adad-shuma-usur and his Babylonian officials (GAL^{meš} *šá* ^{kur}URI^{ki}, Sm 2116+BM 104727 rev. 6′–7′).[50] This precedent can be compared to a similar case, the situation at the end of Tukulti-Ninurta I's reign described in *Chronicle P*: after the Babylonian officials (^{lú}GAL^{meš} *šá* ^{kur}URI^{ki} *šá* ^{kur}*kar-an-*^{d}*dun-ía-àš*) had rebelled and had installed Adad-shuma-usur on the throne, Ashurnasirpal and the Assyrian officals (^{lú}GAL^{meš} *šá* ^{kur}*aš-šur*^{ki}) revolted against Tukulti-Ninurta I and killed him (*Chronicle P* iv.8–11). In both cases, the Assyrian officials, the Babylonian officials and Adad-shuma-usur are involved.[51] Adad-shuma-usur and the officials apparently supported Ashurnasirpal against Tukulti-Ninurta I and later supported Ashurnasirpal's son Ashur-rabi II against Tukulti-Ninurta I's son Enlil-kudurri-usur. This reconstruction fits with the present proposal that Ashur-rabi II avenged the elimination of his father Ashurnasirpal. To that purpose, he sided with both Line 2 and Babylonia.

Line 1b and Line 2 presumably joined forces to expel Line 1a and henceforth reigned in parallel. Given that Ashur-rabi II became king 4 + 6 + 5 = 15 years after Tukulti-Ninurta I's death and that Ninurta-apil-Ekur's reign ended 13 years after Tukulti-Ninurta I's death (see *Figure 3*), Ashur-rabi II became king early in the reign of Ninurta-apil-Ekur's successor, Ashur-dan I. The good understanding between Ashur-rabi II (in Line 1b) and Ashur-dan I (in Line 2) is endorsed by the fact that Ashur-rabi II and Ashur-dan I seem to have shared eponyms.[52] *KAV* 21–22 (a part of the eponym list A7) fragmentarily preserves the eponyms for Ashur-rabi II, and these can be linked to Ashur-dan I's eponyms (no eponym list is preserved for Ashur-dan I):[53]

[48] Forrer (1932, 278 [§41]), Weidner (1935–1936, 6), Grayson (2001), Llop and George (2001–2002, 8, 10) and Bányai (2015, 9) propose Mutakkil-Nusku as the addressee of the letter. An indication for this is that BM 55498+55499 obv. 17′ mentions 'Aššur-dan, your father'.

[49] For chronological reasons (i.e. the author of the letter should rule at about the time of Ninurta-tukulti-Ashur), the writer of the letter has been identified with Itti-Marduk-balatu, Ninurta-nadin-shumi, or Nebuchadnezzar I, three successive kings in Isin II (Brinkman 1968, 101–104). Bányai (2015, 9) opts for Itti-Marduk-balatu.

[50] Cf. Bányai (2015, 9–10).

[51] The Babylonian implication in the murder of Tukulti-Ninurta I is also suggested by the fact that Ninurta-apil-Ekur, the successor of Tukulti-Ninurta I in Line 2, went to and came back from Babylonia before taking the Assyrian throne (Khors. *AKL* iii.27–29); and see n. 36 above.

[52] The eponyms of the kings preceding Ashur-rabi II in Line 1 (the kings Ashur-nadin-apli, Ashur-nirari III and Enlil-kudurri-usur) differ from those of their contemporary Ninurta-apil-Ekur (the predecessor of Ashur-dan I in Line 2); see Freydank (1991, 194–195; 2016, 20–23, 31); Bloch (2012, 408–410). Corresponding eponyms are neither found for the successors of Ashur-rabi II and Ashur-dan I, for whom two important sources are preserved:

- Eponym list A7 = Ungnad's list C^{c}: a fragmentary eponym list from the last 11 years of Tiglath-pileser I until 659 BC (Ungnad 1938, 419–429, 435–436; Millard 1994, 25–53).
- The consecutive list of eponyms from Adad-nirari II until 649 BC (Ungnad 1938, 418–428; Millard 1994, 23–62).

In these two lists, there are no eponym correlations between Line 1b (Ashur-dan II and his successors) and their contemporaries in Line 2 (Tiglath-pileser I and his successors). The sharing of eponyms is apparently restricted to the period of the foundation of Line 1b.

[53] Newgrosh (2007, 545) mentions the identification with Ashur-mushezib, Ittabshi-den-Ashur, Ashur-shezibanni, Ashur-bel-lite, Adad-uballit, Adad-mushabshi, Atamar-den-Ashur, Remanni-Ishtar (or [...]-Ishtar) and Sin-sheya. For [...]u he notes 'common ending', and for [...]ani 'a number of possible matches'. Newgrosh's proposal to identify Nabu-dan (in *KAV* 21–22) with Ilipada must be rejected since Ilipada is an eponym during the reign of Tukulti-Ninurta I (see n. 24 above).

KAV 21–22[a]	Eponyms of Ashur-dan I[b]	Possible eponyms of Ashur-dan I
Ashur-rabi II		
Ashur-mushe[zib?]		Ashur-mushezib[c]
Ittab[shi...]		Ittabshi-den-Ashur[d]
Ashur-shezib[anni?][e]	Ashur-shezibanni	
Nabu-dan		
Ashur-ba[(l?)lit?][f]	Ashur-bel-lite	
Mukin-ahi[g]		
Qu[...]		
[...]		
[...l]it?		Adad-uballit[h]
[...]shi	Adad-mushabshi	
[...]u		
[...]-Ishtar		Remanni-Ishtar[i] or [...]-Ishtar[j]
[...]-Ashur	Atamar-den-Ashur	
[...]ani		*a number of possible matches*
[...]ya	Sin-sheya	

Figure 2: The eponyms of Ashur-rabi II and Ashur-dan I.

[a] Facsimile in Schroeder (1920, 28–29); transliteration in Ungnad (1938, 436 [iv.23–38]); translation in Grayson (1976, vol. 2, 71 [no. 338]).

[b] Saporetti (1979, 133–146); Freydank (1991, 195; 2016, 50–51).

[c] Saporetti (1979, 34, 77) dates Ashur-mushezib, the son of Kidin-Enlil, to the reign of Eriba-Adad I, and a later Ashur-mushezib to the time of Shalmaneser I. Freydank (1991, 190) proposes the reigns of Adad-nirari I or Shalmaneser I for Ashur-mushezib, the son of Kidin-Enlil, and an unknown reign for the second eponym called Ashur-mushezib. Bloch (2012, 157, 408 n. 6) and Wiggermann (see Freydank 2016, 22) both propose the reign of Tukulti-Ninurta I for Ashur-mushezib (without giving a filiation).

[d] Saporetti (1979, 103–105) situates Ittabshi-den-Ashur at the time of Shalmaneser I or Tukulti-Ninurta I. Both Freydank (1991, 191) and Bloch (2012, 407) propose Shalmaneser I.

[e] Ashur-shezib[anni?] in Ungnad; Ashur-KAR-[...] in Grayson.

[f] Ashur-ba[(l?)lit?] in Ungnad; Ashur-[...] in Grayson.

[g] Mukin-aḫi in Ungnad; DU[...] in Grayson.

[h] Saporetti (1979, 114–115) situates Adad-uballit at the time of Tukulti-Ninurta I; Freydank (1991, 189) at the time of Ninurta-apil-Ekur?; and Freydank (2016, 128) at the time of Tiglath-pileser I.

[i] Both Saporetti (1979, 50) and Freydank (1991, 192) situate Remanni-Ishtar at the time of Ashur-uballit I.

[j] Freydank (1991, 192) situates [...]-Ishtar in the (conventional) 12th/11th century (i.e. the period from Ashur-nadin-apli until Ashur-nirari IV). Freydank (2016, 178) does not give a time indication.

If the correspondences are correct, this would confirm both the contemporaneity and the good understanding between Ashur-rabi II and Ashur-dan I.

The agreement between both lines is further reflected in the fact that Ashur-rabi II worked on the *Bīt-naṯḫi* in the complex of Ishtar at Nineveh (see n. 114 below). The *Bīt-naṯḫi* is probably the place of the divine marriage between Ishtar of Nineveh and the god Ashur.[54] Ashur-rabi II might have contributed to the marriage building in order to express, symbolically, the celebration of a pact between Line 1b and Line 2.

Line 1b and Line 2 divided the territory among themselves: Line 2 reigned in the Assyrian mainland (see Section 4), while Line 1b can be situated in Hanigalbat. The inscription of Bel-erish (RIMA 2 A.0.96.2001), who was vice-regent of Shadikanni (on the banks of the Ḫabur) and vassal of Ashur-rabi II and Ashur-resh-ishi II, evidences that Ashur-rabi II (and his

[54] Vieyra (1975).

son Ashur-resh-ishi II) reigned in Hanigalbat.[55] The changing circumstances in Hanigalbat at that time are reflected in an inscription of Tiglath-pileser I as well. The Mushki invaded northern Mesopotamia 50 years before the accession year of Tiglath-pileser I.[56] Ashur-rabi II ascended the throne 4 + 6 + 5 = 15 years after the death of Tukulti-Ninurta I, while Tiglath-pileser I ascended the throne 13 + 36 + 18 = 67 years after the death of Tukulti-Ninurta I (see *Figure 3*). Thus, Ashur-rabi II became king 67 - 15 = 52 years before Tiglath-pileser I. The 52 years might well correspond to the 50 years in the inscription. The Mushki would have invaded when Line 1b emerged in the region.

Following the defeat of Enlil-kudurri-usur, Ashur-rabi II founded a new line of rulers, Line 1b, in the region of Hanigalbat.[57] Contemporary material might confirm that an Assyrian kingdom was located in that region. Gauthier (2016, 182–198) has convincingly argued that Middle Assyrian administrative documents situate the 'Province of Assur' – with apparently also a city named 'Assur' – in the western Habur region, i.e. in the region of Hanigalbat, and not in the vicinity of the capital city 'Assur', on the Tigris. The fact that this province was named 'Assur' supports the present hypothesis that an Assyrian centre of power was located in that region and that these kings could call themselves 'king of Assur'.[58]

2.5. The end and extension of the parallel lines

The beginning of the parallel lines has been set at the time of Tukulti-Ninurta I's death, but we should still fix how long the parallel lines lasted. Synchronisms with Babylonia, the Levant and Egypt show that, beginning from the 8th century, the Neo-Assyrian kings certainly reigned in one, successive line, so that the parallel lines must have come to an end before the 8th century. As an indication for which line discontinued and which line continued, one can note that there is no break in the Assyrian lineage after Ashur-rabi II (see §1.2): the Neo-Assyrian kings of the 8th century continue Line 1b. This implies that Line 2 is the line that came to an end.

Since the break between Line 2 and Line 1b is situated at the entry for Ashur-rabi II (Khors. *AKL* iv.9, the first king of Line 1b), the last kings of Line 2 are the kings mentioned immediately before Ashur-rabi II in the *AKL*. The last two of them, Shalmaneser II (iv.6–7) and Ashur-nirari IV (iv.8), are characterised by a lack of material. The eponym list A7 records the last year of Shalmaneser II as a mere *ša arki* ('the one after') year,[59] and it gives only one personal eponym for the six years of Ashur-nirari IV: Ashur-nirari IV is the first eponym and is then followed by five *ša arki* years.[60] Shalmaneser II and Ashur-nirari IV did not undertake any building activity (see §4.1), and Ashur-nirari IV does not have any record either.[61] The poor material reflects the end of the power of Line 2.

In identifying Line 2 as lasting from Ninurta-apil-Ekur until Ashur-nirari IV and in beginning Ninurta-apil-Ekur's reign at the time of Tukulti-Ninurta I's death, Line 1ab and Line 2 can be chronologically related as follows:

[55] Furlong (2007, 32–33, 68).

[56] RIMA 2 A.0.87.1, i.62–70: 'In my accession year: 20,000 Mušku with their five kings, who had held for 50 years the lands Alzu and Purulumzu – bearers of tribute and tithe to the god Aššur, my lord – (the Mušku), whom no king had ever repelled in battle, being confident of their strength they came down (and) captured the land Katmuḫu.'

[57] Furlong (2007, 63-64) likewise argues that the line of Ashur-rabi II continues the kingdom of Hanigalbat, though he situates Ashur-rabi II in the days of Tiglath-pileser I. Hagens (2002, 67) similarly situates Ashur-rabi II in the Ḫabur region, though in the days of Shalmaneser II.

[58] An additional kingdom (though not called 'of Assur') ruled in Hanigalbat in Middle Assyrian times: the kings 'of Mari', governing from Ṭabetu. On this local dynasty, see Shibata 2012.

[59] Ungnad (1938, 436 [iv.16]); Grayson (1976, vol. 2, 69 [no. 332]).

[60] Ungnad (1938, 436 [iv.18–21]); Grayson (1976, vol. 2, 70 [no. 335]).

[61] Cf. Grayson (1991, 125).

Line 1a	*Line 2*
Ashur-nadin-apli (4 or 3 years)[a]	Ninurta-apil-Ekur (13 or 3 years)[b]
Ashur-nirari III (6 years, iii.23–24)	
Enlil-kudurri-usur (5 years, iii.25–26)	
Line 1b	
Ashur-rabi II (41 years, iv.9)	Ashur-dan I (36 or 46 years)[c]
	Ninurta-tukulti-Ashur (*ṭuppi*,[d] iii.32–33)
	Mutakkil-Nusku (*ṭuppi*, iii.34–36)
Ashur-resh-ishi II (5 years, iv.10–11)	Ashur-resh-ishi I (18 years, iii.37–38)
Tiglath-pileser II (32 years, iv.12–13)[e]	Tiglath-pileser I (39 years, iii.39–40)
Ashur-dan II (23 years, iv.14–15)	Asharid-apil-Ekur (2 years, iii.41–42)
	Ashur-bel-kala (18 years, iii.43–44)
Adad-nirari II (21 years, iv.16–17)	Eriba-Adad II (2 years, iii.45)
	Shamshi-Adad IV (4 years, iv.1–4)
Tukulti-Ninurta II (7 years, iv.18)[f]	Ashurnasirpal I (19 years, iv.5)
Ashurnasirpal II (25 years, iv.19–20)[g]	Shalmaneser II (12 years, iv.6–7)[h]
	Ashur-nirari IV (6 years, iv.8)
Total: 169 years	Total: 169 years

Figure 3: The chronological relationship between Line 1ab and Line 2.

[a] Nass. *AKL* iii.29–31 records four years for Ashur-nadin-apli, while Khors. *AKL* iii.21–22 and SDAS *AKL* iii.11–12 have three years.

[b] Nass. *AKL* iii.36–40 records 13 years for Ninurta-apil-Ekur, while Khors. *AKL* iii.27–30 and SDAS *AKL* iii.15–17 have three years. The variant of three years relates to the 46 (instead of 36) years for Ninurta-apil-Ekur's successor Ashur-dan I: the 13 + 36 years for Ninurta-apil-Ekur and Ashur-dan I in Nass. (*AKL* iii.36–42) correspond in duration to the 3 + 46 years in Khors. (*AKL* iii.27–31) and SDAS (*AKL* iii.15–18).

[c] Nass. *AKL* iii.41–42 records 36 years for Ashur-dan I (Nassouhi 1927, 7; cf. Grayson 1980, 111: 26+[x] years), while Khors. *AKL* iii.31 and SDAS *AKL* iii.18 have 46 years.

[d] The word *ṭuppi* in the *AKL* has often been interpreted as denoting a period of less than a year (following *AHw* 3, 1394 [*ṭuppi* no. 3]: 'in nA Königslisten v Kurzregierungen unter 1 Jahr'). Freydank (2007, 226) proposes that *ṭuppi* expresses 'besides, furthermore, in addition, also'. Janssen (2007, 104) proposes that *ṭuppi* represents the period (i.e. the months and/or days) that a king rules during his last regnal year. Gasche *et al.* (1998, 53–54) and Bányai (2015, 10 n. 7) suggest that it regards the portion that concludes the last regnal year, that is, the months and/or days that follow a king's death (cf. *CAD* 19, 129 [no. d]). Baker (2010) argues that *ṭuppi* denotes a one-year period. In the Assyrian *Distanzangaben*, the *ṭuppi* period was counted as either zero or one years (Mahieu 2021a, 69–70). The present reconstruction of the two lines implies that, in the actual chronology, the *ṭuppi* periods of Ninurta-tukulti-Ashur and Mutakkil-Nusku should be counted as zero years, in order to reach an exact parallel of 169 years.

[e] The eponym list A7 (= Ungnad's list C^{c}, Ungnad 1938, 436 [v.38]) assigns 33 years (instead of 32 years) to Tiglath-pileser II.

[f] The eponym list A2 (= Ungnad's list C^{a}2, Ungnad 1938, 418 [i.5–10]) records six eponyms for Tukulti-Ninurta II (instead of seven years, Khors. *AKL* iv.18). The list of six eponyms omits the eponym Na'id-ilu between the eponyms Ilu-milki and Yari (Poebel 1943, 73–74; Brinkman 1968, 183 n. 1124). List A8 records the seven eponyms (Millard 1994, 24 and the note to the year 885). Lists A2 and A8 are the only eponym lists that record the eponyms of Tukulti-Ninurta II (Millard 1994, 24–25).

[g] The eponym list A7 (vi.16) has been supposed to record 24 years for Ashurnasirpal II (Ungnad 1938, 421 [numbered vi.45]; Millard 1994, 27, 56 [see the year 858]), which cannot be correct (Poebel 1943, 73). The reading is, however, rather 25 years: Zawadzki (1994, 44, 52).

[h] Khors. *AKL* iv.7 preserves the number fragmentarily (Gelb 1954, 220: [1]2 years; Grayson 1980, 113: [x]+2 years). Shalmaneser II's reign is not preserved in SDAS *AKL* iv.2 and is omitted in Nass.: Ashurnasirpal I (Nass. *AKL* iv.19–20) immediately precedes Ashur-nirari IV (iv.21–22).

The two lines rule exactly the same time span, 169 years. The *AKL* first mentions the three kings of Line 1a (Khors. *AKL* iii.21–26), then Line 2 (iii.27–iv.8) and finally Line 1b (iv.9–20). Line 2 was inserted into the successive sequence of Line 1a and b:[62]

[72] Eriba-Adad I
[73] Ashur-uballit
[74] Enlil-nirari
[75] Arik-den-ili
[76] Adad-nirari I
[77] Shalmaneser I
[78] Tukulti-Ninurta I

Line 1a
[79] Ashur-nadin-apli
[80] Ashur-nirari III
[81] Enlil-kudurri-usur

Line 2
[82] Ninurta-apil-Ekur
[83] Ashur-dan I
[84] Ninurta-tukulti-Ashur
[85] Mutakkil-Nusku
[86] Ashur-resh-ishi I
[87] Tiglath-pileser I
[88] Asharid-apil-Ekur
[89] Ashur-bel-kala
[90] Eriba-Adad II
[91] Shamshi-Adad IV
[92] Ashurnasirpal I
[93] Shalmaneser II
[94] Ashur-nirari IV

Line 1b
[95] Ashur-rabi II
[96] Ashur-resh-ishi II
[97] Tiglath-pileser II
[98] Ashur-dan II
[99] Adad-nirari II
[100] Tukulti-Ninurta II
[101] Ashurnasirpal II

Figure 4: The insertion of Line 2 into Line 1ab in the AKL.

[62] The numbers between square brackets in *Figure 4* represent the count of the kings in the *AKL*.

Two arguments can be added in favour of the reconstruction. First, the *Synchronistic History* mentions Ashur-bel-kala of Line 2 (ii.A25″–37″) immediately before Adad-nirari II of Line 1b (iii.A1–21), which shows that both kings are close in time. The reign of Ashur-bel-kala began 13 + 36 + 18 + 39 + 2 = 108 years after the death of Tukulti-Ninurta I, while the reign of Adad-nirari II began 4 + 6 + 5 + 41 + 5 + 32 + 23 = 116 years after the death of Tukulti-Ninurta I. Ashur-bel-kala's reign thus began 116 - 108 = 8 years before Adad-nirari II's. This explains why Ashur-bel-kala comes immediately before Adad-nirari II in the *Synchronistic History*. Second, the nearness in time of Ashur-bel-kala and Adad-nirari II is also indicated by an underground royal tomb complex that contains six vaulted chambers (in the south-east side of the Old Palace in the city of Assur). Five of them have been excavated and three could be identified: the tombs of Ashur-bel-kala, Ashurnasirpal II and Shamshi-Adad V.[63] This suggests that Ashur-bel-kala shortly preceded Ashurnasirpal II (the grandson of Adad-nirari II and the grandfather of Shamshi-Adad V).[64]

The chronological survey found in *Figure 3* sets the end of the parallel in the days of Ashurnasirpal II of Line 1b. This time setting accords with the historical setting. The fact that Ashurnasirpal II moved the capital to Kalhu/Calah (Nimrud)[65] might be related to the disappearance of the parallel lines. Kalhu had been built by Shalmaneser I,[66] shortly before the split into two lines (only the reign of Tukulti-Ninurta I separates the reign of Shalmaneser I from the split). The capital's move to Kalhu might express a return to the situation before the split, with one line of kings. Moreover, Shalmaneser III, the son of Ashurnasirpal II, introduced a system of successive regnal years in addition to the traditional eponym count.[67] This innovation might likewise have intended to mark a new epoch.

[63] Haller (1954, 170–181).

[64] Newgrosh (2007, 611; a contribution by R. M. Porter).

[65] Ashurnasirpal II (re)founded Kalhu during the eponymy of Sha-ilima-damqa (year 5 of Ashurnasirpal II, Millard 1994, 25, 56): RIMA 2 A.0.101.1, ii.86, 131–135; cf. A.0.101.17, iii.138′; v.1–24 (with the eponymy in a lacuna). Kalhu remained the Assyrian capital until Sargon II moved Assyria's centre to Dur-Sharrukin (Postgate and Reade 1980, 321 [§36]). Sennacherib subsequently moved the capital to Nineveh (Reade 2000, 397 [§9]).

[66] RIMA 2 A.0.101.1, iii.132; A.0.101.2, l. 52; A.0.101.23, ll. 14–15; A.0.101.26, ll. 46–47; A.0.101.28, v.1; A.0.101.32, l. 7. The six inscriptions all belong to Ashurnasirpal II.

[67] Accession years and first regnal years are sporadically attested before Shalmaneser III, beginning with Tukulti-Ninurta I; see Tadmor (1958b, 27–28).

2.6. Two possible counterarguments against the reconstructed parallel lines

Notwithstanding the evidence cited above, at least two possible counterarguments can be advanced against the reconstructed parallel lines. First, since the three versions of the *AKL* (Nass., Khors. and SDAS) all contain Line 2 in its entirety, these lists were redacted after the reign of Ashur-nirari IV, the last ruler of Line 2. However, given that Tiglath-pileser II is the last king mentioned in Nass. (iv.27–28), the redaction of Nass. is often dated to the reign of Tiglath-pileser II's successor, Ashur-dan II.[68] In the present scheme, Ashur-dan II comes before Ashur-nirari IV in time, which would refute the reconstruction. Yet, the copyist of Nass. might not have redacted his list in the time of Tiglath-pileser II but rather have halted his work with Tiglath-pileser II because he noted a serious failure: he had skipped the reign of Shalmaneser II. If the redactor of Nass. did not finish the list, he (and Ashur-nirari IV) may have lived after Ashur-dan II.[69]

Second, Ashur-dan II, the son of Tiglath-pileser II, the son of Ashur-resh-ishi II, the son of Ashur-rabi II, repaired the Craftsman's Gate at Assur made by Tiglath-pileser I, the son of Ashur-resh-ishi I, the son of Mutakkil-Nusku (RIMA 2 A.0.98.3, ll. 1–7).[70] The reign of Tiglath-pileser I began 39 - 13 = 26 years before the reign of Ashur-dan II (see §3.3). It might seem unlikely that the Craftsman's Gate could have become dilapidated in such a short time span. Four observations can, however, be made:

[68] Nassouhi (1927, 1); cf. Valk (2019, 2): shortly after Tiglath-pileser II. Khors. was redacted in the second eponymy of Adad-bel-ukin (iv.37–39), that is, in 738 BC (Poebel 1942, 250; Grayson 1980, 101; Valk 2019, 2–3). SDAS was redacted shortly after 722 BC, after the reign of Shalmaneser V (Grayson 1980, 101; Valk 2019, 2).

[69] Newgrosh (2007, 190): 'Now it is often stated, sometimes quite dogmatically, that the former [i.e. Nass.] is much the older version of the AKL since its listing ends with Tiglath-pileser II, and this would be compatible with a compilation date during the reign of his successor, Ashur-dan II. But this idea is rather fanciful. Authentic Mesopotamian documents were signed off using a colophon, giving the name of the scribe and the date of his inscription [both Khors. and SDAS have a colophon: Glassner 2004, 144–145]. In the case of the Nassouhi tablet, although there is plenty of available space in the column II of the reverse side, (see Figure 1) it lacks a colophon. The safest explanation is that the Nassouhi scribe abandoned work on a part-finished tablet because he discovered so many errors (R. M. Whiting in "10th Century Assyrian Chronology", ANEpost of 3rd March 2003). When one considers that the scribe omitted an entire line relating to the kingship of Shalmaneser II see Figure 3 n. h; four entries before Tiglath-pileser II], this is highly likely.'

[70] The differing designations for Ashur-dan II and his ancestors ('strong king, king of the universe') in comparison to those of Tiglath-pileser I and his ancestors ('vice-regent of Aššur') might testify to the two lines: Ashur-dan II reigned in Line 1b, while Tiglath-pileser I in Line 2.

1) In Ashur-dan II's days, the Craftsman's Gate 'became dilapidated' (l. 9 *e-na-ḫu-ma*), and, at its end, the inscription refers to the future, when the gate 'becomes old and dilapidated' (ll. 15–16 *ú-šal-ba-ru-ma e-na-ḫu*). The absence of *ušalbaruma* in l. 9 shows that the gate was not necessarily 'old' in Ashur-dan II's days.

2) Tiglath-pileser I replaced the doors of the Craftsman's Gate (RIMA 2 A.0.89.7, v.9–11 [the Broken Obelisk]).[71] No record reports any construction activity by Tiglath-pileser I on the gate itself. If he did not reconstruct the gate, the gate might have become weakened soon after his reign.

3) A similar case of rapid reconstruction is attested for Tiglath-pileser I's father. Ashur-resh-ishi I restored the Lion Gate at Nineveh that had become dilapidated following an earthquake in the days of his immediate predecessor, Ashur-dan I (RIMA 1 A.0.86.1, ll. 8–10; A.0.86.2, ll. 3–7). Hence, dilapidation can occur quickly.

4) Last, as *Figure 6* will show, Tiglath-pileser I was the last king of Line 2 to reside in Assur, while Ashur-dan II was the first king of Line 1b to reside there. Ashur-dan II might have wanted to mark the arrival of his line at Assur by continuing the work on one of the last constructions of the preceding line there. The gate might, moreover, have suffered damage during the conflicts with the Arameans that caused the power shift from Line 2 to Line 1b in the days of Tiglath-pileser I (see §3.4).

Ashur-dan II might thus well have worked on the Craftsman's Gate shortly after Tiglath-pileser I, and no evidence rejects the present reconstruction of the double line.

3. The Relationship between the Two Lines

3.1. The number of the royal inscriptions

It has been argued above that Line 1ab and Line 2 ruled contemporaneously. A survey of the inscriptions of the Middle Assyrian kings following Tukulti-Ninurta I and of the early Neo-Assyrian kings might reveal the impact of each line. The numbers of their inscriptions ('inscr.', with the regnal years ['ys.'] added) are:[72]

[71] For the attribution of the Broken Obelisk (BM 118898) to Tiglath-pileser I rather than to Ashur-bel-kala (the conventional attribution), see Mahieu (2018, 79–85); Shibata (2022).

[72] For the editions of these inscriptions, see RIMA 1, 300–322; RIMA 2. The inscriptions that cannot be assigned definitely to a ruler (RIMA's numbers 1001+) and the private inscriptions (RIMA's numbers 2001+) are excluded from the survey. The study neither considers administrative tablets and private letters, as these are often hard to link with a specific king.

Line 1a	*Line 2*
Ashur-nadin-apli (4 ys. - **3** inscr.)	Ninurta-apil-Ekur (13 ys. - **2** inscr.)
Ashur-nirari III (6 ys. - **0** inscr.)	
Enlil-kudurri-usur (5 ys. - **0** inscr.)	
Line 1b	
Ashur-rabi II (41 ys. - **0** inscr.)	Ashur-dan I (36 ys. - **1** inscr.)
	Ninurta-tukulti-Ashur (*ṭuppi* - **0** inscr.)
	Mutakkil-Nusku (*ṭuppi* - **0** inscr.)
Ashur-resh-ishi II (5 ys. - **1** inscr.)	Ashur-resh-ishi I (18 ys. - **14** inscr.)
Tiglath-pileser II (32 ys. - **1** inscr.)	Tiglath-pileser I (39 ys. - **37** inscr.)[a]
Ashur-dan II (23 ys. - **6** inscr.)	Asharid-apil-Ekur (2 ys. - **0** inscr.)
	Ashur-bel-kala (18 ys. - **6** inscr.)[b]
Adad-nirari II (21 ys. - **8** inscr.)	Eriba-Adad II (2 ys. - **3** inscr.)
	Shamshi-Adad IV (4 ys. - **5** inscr.)
Tukulti-Ninurta II (7 ys. - **17** inscr.)	Ashurnasirpal I (19 ys. - **0** inscr.)[c]
Ashurnasirpal II (25 ys. - **138** inscr.)	Shalmaneser II (12 ys. - **1** inscr.)
	Ashur-nirari IV (6 ys. - **0** inscr.)

Figure 5: The epigraphical relationship between Line 1ab and Line 2.

[a] Tiglath-pileser I's conventional 30 inscriptions should be increased to 37 inscriptions; see n. 76 below.
[b] Ashur-bel-kala's conventional 13 inscriptions should be reduced to 6 inscriptions; see n. 76.
[c] The brick inscription RIMA 2 A.0.92.1, which Grayson (1991, 122–123) assigns to Ashurnasirpal I, rather belongs to Ashurnasirpal II (as part of RIMA 2 A.0.101.129): Porter (2020).

Though the recovery of inscriptions is often conditioned by particular circumstances and inscriptions may still be hidden, while others may have been irretrievably lost, the corpus of inscriptions that is available at present provides enough material for a comparative analysis. The data show that, at first, the distribution of the inscriptions among the two lines is equal: both Line 1a and early Line 2 rarely issue inscriptions. The situation changes beginning with Ashur-resh-ishi I and Tiglath-pileser I in Line 2, who definitely have more inscriptions than their contemporaries in Line 1b. In the latter part, the higher number appears in Line 1b. This suggests a power shift from Line 2 to Line 1b during the last part of the double line.

3.2. The types of the royal inscriptions

To trace the power shift, not only the number of the inscriptions but also their nature should be considered. The contents of the inscriptions can be summarised as follows (with the amount of each type being added):[73]

Line 1a

Ashur-nadin-apli: 1× shrine (RIMA 1 A.0.79.1, from Assur), 2× '(property of) the palace' (A.0.79.2-3, from Assur).

Ashur-nirari III: None.

Enlil-kudurri-usur: None.

Line 2

Ninurta-apil-Ekur: 1× couple of vase fragments (A.0.82.1, from Assur), 1××× piece of turquoise (A.0.82.2, of unknown provenience).

Ashur-dan I: 1× '(property of) the palace' (A.0.83.1, from Assur).

Ninurta-tukulti-Ashur: None.

Mutakkil-Nusku: None.

Ashur-resh-ishi I: 4× temple of Ishtar at Nineveh (A.0.86.1–3, from Nineveh; RIMA 2 A.0.100.7, probably

[73] The classification is not perfectly strict since categories may overlap: an inscription recording a campaign may also mention building activities (see, for instance, n. 75 below).

from Nineveh),[74] 1× temple of Ishtar at Assur (RIMA 1 A.0.86.9, from Assur), 1× *hinter* house (*bīt kutalli*, A.0.86.4, from Nineveh), 1× palace (A.0.86.14, from Assur), 3× '(property of) the palace' (A.0.86.5–6, from Nineveh; A.0.86.10, from Apqu), 2× temple of Anu-Adad (A.0.86.7–8, from Assur), 2× temple of Assur (A.0.86.11–12, from Assur), 1× without a category (A.0.86.13, from Assur).

Tiglath-pileser I: 5× annalistic (RIMA 2 A.0.87.1–4, 10, from Assur and Nineveh), 2× palace at Assur (A.0.87.5, 8, from Assur), 1× palace at Nineveh (A.0.87.11, from Nineveh), 5× war (A.0.87.9, from Assur; A.0.87.12–13, from Nineveh; A.0.87.15, from near the source of the Tigris; A.0.87.16, from Yoncalı, in the region of Malazgirt), 8× '(property of) the palace' (A.0.87.17, 21, 24–29, from Assur and Nineveh), 2× temple of (Anu)-Adad (A.0.87.22–23, from Assur),[75] 7× without a category (A.0.87.6–7, 18–20, 30, from Assur; A.0.87.14, from Nineveh [by an unidentified Tiglath-pileser]). Seven annalistic inscriptions that are conventionally attributed to Ashur-bel-kala (A.0.89.1–3, 5–6, from Assur; A.0.89.7, 9, from Nineveh) seem to belong to Tiglath-pileser I.[76]

Asharid-apil-Ekur: None.

Ashur-bel-kala: 1× annalistic (A.0.89.4, from Assur), 3× '(property of) the palace' (A.0.89.10, from Nineveh; A.0.89.11–12, from Assur), 1× fragmentary stela (A.0.89.13, in the row of the royal stelae at Assur), 1× without a category (A.0.89.8, from the library of Assurbanipal at Nineveh).

Eriba-Adad II: 2× royal epithets (A.0.90.1–2, from Nineveh), 1× 'monument' (A.0.90.3, *ṣalam*, among the royal stelae at Assur).

Shamshi-Adad IV: 1× temple of the Assyrian Ishtar at Nineveh (A.0.91.1, from Nineveh; see n. 111 below), 2× temple of Ishtar at Nineveh (A.0.91.2–3, from Nineveh), 1× dedication to the god Ashur (A.0.91.4, from Assur), 1× 'monument' (A.0.91.5, among the royal stelae at Assur).

Ashurnasirpal I: None (see *Figure 5* n. c).

Shalmaneser II: 1× 'monument' (A.0.93.1, among the royal stelae at Assur).

Ashur-nirari IV: None.

Line 1b

Ashur-rabi II: None.

Ashur-resh-ishi II: 1× 'monument' (A.0.96.1, among the royal stelae at Assur).

Tiglath-pileser II: 1× 'monument' (A.0.97.1, among the royal stelae at Assur).

Ashur-dan II: 2× annalistic (A.0.98.1–2, from Assur), 1× Craftsman's Gate (A.0.98.3, from Assur), 1× courtyard of the temple of Assur (A.0.98.4, from Assur), 2× '(property of) the palace' (A.0.98.5, from Assur; A.0.98.6, from Kilizi).

Adad-nirari II: 5× annalistic (A.0.99.1–4, from Assur; A.0.99.5, from the library of Assurbanipal at Nineveh), 3× '(property of) the palace' (A.0.99.6, from Assur; A.0.99.7, from Nineveh and Babylon; A.0.99.8, from Shibaniba).

Tukulti-Ninurta II: 2× annalistic (A.0.100.1, 5, from Assur), 1× wall at Assur (A.0.100.2, from Assur), 1× palace at Assur (A.0.100.3, from Assur), 1× war (A.0.100.6, from Nineveh [originally from Nemed-Tukulti-Ninurta]), 1× temple of Assur (A.0.100.14, from Assur), 6× '(property of) the palace' (A.0.100.9, from Kahat; A.0.100.10, of unknown provenance; A.0.100.13, from the library of Assurbanipal at Nineveh; A.0.100.15–16, from Assur; A.0.100.17, from Nineveh), 1× necklace (A.0.100.11, from Khorsabad), 3× without a category (A.0.100.4, from Assur; A.0.100.8, unlocated; A.0.100.12, from Nineveh).

Ashurnasirpal II: 138 inscriptions, of all kinds: annalistic, temple constructions, etc. (from Assur, Kalhu, Nineveh, etc.).

The survey shows that annalistic records occur until Ashur-bel-kala in Line 2 and beginning with Ashur-dan II in Line 1b. This accords with the proposed power shift from Line 2 to Line 1b in the latter part of the double line. Furthermore, the sole annalistic record that certainly belongs to Ashur-bel-kala (RIMA 2 A.0.89.4; see n. 76 above) preserves, apart from the date formula (rev. 1′), only 14 fragmentary lines, which are close in contents to the inscriptions of Tiglath-pileser I, in particular to A.0.87.4 and A.0.87.10.[77] Since the text of Ashur-bel-kala's campaigns in A.0.89.4 (obv. 12–14) literally copies A.0.87.4 (ll. 6–8) and A.0.87.10 (ll. 6–8), the text probably does not render actual campaigns. If so, the genuine war reports in Line 2 would end with Tiglath-pileser I. In the *Synchronistic History* (ii.B9′–A37″), the kings Ashur-dan I, Ashur-resh-ishi I, Tiglath-pileser I and Ashur-bel-kala of Line 2 are all recorded to have intervened in Babylonia. While the chronicle reports devasting campaigns against Babylonia for Ashur-dan I, Ashur-resh-ishi I and Tiglath-pileser I, it

[74] RIMA 2 assigns A.0.100.7 to Tukulti-Ninurta II, but the fragment rather relates to the work by Ashur-resh-ishi I on the temple of Ishtar at Nineveh: MacGinnis (2022).

[75] In addition to these two inscriptions (RIMA 2 A.0.87.22–23), religious building activities by Tiglath-pileser I are also mentioned in three of his annalistic inscriptions (A.0.87.1, vi.86–93; vii.71–viii.22; A.0.87.3, ll. 16–17; A.0.87.10, ll. 29–30) and in two of his war inscriptions (A.0.87.12, ll. 24′–31′; A.0.87.13, l. 11′).

[76] Mahieu (2018, 85–86). Shibata (2022: 121–122 and n. 41) ascribes A.0.89.6–7, 9 to Tiglath-pileser I, but he retains A.0.89.1–3, 5 as inscriptions of Ashur-bel-kala.

[77] Weidner (1930–1931, 79 n. 15).

only records agreements with Babylonia for Ashur-bel-kala: Ashur-bel-kala concluded a treaty with Marduk-shapik-zeri of Isin II, appointed Adad-apla-iddina as king of Isin II, married the daughter of Adad-apla-iddina and joined Assyria and Babylonia together (ii.A25″–37″). This likewise suggests that Line 2 no longer was a warrior force by the time of Ashur-bel-kala. The power change took place in the days of Tiglath-pileser I (the father of Ashur-bel-kala, in Line 2) and Ashur-dan II (in Line 1b). The successors of Tiglath-pileser I in Line 2 do not have war inscriptions,[78] whereas Ashur-dan II and his successors in Line 1b all have war inscriptions.[79]

3.3. Evidence for the power shift

The proposal of a power shift from Line 2 to Line 1b in the days of Tiglath-pileser I and Ashur-dan II fits with the chronological settings of these two reigns. The reign of Tiglath-pileser I ended 13 + 36 + 18 + 39 = 106 years after the death of Tukulti-Ninurta I, while the reign of Ashur-dan II began 4 + 6 + 5 + 41 + 5 + 32 = 93 years after Tukulti-Ninurta I's death (see *Figure 3*). Tiglath-pileser I and Ashur-dan II were thus contemporaries for 106 - 93 = 13 years. The shift took place in their time.

The rise in power of Line 1b in the days of Ashur-dan II further accords with the fact that the beginning of the Neo-Assyrian period is generally set in the days of Ashur-dan II.[80] With Adad-nirari II, the son of Ashur-dan II, supremacy had definitely moved to Line 1b. Ashur-bel-kala is the last king of Line 2 mentioned in the *Synchronistic History* (ii.A25″–37″). The next Assyrian king to interfere in Babylonia is Adad-nirari II of Line 1b: he defeated Shamash-mudammiq and Nabu-shuma-ukin I, exchanged daughters in marriage with Nabu-shuma-ukin I and joined Assyria and Babylonia together (iii.A1–21).[81] The predominance had clearly moved to Line 1b.

The significance of Adad-nirari II's reign is also clear from the fact that the eponym lists A1, A2, A6 and A8 all begin with Adad-nirari II.[82] Moreover, Adad-nirari III claimed descent from Shamshi-Adad V, Shalmaneser III, Ashurnasirpal II, Adad-nirari II, Tukulti-Ninurta (I), Shalmaneser (I) and Ilu-kabkabi (Enlil-kapkapi, RIMA 3 A.0.104.1, ll. 9–27). The identification of the kings Tukulti-Ninurta and Shalmaneser as Tukulti-Ninurta I and Shalmaneser I is obvious: given that Tukulti-Ninurta II (Khors. *AKL* iv.18) comes between Ashurnasirpal II (iv.19–20) and Adad-nirari II (iv.16–17), Tukulti-Ninurta II cannot be intended; Tukulti-Ninurta I must be in view. Further, the relevant king Shalmaneser is called 'great king, strong king, who enlarged Eḫursagkurkurra, "the mountain of the lands"' (trans. RIMA 3 A.0.104.1, ll. 21–22). Since Shalmaneser I rebuilt this temple,[83] Shalmaneser I must be concerned. Hence, in A.0.104.1, Adad-nirari III mentions Adad-nirari II immediately before Tukulti-Ninurta I, Shalmaneser I and Ilu-kabkabi.[84] He linked the reign of Adad-nirari II to these famous reigns.

3.4. The context of the power shift: The Aramean campaigns

The cause of the power shift from Line 2 to Line 1b is revealed by the historical circumstances. The reign of Tiglath-pileser I was marked by the conflict with King Marduk-nadin-ahhe of Isin II (*Synchronistic History* ii.A14″–24″; *Walker Chronicle*, ll. 22–26;[85] inscriptions of Tiglath-pileser I)[86] and the intense campaigning against the Arameans (inscriptions of Tiglath-pileser I).[87] Tiglath-pileser I states that he 'crossed the Euphrates twenty-eight times, twice in one year, in pursuit of the *aḫlamû*-Aramaeans' (trans. RIMA 2 A.0.87.4, l. 34).[88] Though Tiglath-pileser I only reports victories over the Arameans, Tiglath-pileser I faced defeat as well: at the time of the death of Marduk-nadin-ahhe (*Chronicle of Tiglath-pileser I* ll. 8′–9′), the Arameans captured and plundered districts of Assyria (ll. 3′–5′), including the district of Nineveh and the land of Kilizi (l. 12′).[89] As a

[78] In Eriba-Adad II's three inscriptions (RIMA 2 A.0.90.1–3), the royal epithets found in the first two inscriptions describe Eriba-Adad II as a defeater of enemies, but they might not pertain to actual campaigns.

[79] The absence of campaigns for the kings following Tiglath-pileser I in Line 2 favours the ascription of the White Obelisk (BM 118807, RIMA 2 A.0.101.18) – which mainly records campaigns – to Ashurnasirpal II of Line 1b (the proposal of Sollberger 1974; Grayson 1991, 122, 254) rather than to Ashurnasirpal I of Line 2 (the proposal of Reade 1975).

[80] For instance, Grayson (1991, 131).

[81] The next Assyrian king who is recorded with a campaign against Babylonia is Shalmaneser III (*Synchronistic History* iii. A22–C5′).

[82] Millard (1994, 17–19). List A7 (= Ungnad's list C^c) is the only list to extend further back in time (see n. 52 above).

[83] RIMA 1 A.0.77.1, l. 6; ll. 6, 112–148; A.0.77.2, ll. 5–21; A.0.77.3, ll. 5–38; A.0.77.4, ll. 19–26; A.0.77.5, ll. 6–27.

[84] Ilu-kabkabi was the father of Shamshi-Adad I (Khors. *AKL* i.39 and an inscription of Shamshi-Adad I: RIMA 1 A.0.39.9).

[85] Glassner (2004, 282–283 [no. 46]).

[86] RIMA 2 A.0.87.4, ll. 44–51; A.0.87.10, ll. 45–53; Frame (2011, 129–131; CUSAS 17 68, ll. 34–48).

[87] RIMA 2 A.0.87.1, v.44–63; A.0.87.2, ll. 28–29; A.0.87.3, ll. 29–35; A.0.87.4, ll. 34–36; A.0.89.6, l. 7′; A.0.89.7, col. iii (the Broken Obelisk); A.0.89.9, l. 4′. For the ascription of A.0.89.6–7, 9 to Tiglath-pileser I instead of to Ashur-bel-kala, see n. 76 above.

[88] Cf. RIMA 2 A.0.87.3, ll. 29–30: 'I have crossed the Euphrates [...] times, twice in one year, in pursuit of the *aḫlamû*-Aramaeans, to the land Ḫatti'; A.0.89.6, l. 7′: '[...] in pursuit of the Aramaeans, which *twice* in one year [I crossed the Euphrates]'; A.0.89.9, ll. 3′–4′: '[... in pursuit of] the Aramaeans, which [*twice*] in one year [*I crossed the Euphrates*]' (italics by RIMA).

[89] Glassner (2004, 188–191 [no. 15]); Younger (2017, 218–219).

result, the Assyrians fled to the mountains of Habruri (ll. 6′–7′), and Tiglath-pileser I marched to Katmuhu (l. 13′). This Aramean assault might well have caused the breakdown of Line 2. It lost its hegemony over Assyria.

The expansion of Line 1b should be situated in this context as well. When campaigning against the Arameans, Tiglath-pileser I of Line 2 invaded the region of Hanigalbat,[90] that is, the territory of Line 1b. This local dynasty thus became involved in the conflict: the annals of Ashur-dan II of Line 1b record Ashur-dan II's victory over the Arameans (RIMA 2 A.0.98.1, ll. 15, 23–32; A.0.98.2, ll. 6′–16′). Following Tiglath-pileser I's defeat, the campaigns of Line 2 were apparently continued by Line 1b. Like Tiglath-pileser I, Ashur-dan II marched to Katmuhu (A.0.98.1, ll. 33–41). Ashur-dan II, moreover, brought back the exhausted people from Habruri (A.0.98.1, ll. 54–67),[91] the location to where the Assyrians had fled in Tiglath-pileser I's days. Ashur-dan II thus regained the territory that had been lost in Tiglath-pileser I's days. Conventionally, the reconquest is situated after 'more than a century'.[92] The reconquest seemingly rather came soon after.

The contents of an inscription of Ashur-dan II is enlightened by this reconstruction of the events. In RIMA 2 A.0.98.1, ll. 23–26, Ashur-dan II refers back to Ashur-rabi II: '[... I]ahānu, the land of the Aramaeans, which is behind the land Pi[..., which from the time of Aššur-ra]bi (II), king of Assyria, my forefather, the cities of the district of [my land, ...] they captured for themselves; [I mustered] chariots (and) troops. [*I plundered* ...] (and) inflicted upon them a major defeat' (italics by RIMA). Ashur-rabi II, the founder of Line 1b, was the first king of Ashur-dan II's dynasty to rule the area of Hanigalbat. In the context of his campaigns against the Arameans, Ashur-dan II referred back to the time when the tensions between his line and the Arameans began and thus justified his right to control the region: Line 1b had been there since the days of Ashur-rabi II.

A similar retrospection is found on the Kurkh Monolith of Shalmaneser III: 'At that time the city (Ana)-Aššur-utēr-aṣbat, which the people of the land Ḫatti call Pitru (and) which is on the River Sagura [by the opposite bank] of the Euphrates, and the city Mutkīnu, which is on the bank of the Euphrates, which Tiglath-pileser (I), my ancestor, a prince who preceded me, had established – at the time of Aššur-rabi (II), king of Assyria, the king of the Aramaeans had taken (these two cities) away by force – these cities I restored' (trans. RIMA 3 A.0.102.2, ii.35b–38). Like Ashur-dan II, Shalmaneser III (a descendant of Line 1b) refers back to his forefather Ashur-rabi II, in whose days the region was first inhabited by Line 1b. When Line 1b was founded by Ashur-rabi II in Hanigalbat, the Arameans took away part of the region. Later, in the context of his Aramean campaigns, Tiglath-pileser I established the cities Pitru and Mutkinu,[93] which two cities were finally restored by Shalmaneser III.[94]

The conflicts with the Arameans determined the political affairs in Assyria. Ashur-dan II's victory over the Arameans resulted in the emergence of Line 1b; it became the main Assyrian power. Line 2, on the other hand, declined following Tiglath-pileser I's defeat and became a minor power. The relationship between Line 1b and Line 2 likely continued to be peaceful, as it had been the case since the days of Ashur-rabi II and Ashur-dan I (see §2.4).

4. The Residences of the Two Lines

It has been argued above that, after the elimination of Line 1a, Line 2 became the major Assyrian power and that, following Tiglath-pileser I's defeat, it declined. Conversely, Line 1b first reigned in Hanigalbat as a minor power and, beginning with Ashur-dan II, it became the major power. A study of the royal building activities – and related issues – should clarify where these lines were located.

4.1. The locations of the royal building activities

The building enterprises by the Middle Assyrian kings following Tukulti-Ninurta I and by the early Neo-Assyrian kings can be summarised as follows:[95]

[90] For the development of Tiglath-pileser I's Aramean campaigns, see Younger (2017, 200–209). Tiglath-pileser I's presence in northernmost Mesopotamia is evidenced by 'the statues of Tiglath-pileser and Tukultī-Ninurta, kings of Assyria', which Ashurnasirpal II saw near the source of the River Subnat (RIMA 2 A.0.101.1, i.104–105). These kings can be identified with Tiglath-pileser I and Tukulti-Ninurta II: Grayson (1991, 61).

[91] RIMA's Kirriuru (ll. 54–55) should be read Habruri: Younger (2017, 218 n. 106, 220).

[92] Grayson (1991, 131).

[93] Tiglath-pileser I's conquest of Pitru may originally have been reported in A.0.89.6, l. 12′ and A.0.89.9, l. 7′: Grayson (1991, 98 [note to ll. 12′–13′]), 107 [note to ll. 6′b–8′]). Grayson ascribes both inscriptions to Ashur-bel-kala rather than to Tiglath-pileser I (cf. n. 76 above).

[94] The text does not imply that Tiglath-pileser I preceded Ashur-rabi II in time: it does not mention '(these two cities)'; the cities did not exist in the time of Ashur-rabi II but were established later, by Tiglath-pileser I.

[95] The survey is limited to construction activities that are explicitly located. Inscriptions with merely the formula '(Property of) the palace of PN' (without specification of the location of the palace) are not considered since the find spots of these inscriptions do not necessarily correspond to the locations of the relevant constructions. For instance, the stone slab RIMA 2 A.0.99.7 Ex. 6 (reporting the '[Property of] the palace of Adad-nārārī') was found at Babylon, where

Line 1a

Ashur-nadin-apli: Ashur-nadin-apli constructed at Assur, his city.[96]

Ashur-nirari III: No topographical indications of building activities.

Enlil-kudurri-usur: No topographical indications of building activities. Since Ninurta-apil-Ekur marched out to take Libbi-ali (the old city centre of Assur) from his opponent Enlil-kudurri-usur (*Synchronistic History* ii.B7′), Enlil-kudurri-usur resided at Assur.

Line 2

Ninurta-apil-Ekur: Following his failure to take the city of Assur, Ninurta-apil-Ekur retreated and '[went home/back to this country]' (*Synchronistic History* ii.B8′),[97] which implies that he resided in a city other than Assur.

Ashur-dan I: The temple of Ishtar at Nineveh suffered from an earthquake in the days of Ashur-dan I (RIMA 1 A.0.86.1, l. 10; A.0.86.2, l. 5; A.0.86.3, l. 4′).[98] This shows that Nineveh belonged to Ashur-dan I's dominion. At Assur, Ashur-dan I pulled down the temple of Anu-Adad (RIMA 2 A.0.87.1, vii.66–68).[99]

Ninurta-tukulti-Ashur: No topographical indications of building activities.

Mutakkil-Nusku: Mutakkil-Nusku rebuilt a palace at Nineveh (A.0.87.10, l. 80).[100] Since Mutakkil-Nusku only ruled for a *ṭuppi* period (like his predecessor Ninurta-tukulti-Ashur; see *Figure 3* n. d above) and then died (Khors. *AKL* iii.36), the palace must have been built during the reign of Mutakkil-Nusku's father, Ashur-dan I.

Ashur-resh-ishi I: At Nineveh, Ashur-resh-ishi I built a palace (RIMA 1 A.0.86.5; RIMA 2 A.0.87.10, l. 63), restored the temple of Ishtar (RIMA 1 A.0.86.1–3) and restored the royal store house (A.0.86.4). At Assur, he worked on the temple of Anu-Adad (A.0.86.7–8), the shrine of Assur (A.0.86.11–12), the temple of Ishtar (A.0.86.9) and a palace (A.0.86.14). He built a palace at Apqu as well (RIMA 2 A.0.89.7, v.34 [the Broken Obelisk]).

Tiglath-pileser I: At Nineveh, Tiglath-pileser I restored the city wall (A.0.87.10, ll. 54–62), restored the temple of Ishtar (A.0.87.12, ll. 24′–31′) and worked on two palaces (A.0.87.10, ll. 63–88; A.0.87.11). The Arameans attacked Tiglath-pileser I in the district of Nineveh (*Chronicle of Tiglath-pileser I*, l. I 12′).[101] At Assur, he worked on a palace A.0.87.4, ll. 72–89; A.0.87.5; A.0.87.8; A.0.87.17, l. 4; (A.0.89.7, v.14–19),[102] and he reconstructed the temple of Anu-Adad (A.0.87.1, vii.71–viii.22; A.0.87.3, ll. 16–17; A.0.87.10, ll. 29–30; A.0.87.13, l. 11′; A.0.87.22–23; A.0.89.7, ii.14–15; RIMA 3 A.0.102.39, ll. 6–8),[103] other temples (RIMA 2 A.0.87.1, vi.85–93), the moat and wall (A.0.87.3, ll. 36–44; A.0.89.7, v.6–8, 11–14), the doors of the Craftsman's Gate (A.0.89.7, v.9–11)[104] and further constructions (storehouses, the house of the *šaḫūru*, the terrace on the north side, a canal, gardens, the quay by the Tigris Gate and the terrace of the New Palace: A.0.89.7, v.1–6, 20–31). Tiglath-pileser I called Assur 'my city'.[105] He also worked on the quay wall facing the river Ḫusir (A.0.87.24–27), built palaces at Pakute,[106] Sikkatu and Saqa and worked on the palace of Apqu (A.0.89.7, v.32–34).

Asharid-apil-Ekur: No topographical indications of building activities.

Ashur-bel-kala: A stone female torso belonging to the palace of Ashur-bel-kala was found at Nineveh (A.0.89.10), suggesting that Ashur-bel-kala had a palace at Nineveh.[107] The building activities at Assur attributed to Ashur-bel-kala on the basis of the Broken Obelisk belong to Tiglath-pileser I.[108]

Adad-nirari II did not build. Palace belongings could be moved, and the formula '(Property of) the palace of PN' traced the objects' origins. Cf. RINAP 2 65, ll. 408–409 (of Sargon II): 'I loaded the property of the palace of Urzana and of the god Ḫaldi, together with his (Urzana's) numerous possessions that I carried off as booty from the city Muṣaṣir, on (the backs of the soldiers of) the main body of my extensive army and I had (them) convey (it) to Assyria.' Similarly, the Broken Obelisk 'was found at Nineveh but originally must have been erected in Aššur' (Grayson 1991, 99).

[96] RIMA 1 A.0.79.1, ll. 15–32, with l. 16: 'my city Aššur'; ll. 25, 29: 'my city'.

[97] The reconstruction '[went home]' is Grayson's. Tadmor and Glassner propose '[went back to his country]' (see n. 46 above).

[98] The three inscriptions all belong to Ashur-resh-ishi I.

[99] An inscription of Tiglath-pileser I.

[100] An inscription of Tiglath-pileser I.

[101] Glassner (2004, 188–191 [no. 15]); Younger (2017, 219).

[102] The Broken Obelisk (RIMA 2 A.0.89.7) belongs to Tiglath-pileser I rather than to Ashur-bel-kala; see n. 108 below.

[103] An inscription of Shalmaneser III.

[104] Cf. RIMA 2 A.0.98.3, ll. 5–8 (an inscription of Ashur-dan II); see n. 70 above.

[105] RIMA 2 A.0.87.1, ii.95; iv.37; v.24–26, 62; vi.69, 75, 89; A.0.87.2, ll. 24, 27, 29, 33; A.0.87.3, ll. 35–36; A.0.87.4, ll. 36, 40, 43; A.0.87.10, ll. 40, 44; A.0.87.12, l. 2′; A.0.89.1, l. 13′ (conventionally attributed to Ashur-bel-kala; see n. 76 above); A.0.89.7, ll. 7, 12, 16; Frame (2011, 130; CUSAS 17 68, l. 23).

[106] Frame (2011, 131; CUSAS 17 68, ll. 49, 58–63).

[107] Cf. Goodspeed (1902, 172 [§150]): 'It has been inferred, from the finding of a statue in Nineveh hailing from the king's palace, that Ashur-bel-kala removed the capital from Assur to Nineveh.'

[108] For the attribution of the Broken Obelisk (BM 118898) to Tiglath-pileser I rather than to Ashur-bel-kala (the conventional attribution), see Mahieu (2018, 79–85); Shibata (2022).

The supposed mention of a renovation of the wall of the palace terrace at Assur by Ash[ur-bel-k]ala in an inscription of Tukulti-Ninurta II (A.0.100.5, l. 137) rather concerns Ash[ur-uba]llit I.[109] There is no evidence for building activities by Ashur-bel-kala at Assur.

Eriba-Adad II: The building portion in an inscription of Eriba-Adad II found at Nineveh is badly broken (A.0.90.1).[110]

Shamshi-Adad IV: At Nineveh, Shamshi-Adad IV worked on the towers of the temple of the Assyrian Ishtar (A.0.91.1, ll. 4–5)[111] and on the temple of Ishtar (A.0.91.2–3).

Ashurnasirpal I: No topographical indications of building activities. When ill, Ashurnasirpal I addressed a prayer to Ishtar of Nineveh, the goddess who brought him to kingship.[112] A further hymn was issued on the occasion of an offering to Ishtar (probably of Nineveh) by Ashurnasirpal I.[113]

Shalmaneser II: No topographical indications of building activities.

Ashur-nirari IV: No topographical indications of building activities.

Line 1b

Ashur-rabi II: Ashur-rabi II worked on the *Bīt-naṯi* in the complex of Ishtar at Nineveh (RIMA 2 A.0.101.58; A.0.101.65).[114]

Ashur-resh-ishi II: No topographical indications of building activities.

Tiglath-pileser II: No topographical indications of building activities.

Ashur-dan II: At Assur, Ashur-dan II rebuilt the New Palace (A.0.98.1, ll. 73–81), rebuilt the Craftsman's Gate (A.0.98.3) and adorned the courtyard of the temple of Assur with clay cones (A.0.98.4). Ashur-dan II called Assur 'my city' (A.0.98.1, ll. 21, 37–38, 45, 57; A.0.98.3, l. 13).

Adad-nirari II: At Assur, Adad-nirari II restored the quay wall (A.0.99.1, rev. 10′–16′) and the Gula temple (A.0.99.2, ll. 128–131). Adad-nirari II called Assur 'my city' (A.0.99.1, obv. 16; rev. 5′, 9′; A.0.99.2, ll. 60, 88, 119). He rebuilt the city of Apqu as well (A.0.99.2, ll. 36–38). He deported prisoners to Nineveh (A.0.99.2, ll. 80–81). There is no explicit evidence for building activities at Nineveh.[115]

Tukulti-Ninurta II: At Assur, Tukulti-Ninurta II reconstructed the wall of (probably) Baltil (the oldest quarter of the city Assur, A.0.100.2), the terrace of his palace (A.0.100.3; A.0.100.5, ll. 136–142) and the shrine of the god Enpi in the temple of Assur (A.0.100.14;[116] cf. A.0.100.5, l. 27). He had palaces at Nemed-Tukulti-Ninurta (A.0.100.6, l. 10) and Kahat (A.0.100.9). There is no explicit evidence for building activities at Nineveh.[117] Tukulti-Ninurta II stayed temporarily at Nineveh (A.0.100.5, ll. 8–9, 13) and had his residency at Assur (A.0.100.5, ll. 30, 41).

Ashurnasirpal II: At Nineveh, Ashurnasirpal II worked on the temple of Ishtar (including the *Bīt-naṯi*) and the temple of Adad. At Assur, he worked on the temple of Assur and the temple of Sin-Shamash. He built a small palace at Nineveh[118] and had a palace at Assur.[119] He also built the capital of Kalhu and worked on the cities of Imgur-Enlil and Apqu.[120] Ashurnasirpal II called both Assur (A.0.101.1, l. 46; A.0.101.18, l. 8′; A.0.101.52, l. 2′; A.0.101.67, l. 10) and Kalhu (A.0.101.2, l. 37; A.0.101.4, l. 3′) 'my city'.

4.2. The identification of the residences on the basis of the topographical data

The survey provides valuable information on the locations of the Assyrian powers.

Line 1a resides at Assur: both Ashur-nadin-apli and Enlil-kudurri-usur relate to the city of Assur.

Line 1b first resides in Hanigalbat: Ashur-rabi II (except for the *Bīt-naṯi* at Nineveh), Ashur-resh-ishi II and Tiglath-pileser II do not construct in Assyria.[121]

[109] Mahieu (2020c).

[110] The Eḫursagkurkurra is mentioned, though in relation with prayer rather than with building activities (RIMA 2 A.0.90.1, rev. 1″–5″): '… […] storeroom […] for number […] and may he receive his prayer […] Eḫursagkurkurra […]'. George (1993, 101–102 [no. 486]) links it with the Eḫursagkurkurra at Assur, but Reade (2000, 409 [§13.2]) proposes that it might concern an Eḫursagkurkurra for the god Ashur at Nineveh.

[111] All the clay cones that attest the inscription were found at Nineveh (Grayson 1991, 117). This worship of the Assyrian Ishtar is exceptionally located at Nineveh instead of at Assur: Meinhold (2009, 63–64).

[112] Foster (2005, 327–330).

[113] Foster (2005, 331–333).

[114] These two very fragmentary inscriptions are generally both ascribed to Ashurnasirpal II.

[115] Cf. Reade (2005, 374).

[116] RIMA 2 relates A.0.100.7 to Tukulti-Ninurta II's restoration of the shrine of the god Enpi as well, but it rather pertains to Ashur-resh-ishi I (see n. 74 above).

[117] Cf. Reade (2005, 375).

[118] Thompson and Hutchinson (1931, 82–83).

[119] Orlamünde (2004).

[120] Ashurnasirpal II's building activities are summarised in Grayson (1991, 189–190). His inscriptions are presented on pp. 191–386 (RIMA 2 A.0.101.1–138).

[121] The sole inscriptions for the first three kings of Line 1b are the two stelae of Ashur-resh-ishi II and Tiglath-pileser II

Assur	Hanigalbat (with the 'Province of Assur')
Line 1a (Ashur-nadin-apli, Ashur-nirari III and Enlil-kudurri-uṣur)	Line 2 (Ninurta-apil-Ekur)
Line 2 (Ashur-dan I, Ninurta-tukulti-Ashur, Mutakkil-Nusku, Ashur-resh-ishi I and Tiglath-pileser I)	Line 1b (Ashur-rabi II, Ashur-resh-ishi II and Tiglath-pileser II)
Assur	**Nineveh**
Line 1b (Asharid-dan II, Adad-nirari II, Tukulti-Ninurta II and Ashurnasirpal II)	Line 2 (Asharid-apil-Ekur, Ashur-bel-kala, Eriba-Adad II, Shamshi-Adad IV, Ashurnasirpal I, Shalmaneser II and Ashur-nirari IV)

Figure 6: The residences of Line 1ab and Line 2.

Beginning with Ashur-dan II, Line 1b is clearly located at Assur: Ashur-dan II, Adad-nirari II, Tukulti-Ninurta II and Ashurnasirpal II all construct at Assur. Ashur-dan II calls himself *ni-bit Aš-šur*, 'designate of Aššur' (RIMA 2 A.0.98.1, l. 1), a unique epithet.[122] Election by a god/gods could point to the instigation of a new reign (see n. 19 above). Here, it would denote the installation of Line 1b at Assur.

Line 2 likewise attests an evolution in its location. The first king, Ninurta-apil-Ekur, does not reside at Assur; the contemporary Line 1a is residing there. Given that he is nevertheless called a king of Assyria, he likely reigns in Hanigalbat, where an Assyrian kingdom (including the 'Province of Assur') seems to have been established (see §2.4). His father Ili-pada likewise resides in the area of Hanigalbat, though more to the south, probably in Dur-Katlimmu.[123] Following the defeat of Line 1a, Line 2 settles at Assur: Ashur-dan I, Ashur-resh-ishi I and Tiglath-pileser I have Assur in their control. Beginning with Asharid-apil-Ekur, Tiglath-pileser I's successor, no king of Line 2 constructs at Assur. Henceforth, Line 2 resides at Nineveh. Most kings of Line 2 are related to that city: Ashur-dan Mutakkil-Nusku I, Ashur-resh-ishi I, Tiglath-pileser I, Ashur-bel-kala, Eriba-Adad II, Shamshi-Adad IV and Ashurnasirpal I. The only exceptions Ninurta-tukulti-Ashur are Asharid-apil-Ekur, Shalmaneser II and Ashur-nirari IV, but these four kings lack any topographical indication. The retreat of Line 2 to Nineveh explains why Shamshi-Adad IV venerated the Assyrian Ishtar at Nineveh instead of at Assur (see n. 111 above): Line 2 moved the veneration of the Assyrian Ishtar, the goddess of its former residence, to its new residence, Nineveh.

The proposed link between Line 2 and Nineveh is endorsed by the fact that, except for Ashur-rabi II and Ashurnasirpal II, no king of Line 1b constructed at Nineveh. The cases of Ashur-rabi II and Ashurnasirpal II are even closely connected: Ashur-rabi II's work on the *Bīt-naṯi* at Niniveh is only attested in inscriptions of Ashurnasirpal II (see n. 114), and the *Bīt-naṯi* is likewise only mentioned in inscriptions of Ashurnasirpal II.[124] Ashur-rabi II's work on the *Bīt-naṯi* likely relates to the foundation of the double line (see §2.4), and Ashurnasirpal II lived at the time of the end of that double line. In referring back to the *Bīt-naṯi* of his forefather Ashur-rabi II, Ashurnasirpal II apparently evoked the entire period of parallel reign.

To conclude, Line 1a resided in Assur, with Ninurta-apil-Ekur residing contemporeously in Hanigalbat. When Line 1a was defeated, Line 1b emerged in Hanigalbat and Line 2 installed itself in Assur. Following the power shift in the days of Ashur-dan II of Line 1b and Tiglath-pileser I of Line 2, Line 1b moved to Assur and Line 2 retreated to Nineveh. The data can be schematised as follows *(fig. 6)*.[125]

among the royal stelae at Assur (see §3.2). The three kings did not issue material in Assyria.

[122] Seux (1967, 205).

[123] The grand viziers of Hanigalbat seem to have resided at Dur-Katlimmu: Radner and Kühne (2008, 543, 546). For a schematic map with the positions of 'Province of Aššur' (labelled the 'Possessions of Aššur') and Dur-Katlimmu, see Gauthier (2016, 13; Figure Intro-2).

[124] In addition to RIMA 2 A.0.101.58 and A.0.101.65, the *Bīt-naṯi* occurs in three more inscriptions of Ashurnasirpal II: RIMA 2 A.0.101.18, epigraph l. 1; A.0.101.56, l. 15; A.0.101.137, l. 3.

[125] The three identified royal tombs at Assur (see n. 63

The existence of these two Assyrian kingdoms might be alluded to in the *Babylonian King List A*, which mentions Tiglath-pileser III and Shalmaneser V (Ululayu) as kings of Baltil (iv.8-9), i.e. as kings of the city Assur, and Sargon II and Sennacherib (and the latter's son, the Babylonian king Ashur-nadin-shumi) as kings of Habigal (iv.11-12, 16), i.e. as kings of Hanigalbat.[126] Sargon II resided at Khorsabad and Sennacherib at Nineveh, both cities to the north of Kalhu and Ashur. Therefore, the *Babylonian King List A* links these two kings with the northern kingdom of Hanigalbat.

The geographical distribution sheds light on the adventures of the statue of Marduk as well. Tukulti-Ninurta I brought Marduk's statue to Assyria following his capture of Babylon, and the statue was later returned by Ninurta-tukulti-Ashur[127] (*Chronicle P* iv.4–6, 12–13). Given that the Kassite king Adad-shuma-usur assisted Ninurta-apil-Ekur against Enlil-kudurri-usur (see n. 46 above), one might wonder why Ninurta-apil-Ekur did not return the statue to Babylon, as a thanksgiving, and why this happened only in the days of his grandson Ninurta-tukulti-Ashur.[128] The differing residences might reveal the reason: since Ninurta-apil-Ekur did not reign at Assur, he did not have the statue in his possession. Only when Adad-shuma-usur brought an end to Enlil-kudurri-usur's reign (see n. 50), that is, when Line 1a ended, did Line 2 take control over Assur. By that time, Ninurta-apil-Ekur had died and his son Ashur-dan I was ruling.[129] Ninurta-tukulti-Ashur, the son of Ashur-dan I, then returned the statue to Babylon.[130]

above) all belong to kings who did not reside at Assur: Ashur-bel-kala resided at Nineveh, and Ashurnasirpal II and Shamshi-Adad V had their capital at Kalhu. The Assyrian kings were apparently buried at Assur and not in their respective residences.

[126] Thus Fales 2014. For the *Babylonian King List A*, see Grayson 1980, 90-96 (§3.3).

[127] The name Tukulti-Ashur in *Chronicle P* iv.12 is generally emended into Ninurta-tukulti-Ashur: Brinkman (1968, 102 n. 557); Boese (1982, 20 and n. 36); Chen (2020, 72).

[128] Llop and George (2001–2002, 12): 'Es ist sehr wahrscheinlich, dass Ninurta-apil-Ekur, der Grossvater von Ninurta-tukulti-Aššur, die Hilfe des babylonischen Königs Adad-šuma-uṣur in seinem Kampf um den assyrischen Thron gegen Enlil-kudurrī-uṣur in Anspruch genommen hatte. Wie ist es zu erklären, dass nicht bereits Adad-šuma-uṣur als Gegenleistung für die Unterstützung das Marduk-Kultbild zurückgefordert hatte? Warum hatte man bis in die Regierung des Ninurta-tukulti-Aššurs gewartet, um das Kultbild zurückzugeben? Diese Fragen sind leider noch nicht zu beantworten.'

[129] Ninurta-apil-Ekur's reign ended two years before Enlil-kudurri-usur's (see *Figure 3*).

[130] The statue of Marduk returned before the victory over the Kassite king Enlil-nadin-ahi by the Elamite king Kutir-Nahhunte II, who brought the statue to Elam on that occasion (an inscription of Nebuchadnezzar I: RIMB 2 B.2.4.6, obv. 3′–13′).

4.3. The status of Assur and Nineveh during the late Middle Assyrian period

Since Old Assyrian times, Assur had been the capital of the Assyrian Empire. The situation briefly changed when Tukulti-Ninurta I moved the capital to Kar-Tukulti-Ninurta (3km to the north of Assur). Following Tukulti-Ninurta I's death, the capital returned to Assur.[131] Assur remained the centre during the late Middle and early Neo-Assyrian periods: first as the capital of Line 1a, then as the capital of early Line 2 and finally as the capital of Line 1b.[132]

In addition to Assur, Nineveh was an important centre as well. Ashur-uballit I (RIMA 1 A.0.77.17, ll. 7, 10;[133] RIMA 2 A.0.87.12, l. 26′),[134] Shalmaneser I (RIMA 1 A.0.77.17–19; A.0.77.24; A.0.77.29; A.0.86.1, l. 9;[135] RIMA 2 A.0.87.12, l. 27′)[136] and Tukulti-Ninurta I (RIMA 1 A.0.78.33–34) all worked on the temple of Ishtar at Nineveh. Nineveh's fame explains why the city could become the residence of late Line 2.[137] Its status as a centre is further evidenced by an inscription of Sennacherib that states that, long before Sennacherib (who made Nineveh his capital), Nineveh had been a royal residence.[138]

Conclusion

As a general conclusion, it can be noted that the late Middle and early Neo-Assyrian periods manifest a double line of rulers that lasted for 169 years: the kings Ashur-nadin-apli until Enlil-kudurri-usur (in Line 1a) followed by Ashur-rabi II until Ashurnasirpal II (in Line 1b) ruled in parallel to the kings Ninurta-apil-Ekur until Ashur-nirari IV (in Line 2). The parallel line began during the turmoil following the death of Tukulti-Ninurta I. Ninurta-apil-Ekur, son of the viceroy

[131] Kar-Tukulti-Ninurta continued to be settled through the Neo-Assyrian period, though no longer as an administrative and religious centre: Dittmann (2011, 176).

[132] At the end of the double line, Ashurnasirpal II moved the capital to Kalhu (see n. 65 above).

[133] An inscription of Shalmaneser I.

[134] An inscription of Tiglath-pileser I.

[135] An inscription of Ashur-resh-ishi I.

[136] For a survey of the archaeological and textual evidence for Shalmaneser I's work on the temple of Ishtar at Nineveh, see Reade (2005, 371–372).

[137] A similar situation with two contemporary capitals had occurred before: Kar-Tukulti-Ninurta, as capital of Tukulti-Ninurta I's kingdom, probably coexisted with Assur, as capital of Shalmaneser I's kingdom, during the last seven years of Shalmaneser I's reign: Mahieu (2020b, 244–245 and n. 35).

[138] RINAP 3/1 1, ll. 66–67: 'in which [city of Nineveh] since time immemorial earlier kings, my ancestors, before me exercised dominion over Assyria and ruled the subjects of the god Enlil, and wherein annually, without interruption, they received an income unsurpassed in amount, the tribute of the rulers of the four quarters (of the world).'

Ili-pada in Hanigalbat, founded Line 2, in opposition to the reign of Tukulti-Ninurta I's son Ashur-nadin-apli (in Line 1a). Line 1a (centred at Assur) came to an end when Ashur-rabi II (in Line 1b), a grandson of Tukulti-Ninurta I, sided with Line 2 (and Babylonia) to overthrow Line 1a. Henceforth, Line 2 resided at Assur, and Line 1b resided in Hanigalbat. When Tiglath-pileser I of Line 2 was defeated by the Arameans, Line 1b emerged as a power. Line 1b installed itself at Assur, and Line 2 retreated to Nineveh. Line 2 ended in the days of Ashurnasirpal II and only Line 1b continued, with Shalmaneser III as the first sole Assyrian king.

Abbreviations and Ancient Sources

AKL *Assyrian King List*. (1) Edited and transliterated by Nassouhi 1927 (Nassouhi list). (2) Edited, transliterated and translated by Gelb 1954 (Khorsabad and SDAS lists). (3) Transliterated by Grayson 1980, 101–115 (§3.9). (4) Transliterated and translated by Glassner 2004, 136–145 (no. 5).

CAD 19 Roth 2006

Chronicle P. (1) Transliterated and translated by Grayson 1975, 170–177 (Chronicle 22). (2) Transliterated and translated by Glassner 2004, 278–281 (no. 45).

EA *Amarna Letters*. (1) Transliterated and translated into German by Knudtzon 1908. (2) Translated by Moran 1992. (3) Transliterated and translated by Rainey 2015.

RIMA 1 Grayson 1987

RIMA 2 Grayson 1991

RIMA 3 Grayson 1996

RIMB 2 Frame 1995

RINAP 2 Frame 2021

RINAP 3/1 Grayson and Novotny 2012

Synchronistic History. (1) Transliterated and translated by Grayson 1975, 157–170 (Chronicle 21). (2) Transliterated and translated by Glassner 2004, 176–183 (no. 10).

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EA 16: Napḫuriya Revisited

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The Amarna letter EA 16 is generally considered to provide one of the most important synchronisms between Assyria and Egypt in the Late Bronze Age. Together with EA 15, it contains the only correspondence between Assyria and Egypt that has come down to us from the Amarna period. However, the names of both the sender and the recipient, the latter poorly preserved, are only mentioned in EA 16. Therefore, it is the aim of this article to revisit the reading of the recipient's name by collating it from newly available photographs since direct access to EA 16 is currently not possible.

EA 16

Provenience and current location

EA 16 is among 382 cuneiform-inscribed clay tablets believed to have been found at 'Tell el-Amarna', ancient Akhetaten, the capital of the pharaoh Amenhotep IV/Akhenaten, located about 300km south of Cairo. Nearly all these tablets were acquired in the antiquities market beginning in 1887 rather than through controlled archaeological excavations.[1] Today, EA 16 should be located in the Egyptian Museum in Cairo. But since Anson F. Rainey collated it in 1980 and 1981,[2] it has been nearly impossible for scholars to gain access to it. Although not available to me in the Egyptian Museum in Cairo,[3] Prof. Jana Mynářová, Charles University, kindly provided the present author with some eleven photographs of the tablet.[4]

Reading the pharaoh's name

EA 16 was written by one Aššur-uballiṭ to an Egyptian pharaoh, and it is broadly accepted that that name should be read '*Napḫuriya*', which is a throne name of Akhenaten.[5] However, because the tablet surface is damaged in this line, various suggestions have been made over the years as to how the name should be read. Some of these suggestions are presented here.

Jörgen Alexander Knudtzon transliterated the first line as 'a-na ᴵn[a-a]p-ḫu-[r]i-i-ᵍ _ [_ *šarri rabî*]'.[6] In his note g, Knudtzon wrote: 'Nach *i* sind zwei schräge Keile, der eine über dem anderen, zu sehen und dahinter Reste von zwei ähnlichen solchen oder vielleicht nur von einem größeren.'[7]

Edmund Gordon also collated this line. However, he did not publish his notes, which are only available to the public as quotations by William L. Moran and Pinhas Artzi. Moran quoted Gordon's transliteration as follows: 'ᵐ˹ḫu˺-[r]u/[r]a-*ḫu-ri-i-*˹tu˺-[x-x'.[8] However, Artzi cited Gordon's transliteration a little differently: 'EG[9] (whose collations are at my disposal, thanks to Dr Seymour Gittin, director of Albright Institute, Jerusalem) collated the Royal PN and found the following traces: *hu!-ra!-hu-ri-i-tu* [xx] (the last xx: place for [LUGAL GAL]). The result may be that the addressee is no [sic!] other than Ay, the post Amarna Pharaoh, whose throne-name is: Heper-heperu-reᶜ; this would be an obviously totally impossible solution!'[10]

[1] See Rainey (2015, 1–2).

[2] See Rainey (2015, XI).

[3] In response to a recent inquiry to the museum, the author received the following response: 'Dear Mr. Dams, after consulting the director of the Egyptian Museum in Cairo, she informed me that the tablet is not housed in their premises' (email correspondence May 22, 2021, with Dr Hala Ghoname, research associate at the University of Marburg, who kindly helped me with my request). However, the author could identify EA 16 on private pictures taken in November 2022 at the exhibition in the Egyptian museum.

[4] Made available on May 29, 2021. After obtaining permission for publication of the photos by the Egyptian Museum in Cairo, Jana Mynářová also gave her permission for publication. For this reason, it is possible – as far as the author is aware – to publish some photos of EA 16 in this article for the first time.

[5] See von Beckerath (1999, 142).

[6] Knudtzon (1915, 126).

[7] Knudtzon (1915, 127, n. g). The English translation is: 'After *i* there are two oblique wedges, one above the other, and behind them the remains of two similar ones, or perhaps only one larger one.'

[8] Moran (1992, 39, n. 1).

[9] That is: Edmond Gordon's collations, author's note.

[10] Artzi (1997, 326).

Moran himself did not transliterate the name at all, but instead commented, 'The reading of the name is uncertain. (...) I thought the last sign might be *ʾa*, ʾ replacing *y* as glide.'[11]

Artzi, on the other hand, was not very enthusiastic about leaving the name open: 'I wish to signal my courteous disagreement with EAMr's[12] solution, who left the space empty under the influence of EG-s collation.'[13] Artzi himself transliterated the first line as '*a-na* (1) ***N⌜a-a⌝p-ḫu⌜-ri⌝-i-⌜ʾå⌝*** ⌜LUGAL GAL⌝'.[14] As for the last sign of the name he wrote: 'The only point of difference from the original reading is, that – like EAMr – I suspect the last sign after -*i*-, in EAK[15] (and EGs) reading, being *ʾa* (AS 233); cf. EAK note g! This would be an extraordinary exact transliteration of the EDN.'[16]

In the latest publication available for EA 16, Rainey transliterated this first line, '⌜*a*⌝-⌜*na*⌝ ⌜⌝ ⌜*Na*⌝-⌜*ap*⌝-⌜*ḫu*⌝-⌜*ri*⌝-*i*-*ia*!(TU?) [LUGAL GAL]'.[17] However, his wife Zipora Cochavi-Rainey, who, together with William M. Schniedewind, edited Rainey's work *post mortem*, wrote in the commentary: 'Rainey did not accept Gordon's reading. He noted the traces of the sign ⌜Ḫ⌝ and added the sign ⌜IA⌝ to Knudtzon's restoration of the Pharaoh's personal name.'[18]

In summary, from all these readings, three facts are particularly striking. First, that there is no complete consensus on the reading, although the overwhelming majority is in favour of '*Napḫuriya*'. Second, the markings of the damage differ greatly in the transliterations. This shows how uncertain scholars are about the quality of the preserved signs, even when they come to the same reading for various reasons. Third, the last sign of the royal name seems to be particularly controversial. This indicates that this reading must be treated with caution, as well as the resulting synchronism.[19] For this reason, the name will be collated again in the following section with special attention to the last sign of the pharaoh's name.

[11] Moran (1992, 39, n. 1).

[12] That is: Artzi's abbreviation for Moran (1992), author's note.

[13] Artzi (1997, 325).

[14] Artzi (1997, 321).

[15] That is: Artzi's abbreviation of Knudtzon (1915), author's note.

[16] Artzi (1997, 326). EDN means 'Egyptian Deity Name'.

[17] Rainey (2015, 130).

[18] Rainey (2015, 1348).

[19] If the reading of the name were certain, the pharaoh's connection to Aššur-uballiṭ I through this letter EA 16 would represent an important synchronism between Egyptian and Mesopotamian chronologies. As long as this is not the case, the synchronism must also be treated with caution.

Collation and Discussion of EA 16:1

Siglum:	C 4746 (12209).
Editions:	Knudtzon (1915, 126–131 and 1037–1039); Grayson (1972, 48–49, only translation); Moran (1992, 38–41, translation and commentary); Artzi (1997, 320–336); Liverani (1999, 363–364, only translation); Rainey (2015, 130–133 and 1348–1349).
Copy:	L. Abel in: Winckler (1889, no. 9).
Photos:	J. Mynářová.
Collection:	Egyptian Museum in Cairo.
Provenience:	From the antiquities market, probably 'Tell el-Amarna'.
Content:	Letter from the Assyrian king Aššur-uballiṭ to the Egyptian pharaoh.
Collation:	By the author of photos.

In the following, a photo showing EA 16 line 1 is given. Below is an autograph by the author and an excerpt from Ludwig Abel's drawing for direct comparison (*Figure 1*), as well as a transliteration by the author. Each cuneiform sign is then discussed in turn. As far as the marking of the damaged text in the transliteration is concerned, this is always subject to a certain degree of interpretation and will therefore be slightly different for each researcher. In this article, partial damage is only marked if individual wedges are difficult or impossible to identify.

Transliteration:

Obv. 1:

⌜*a*⌝-*na* ⌜*a*⌝-⌜*ap*⌝-⌜*ḫu*⌝-⌜*ri*⌝-*i*-*ʾa* [LUGAL GAL]

Discussion of the individual signs:

⌜*a*⌝: This sign is badly damaged and difficult to see due to poor lighting in the photograph. However, it is to be expected here following the typical Assyrian epistolary format and can therefore be reconstructed with certainty.[20] In terms of shape, a comparison with other A-signs in the text shows that the broken wedge on the right side of the sign often looks like a single continuous wedge. Also, the A-signs at the beginnings of lines tends to slant to the right (see, e. g., the first signs in obv., ll. 5, 8 and 12 in *Figure 5*).

na: This sign is clearly visible. Only the head of the first horizontal wedge is not easy to identify. Its shape corresponds to the NA-sign on the obverse in the middle of l. 5. Together with the preceding 'A', it forms

[20] See Salonen (1967, 62–63); Mynářová (2007, 117–118).

the Akk. preposition *ana*, 'to', 'for', introducing the letter's recipient.

ᵎ: The so-called *Personenkeil* is also clearly visible, even if its head might have broken off. This is not easy to decide because the break above the line starts right there and might coincide with the wedge's head. This Akkadian determinative typically characterizes the following personal name as masculine.

⌜*na*⌝: This sign begins the pharaoh's name and is not easy to identify due to the damage. There is a big break above this and the preceding sign, which looks like a wedge but presumably is not. Unfortunately, this break complicates the reading. If the remaining traces represent a NA-sign, it would have a remarkably different shape in comparison with the first 'NA' in the same line and elsewhere in the text. The two *Winkelhaken*s are too far removed from the vertical wedge. Also, in other instances of 'NA' in EA 16, they seem to look more like oblique wedges (compare the first 'NA' in the same line). Nevertheless, the author reads here "⌜*na*⌝", because there does not seem to be a better reading for now.

⌜*ap*⌝: There is a very big hole at the beginning of this sign, which seems to damage almost everything of its first part. However, there are some traces remaining. Starting from the right, a vertical wedge can be clearly identified. In front of it there seem to be traces of two

Figure 1a. EA 16, upper lines including line 1 (Cairo SR 4/12209; photo: Prof. Dr Jana Mynářová, Karls-University of Prague, courtesy of the Egyptian Museum in Cairo).

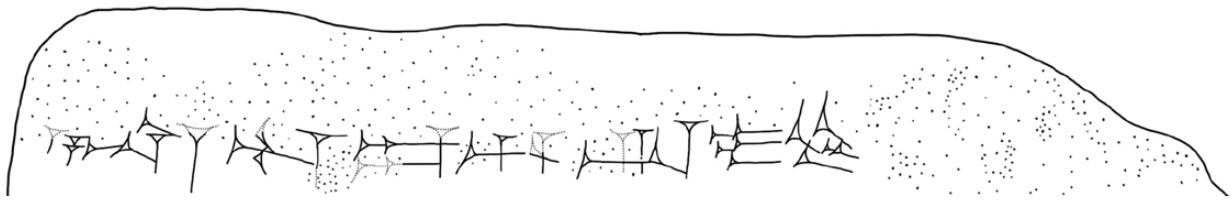

Figure 1b-a. EA 16, line 1 as drawn by the author.

Figure 1b-b. EA 16, line 1 as drawn by L. Abel (line-drawing after Abel in Winckler 1889).

parallel horizontal wedges, with the head of the upper one still visible. In front of this head, a stroke seems to indicate a second horizontal wedge on the same level. Due to the hole, it is not clear whether there is also a second horizontal wedge in front of the lower one, as would be expected for '*ap*'. Unfortunately, there is no other AB-sign (read *ab/ap*) in EA 16 with which the present sign could be compared. However, it is possible to read 'AB' at this point, so for now, the reading '⌜*ap*⌝' is suggested.

⌜*ḫu*⌝: This sign is also damaged in its upper part. However, a horizontal wedge in the beginning and a *Winkelhaken* at its end are easy to spot. In addition, under the horizontal wedge, the remains of some vertical wedges can be traced both in front of and behind the *Winkelhaken*. That is what one would expect for the sign 'ḪU'. Therefore – and although the upper part is not visible – it is suggested that the sign should be read as '⌜*ḫu*⌝'.

⌜*ri*⌝: In this sign the first horizontal and last vertical wedge as well as the *Winkelhaken* in front of the last vertical wedge are clearly visible. In front of the *Winkelhaken* one would expect for the sign 'RI' two additional vertical wedges crossing the horizontal one. There seem to be some remains of heads directly below the fracture line and also some vertical scratches, which are not very easy to align with these heads. Still, it makes a lot of sense to search for a 'RI' here, since as a theophoric element for the Egyptian god *Ra*[21] the name would have to end in something like '*-ria*'. Therefore '⌜*ri*⌝' must be read here, which is consistent with the visible remains.

i: This sign is clearly visible, and its reading is secure.

ʾa: This sign has been read very differently. Some scholars[22] thought that the remains should be identified as a group of *Winkelhaken*s representing the beginning of the Babylonian form of the sign 'TU'. But why 'TU'? There are several Babylonian signs that begin with such a group. Perhaps the reading *tu* – which, by the way, makes no sense and therefore has to be identified as a mistake and emendated – is only due to the drawing of Abel, which shows much more than the preserved remains of the sign. One may argue that the damage has increased over time but already Knudtzon, about 25 years later, did not see what Abel drew.[23] Artzi wrote: 'Indeed, as EAK testifies, the Royal PN is far less preserved than WA indicates.'[24] However, this is not due to the fact that Abel did a bad job, but rather that the goal of an autograph was different in his day than it is today. At that time, it was important to reproduce the text. Damage and hard-to-read areas were only indicated – if at all. Gordon still read it as 'TU', but Rainey was not convinced of this reading, identifying the remains as 'ḪI', interpreting it as 'IA!' and adding '(TU?)'. However, both Moran and Artzi suggested, reading it as '*ʾa*'.[25] Unfortunately, neither explained his suggestion by presenting his own drawing and comparing it to a fitting reference elsewhere. That should be done at this point. The sign's drawing of the author is as follows (*Figure 2*):

Figure 2. The relevant signs as drawn by the author.

A similar sign can be found in a text written in a Middle Babylonian script from the 15th regnal year of the Kassite king *Kadašman-Turgu*, conventionally dated, *c.* 1266 BC,[26] thus close to the Amarna period of the latter half of the 14th century BC. This text was copied by Clay[27] and photos of the tablet are accessible online.[28] Here the drawing by Clay is compared with the drawing of the same sign by the author as based on the photograph (*Figure 3*).

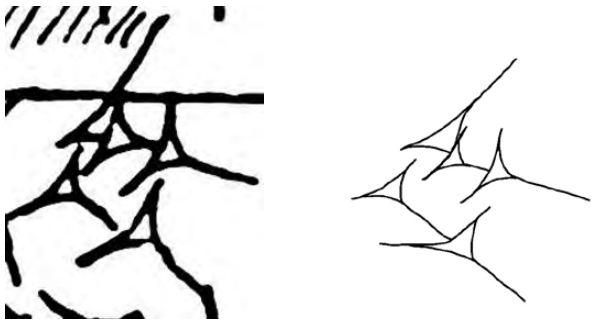

Figure 3. Clay's drawing (after A. T. Clay 1906) as compared with the author's own drawing of the same sign.

The cuneiform sign 'ʾA' can be described as a combination of the signs 'ḪI', containing a cluster of three *Winkelhaken*s and the sign 'ḪAL',[29] containing two consecutive horizontal wedges. By analysing the

[21] In Egyptian transcription: *rʿ*.

[22] Gordon and Rainey, although the latter was unsure about it. Perhaps also Abel, per his hand copy.

[23] See his description of this sign in Knutzon (1915, 127, n. g) and note 7 in this article.

[24] Artzi (1997, 325–326). EAK is Knudtzon (1915) and WA is Abel's drawings in Winckler (1889) according to Artzi's abbreviations.

[25] For the mentioned readings compare page 2 above.

[26] According to Brinkman (1976, 31) Kadašman-Turgu reigned from *c.* 1281–*c.* 1264 BC.

[27] See Clay (1906, Pl. 43, no. 114:17).

[28] https://cdli.ucla.edu/search/archival_view php?ObjectID=P259451 (accessed Feb. 18, 2022).

[29] This refers to the Middle Babylonian sign form found in EA 16. See Labat and Malbran-Labat (2011, 182–183).

alignment of the discussed sign's wedges in EA 16, one can see that the angle of the first three wedges, suggesting a 'ḪI', is slightly different compared to the last two wedges, forming a 'ḪAL' (*Figure 4*):

Figure 4. Alignment of the discussed wedges.

Finally, '*ʾa*' is the only meaningful reading of the sign that does not require an emendation. If one compares the other spellings of *Napḫuriya* in the Amarna texts, they always end in '*-ia*'.[30] However, EA 16 is the only document of Assyrian origin[31] with this name (the Egyptian recipient of EA 15 is not named), and with which other attested writings can only be compared to a limited extent. It must be noted that the glottal plosive at the end of the Egyptian deity name '*Ra*' – transcribed in Egyptian as '*rꜣ*' – is rendered particularly accurately with the sign 'ʾA'. Artzi therefore commented on the suggested reading '*ʾa*': 'This would be an extraordinary exact transliteration of the EDN.'[32] Putting all this together, '*ʾa*' seems to be the best solution for the sign's reading.

[LUGAL GAL]: This restoration is suggested by the conventional letter formulary.[33]

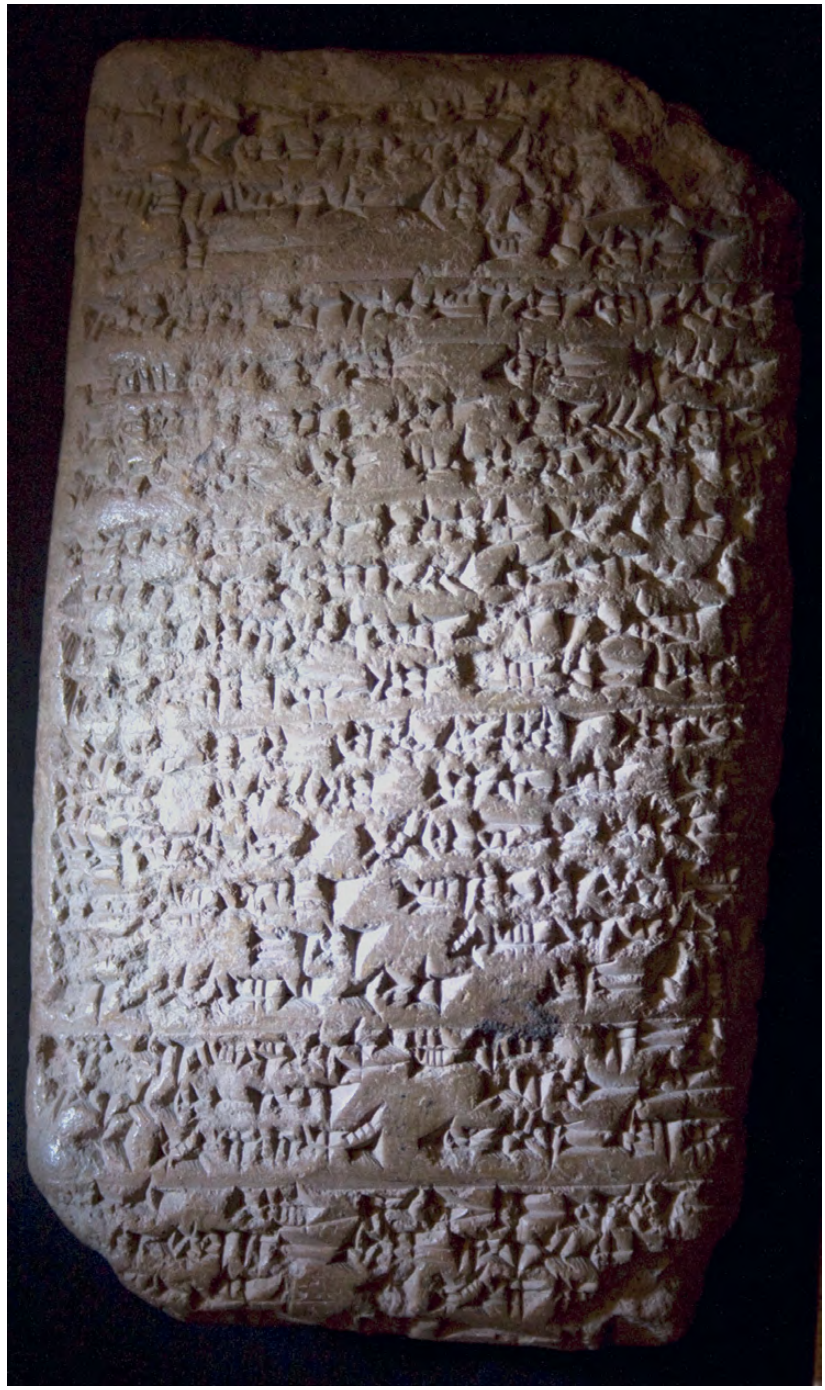

Figure 5. EA 16 recto (Cairo SR 4/12209; photo: Prof. Dr Jana Mynářová, Karls-University of Prague, courtesy of the Egyptian Museum in Cairo).

Conclusion

Although a collation from photographs is never the best option, in this case it is the only one currently possible. The reading '*ʳa*˺-*na*ᴵʳ*na*˺-ʳ*ap*˺-ʳ*ḫu*˺-ʳ*ri*˺-*i*-ʾ*a* [LUGAL GAL]' results from the proviso that the traces recognizable on the available photos were correctly interpreted. Despite the fact that some characters are very difficult to read, this reading is possible, sensible, and therefore the only reasonable suggestion at the moment. To quote Artzi again: 'Consequently, for the time being, until some

[30] See Hess (1993, 115). The only exception is the ending '*-me*' added to '*-ia*'.

[31] This means from the Assyrian realm but, unlike EA 15 (e.g. Moran 1992, xix; Mynářová 2007, 46), not necessarily in the Assyrian language. The Ḫurro-Akkadian dialect of EA 16 is much more complicated and not the subject of this article. Contra Rainey 2015, 1348, who wrongly notes that '*EA* 16 [rather than *EA* 15] is the only one of the Amarna letters that is written in the pure Assyrian dialect)'; see further Wallenfels in this volume ["Middle Assyrian ...Chronology", s.v. *EA 15–16*].

[32] Artzi (1997, 326).

[33] See Mynářová (2007, 125–131).

hard contrary evidence emerges, let us continue with Amenhotep IV!'[34] This statement is still true today.

[34] Artzi (1997, 326).

Photos of EA 16

For the convenience of the reader to follow the arguments put forward in this article, complete photos of the obverse, the bottom line and reverse (*Figures 5*, *6* and *7*) are shown here.

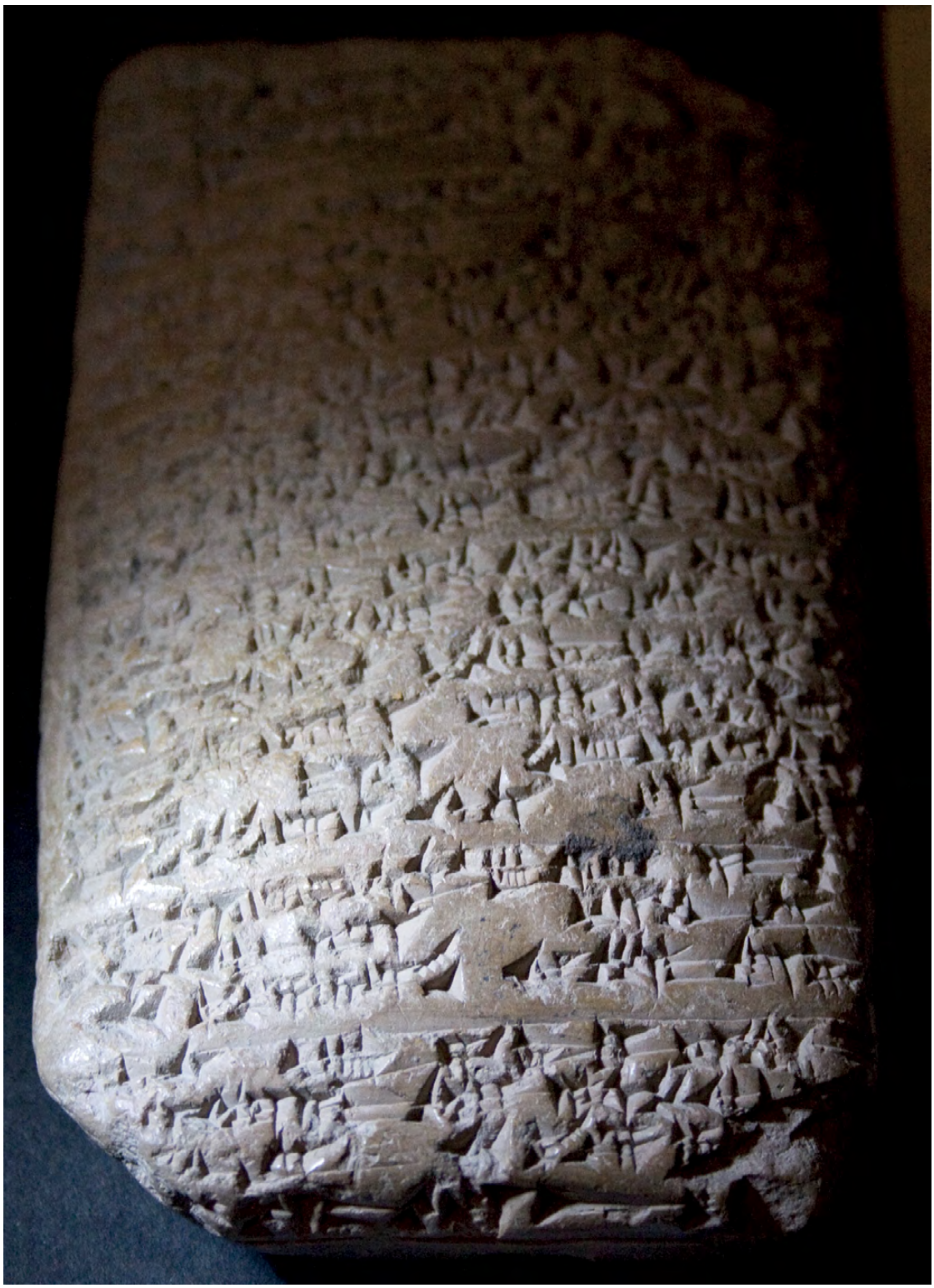

Figure 6. EA 16 recto lower part (Cairo SR 4/12209; photo: Prof. Dr Jana Mynářová, Karls-University of Prague, courtesy of the Egyptian Museum in Cairo).

Figure 7. EA 16 verso (Cairo SR 4/12209; photo: Prof. Dr Jana Mynářová, Karls-University of Prague, courtesy of the Egyptian Museum in Cairo).

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Elamite Dark Age Chronology

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This paper presents the timeline of the Elamite Dark Age, from Nebuchadnezzar I's capture of Susa to the ascent of Huban-nikash I to the Elamite throne. The author equates this Dark Age with the Neo-Elamite I (NE I) period and proposes a further division into NE IA (from c. 1100 BC to c. 950–900 BC) and NE IB (from c. 950–900 BC to c. 750 BC). An alternative perspective on the events concluding the Shutrukid dynasty is discussed in the 'Reconnection' section. Finally, the Addendum examines the plausibility of the Alternative Chronology (CoD concept) in the context of Elam.

Introduction

This paper discusses the timeline and the periodization of the Dark Age in Elam. Throughout the main text, I use the Mesopotamian Middle Chronology (MC). The version compatible with the Alternative Chronology (AC), based on the concept introduced in *Centuries of Darkness* (= *CoD*),[1] is presented in the Addendum.

Author	Timeline	Main criteria
de Miroschedji	*c.* 1000–700 BC	archaeological
Vallat	*c.* 1110–770 BC	archaeological, historic
Malbran-Labat	*c.* 1000–760 BC	archaeological, historic
Gorris and Wick	*c.* 1100–760 BC	archaeological, historic
Waters	*c.* 1000–743 BC	historic
Steve	*c.* 1000–900 BC, IA	linguistic

The Elamite Dark Age spans from the Babylonian attack on Elam in the time of Nebuchadnezzar I (*c.* 1120 BC) to the ascent of Huban-nikash I to the Elamite throne (743 BC).[2] Similar dates are often used to define the first part of the Neo-Elamite Period (NE I). Scholars hold varying opinions regarding the timeframe of the NE I period, depending on the criteria used. De Miroschedji's periodization is based on the results of his excavation in Susa, Steve used the linguistic data, and he is the only one who subdivides NE I into two sub-periods. These opinions are summarized in the table below.[3]

This paper suggests dividing NE I into NE IA and IB, marking the division point at the end of the 10th century BC. Combining political, archaeological and linguistic evidence, I propose the following periodization, with transitional periods on both sides of the Dark Age:

1. The last phase of the Middle Elamite period (ME III) ends with Nebuchadnezzar I's attack on Susa.
2. The first part of the Dark Age, or the Neo-Elamite IA period (NE IA), starts after that attack and ends at the time of decreased activity or gaps in Elamite locations in Susiana and the end of activity in Tal-i Malyan.
3. The second part, NE IB, covers the period between the drastic reduction of activity in Susa and the abandonment of Tal-i Malyan on one side, and Elam's reconnection to Mesopotamian history in the mid-8th century BC on the other.
4. The start of Neo-Elamite II (NE II) is signalled with Elam's re-appearance in Neo-Assyrian and Neo-Babylonian historical texts.

The timeline for the Dark Age is presented on the diagram (*Figure 1*). The vertical axis reflects time, going from the top down (the latest events are at the bottom).

- In the central portion of the diagram, there are three columns corresponding to the timelines of Assyria, Babylonia and Elam. The columns consist of boxes denoting documented kings with their conventional reign dates; the top edge of each box is aligned with the timeline. For clarity, rulers with extant written records are shaded in green. The most important dates and events are marked with

[1] James *et al.* (1993).

[2] Grayson (2000, 71): fifth year of Nabu-nāṣir, i.e. 743/2 BC. We use 743 BC, following Grayson (2000).

[3] De Miroschedji (1982, 215–220), Vallat (2008), Malbran-Labat (1995, 129), Gorris and Wick (2018, 324–325), Waters (2000, 3), Steve (1992, 21–22).

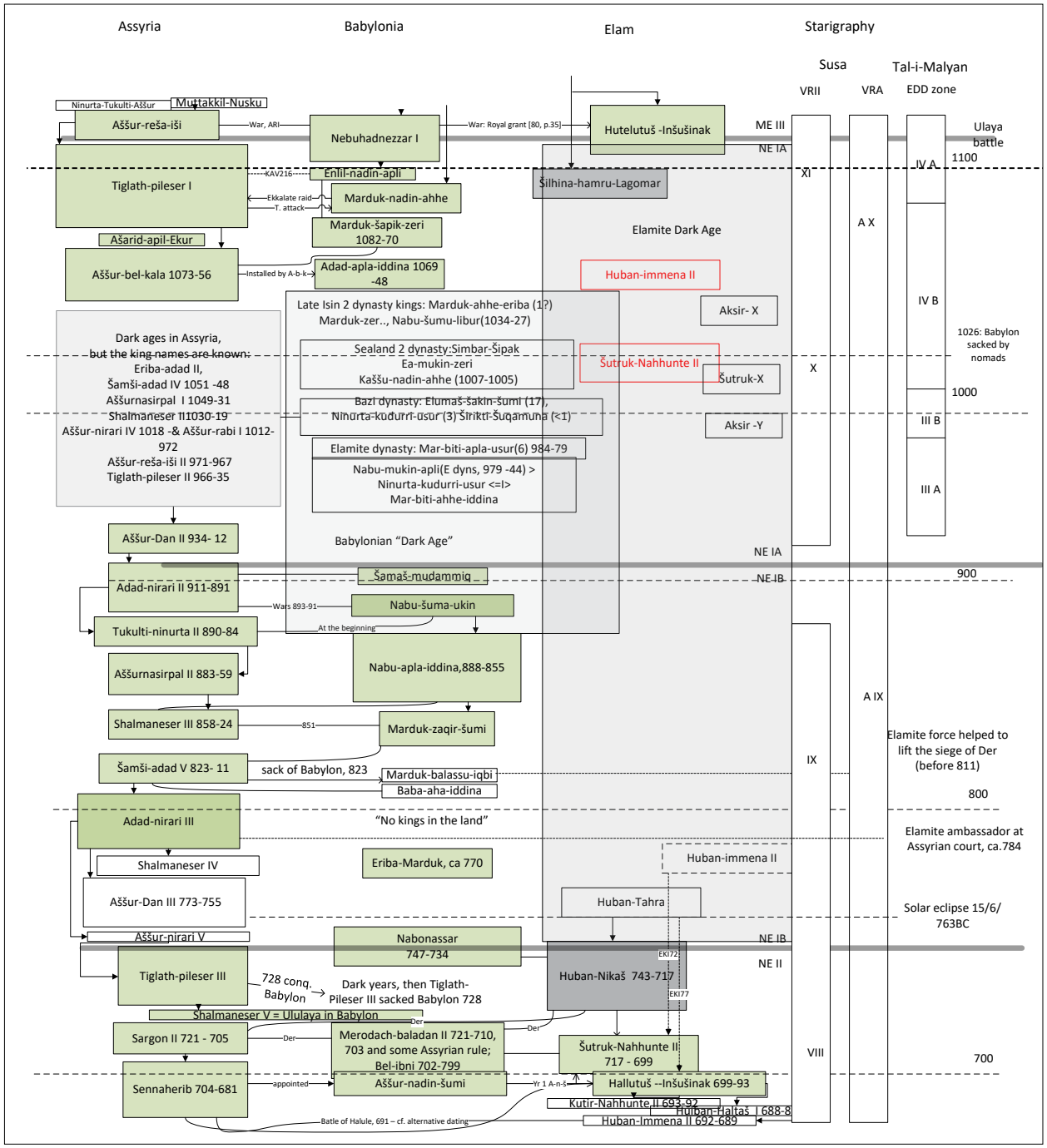

Figure 1. The timeline of the Elamite Dark Age.

horizontal lines. Lines connecting boxes signify established synchronizations between rulers (horizontally) and their familial relationships (vertically). The grey-shaded regions indicate periods of obscurity for Assyria,[4] Babylon,[5] and Elam along their respective columns. Notably, the Dark Age within Elam significantly exceeds comparable periods of uncertainty in Assyria and Babylon.

- The alternative perspective on the events concluding the Shutrukid dynasty is visually highlighted in red ink. According to this

[4] According to Grayson (1991, 113), the Dark Age in Assyria extends from the end of the reign of Aššur-bel-kala (*c.* 1056 BC) to the start of the reign of Aššur-dan II (*c.* 934 BC). Dates for the Assyrian kings' reigns are taken from Grayson (1991; 2000).

[5] Dates for Babylonian kings are taken mostly from Brinkman (1968). We have very little information about Babylonian kings between Adad-apla-iddina (*c.* 1069–1048 BC) and Nabu-apla-iddina (*c.* 888–855 BC); cf. Brinkman (1968, 144–182). [Cf. Discussion 2, elsewhere in these Proceedings, pp. 129–131. Ed.]

viewpoint, Huban-immena II ascended the throne immediately following Shilhina-hamru-Lagamar's reign, followed in succession by Shutruk-Nahhunte II.[6]

- The rightmost column of the diagram portrays stratigraphic sequences from Ville Royale in Susa (VR II and VRA) and Tal-i Malyan. This representation is based upon research by J. de Morgan and P. de Miroschedji in Susa,[7] and with E. Carter's work in Tal-i Malyan.[8] While the layers are not precisely dated, a conspicuous gap emerges in the latter half of the 10th century BC.

Final Stages of the Middle Elamite Period and Nebuchadnezzar I's Campaign

In *c.* 1155 BC the Elamite king Kutir-Nahhunte conquered Babylon, deposed king Enlil-nadin-ahi, terminating the Babylonian Kassite dynasty, and moved the statue of Marduk to Elam.[9] This event had a big impact on the Babylonian collective psyche. Grievances regarding the destruction and a strong desire for the return of the Marduk statue are well represented in later Babylonian texts.[10] The fourth king of the new dynasty, the Second Dynasty of Isin, Nebuchadnezzar I (*c.* 1126–1104 BC), took on Elam to avenge the defeats during Kassite times.

The events of his campaign are recorded in several texts forming the so-called Nebuchadnezzar I's epic.[11] The Babylonian army marched from Der and met the army of the Elamite king Hutelutush-Inshushinak at the Ulaya River near Susa. Elam was utterly defeated. [12] Nebuchadnezzar I concluded in his report to the Babylonians: 'he (the king of Elam) abandoned his strong cities... and disappeared'.[13] Nebuchadnezzar I 'achieved considerable fame in his own time and with later generations'[14] by eliminating the Elamite danger and returning the Marduk statue to Babylon.

The battle at the Ulaya River is not dated precisely. It was preceded by a failed attempt,[15] and therefore it happened no earlier than in the second year of Nebuchadnezzar I's reign (*c.* 1125 BC). The astrological omen text[16] tells that Marduk would stay in Elam for 30 years before returning to Babylon. The statue was most likely taken to Susa during the Kutir-Nahhunte campaign against Enlil-nadin-ahi in *c.* 1155 BC,[17] and therefore *c.* 1125 BC looks like a possible date of the return.

The text of the Hinke *kudurru*, [18] 'He (Enlil) broke the weapon of (Nebuchadnezzar I's) enemy and placed in his hands the sceptre of his enemy', most likely refers to the victory in Elam. The *kudurru* is dated to the sixteenth year of Nebuchadnezzar I's reign (*c.* 1110 BC) and provides a *terminus ante quem* for the event. Considering these factors, it is plausible to date Nebuchadnezzar I's attack to *c.* 1120 BC.

Neo-Elamite 1A Period

The campaign is traditionally considered to be a crushing blow to the Elamites and signals the end of the Middle Elamite 'imperial' period. From the chronological point of view, it is the last synchronism between Elamite and Mesopotamian rulers for the next 370 years. There are no royal inscriptions in Susa after Hutelutush-Inshushinak until the time of Shutruk-Nahhunte II.[19] Mesopotamian sources (chronicles, royal inscriptions) do not mention Elam and Elamite rulers for the next 300 years, until 814 BC. This is one of the most obscure periods in the history of Elam, with almost no written documents and badly disturbed archaeological material.

The number of Elamite settlements in Susiana had already begun to decline from *c.* 1350 BC.[20] Surveys show significant depopulation on the western part of the Susiana plain and migration of people to the east of the Dez river. Indeed, by *c.* 1000 BC, over half of the twenty-one Neo-Elamite I sites on the northern Susiana plain were new foundations, and ten of the thirteen new sites were on the East side of the Dez river.[21] New sites are often the result of fragmentation of Middle Elamite settlements such as Deh-e Now and Choga Zanbil (Dur-Untash).[22] The depopulation of the Susiana plain is illustrated graphically in *Figure 2*, used with the kind permission of P. de Miroschedji from his paper.[23]

On the other side of the Dark Age, Elam reemerges as a unified nation, strong enough to oppose the Assyrians, with new urban centres – Madaktu and Hidalu –

[6] This approach was proposed by Quintana (2004, 2021) based on the inscription *EKI* 72. Its pros and cons were analyzed in detail in Khramov (2023).

[7] De Miroschedji (1978, 215–220), Potts (2016), FARS (2005).

[8] Carter (1996, 51–53), Potts (2016), FARS (2005).

[9] Frame (1995, 19–20), RIMB 2, B.2.4.6.

[10] For example, RIMB 2, B.2.4.5.

[11] RIMB 2, B.2.4.5–B.2.4.10.

[12] RIMB 2, B.2.4.11.

[13] RIMB 2, B.2.4.7.

[14] Brinkman (1968, 192).

[15] Described in RIMB 2, 2.4.6.

[16] See the text and explanations in Brinkman (1968, 108, n. 595).

[17] Brinkman (1968, 89).

[18] Nielsen (2018, 55) II. i 14–i 15.

[19] Cf. the gap between *EKI* 65 (Hutelutush-Inshushinak inscriptions) and *EKI* 71 (Shutruk-Nahhunte II).

[20] Carter (2007, 143).

[21] Carter (2007, 143–144).

[22] Carter (2007, 144).

[23] De Miroschedji (1990, Fig. 3).

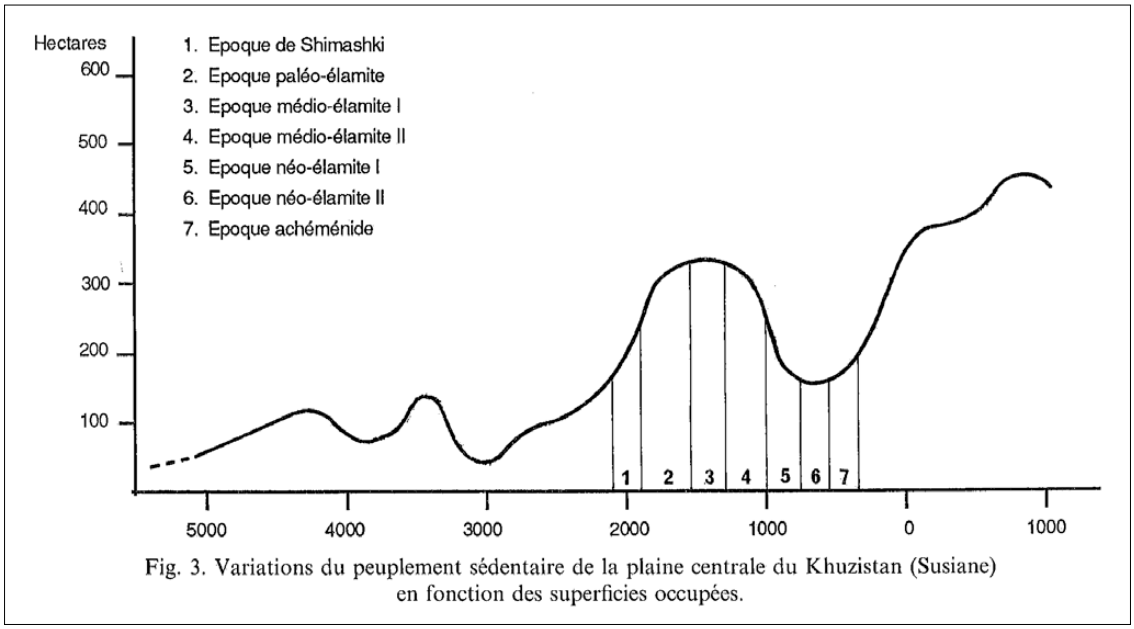

Fig. 3. Variations du peuplement sédentaire de la plaine centrale du Khuzistan (Susiane) en fonction des superficies occupées.

Figure 2. Combined area of Elamite active sites in Susiana. (After de Miroschedji 1990, Fig. 3.)

replacing the old cities of Susa and Anshan. It seems therefore natural to look for a turning point during the Dark Age, and the second half of the 10th century – one [of the lowest and the most obscure times in Elamite history – is an appropriate date for it. The period from *c.* 1120 BC to *c.* 950–900 BC is called here 'Neo-Elamite IA' (NE IA), following Steve.[24] It is defined by the near absence of Elam or Elamites in Babylonian and Assyrian sources, significant depopulation of the western part of the Susiana plain and the migration of people towards the east of the Dez River.[25]

The Dark Age activity was detected in several Susa locations (Acropolis, Ville Royal II and Apadana). The systematic research of the Neo-Elamite stratum was done by de Miroschedji during his excavation at Ville Royal II (VR II on the *Figure1* diagram).[26] He found 13 levels with a hiatus of uncertain duration between level 10 and levels 9 and 8.[27] Elongated goblets (so-called Elamite goblets) are considered to be the most characteristic form for the 10th–12th centuries BC in Elam. At VR II, they first appear in level 13, reach the peak in level 11 (35% of all wares), and are still present (though less prominently, 14%) in level 10.[28] The dating of *c.* 1100–950 BC[29] for these levels is confirmed by similar wares from EDD level IVA from Tal-i Malyan.[30]

The situation is different in levels 8 and 9: Elamite goblets disappear, red-brown pots and glazed pots become prominent.[31] These levels likely belong to the time between 900 and 750 BC.

In the excavations in Ville Royal IA (VRA on *Figure 1* diagram) this period is represented by Levels X and IX. There is an identifiable break between Levels XI and X, but then Level X and IX are homogeneous from the beginning to the end of the Dark Age.[32] These levels have been dated as old as *c.* 1100 BC,[33] or as late as 6th century BC.[34]

Tal-i Malyan layers (see below) could be aligned with the Susa archaeological material, too: EDD 2 (IV A) is parallel with VR II 11; EDD IIIB/ IV A could be compared with VR II level 10.[35] However, the site of Tal-i Malyan was completely abandoned by the middle of the *c.* 10th century BC at the latest. Therefore, the second half of 10th century BC is marked by a decrease of activities and then even hiatuses in most Elamite locations.

Most researchers now suggest that Hutelutush-Inshushinak survived Nebuchadnezzar I's raid and withdrew eastward. The most probable destination is Anshan, modern Tal-i Malyan, the old Elamite Highland capital. During excavations in the 1970s and in 2004, numerous brick fragments with Hutelutush-Inshushinak inscriptions were found on the surface inside the city walls.[36] Additional bricks were found

[24] Steve (1992, 11–12).

[25] See de Miroschedji (1990, 58–60), Carter (2007, 140–145).

[26] De Miroschedji (1978).

[27] De Miroschedji (1978, 225).

[28] De Miroschedji (1978, 225).

[29] De Miroschedji (1978, 226).

[30] Carter and Stolper (1984, 164).

[31] Harper *et al.* (1992, 203).

[32] Steve *et al.* (1980, 76–77).

[33] Steve *et al.* (1980, 78).

[34] See de Miroschedji (1978, 215).

[35] Carter and Stolper (1984, 164).

[36] Carter (1996, 2), FARS (2005, 7–8).

in secondary and tertiary contexts in the EDD sector excavations,[37] showing that Hutelutush-Inshushinak must have built extensively at the site.[38]

The circumstantial evidence suggests that Shilhina-hamru-Lagamar, a younger brother of Hutelutush-Inshushinak, reigned after him in Elam. He is mentioned in several Shilhak-Inshushinak inscription as one of his sons.[39] There are no inscriptions belonging to Shilhina-hamru-Lagamar, and he is not credited with any building activity. However, his presence is recorded on the stele *EKI* 72, which contains the following text:[40]

> I, Šutruk-Nahhunte son of Huban-immena, beloved servant of the Great God and Inšušinak, for *sugir* Hutelutuš-Inšušinak, *sugir* Šilhina-hamru-Lagamar, *sugir* Huban-immena – three kings in total (PAP 3 *su-un-kip*) ....

Based on this evidence, Quintana proposed that Huban-immena II ascended the throne immediately after Shilhina-hamru-Lagamar's reign, followed in succession by his son Shutruk-Nahhunte II.[41] Therefore, Hutelutush-Inshushinak, Shilhina-hamru-Lagamar, Huban-immena II and Shutruk-Nahhunte II reigned in direct sequence. The end of Hutelutush-Inshushinak's rule could be dated approximately to *c.* 1100 BC.[42] His brother Shilhina-hamru-Lagamar ruled, perhaps, another 10–15 years. There is no data about the start date or the length of Huban-immena II's reign, but it is reasonable to assume that he reigned during the 11th century BC. Importantly, this suggestion requires that his son Shutruk-Nahhunte II is a different person from the king mentioned in Mesopotamian chronicles.

Frequent raids of Suteans, Arameans and later Chaldeans led to the depopulation of the Susiana plain, and dramatically decreased the communication between Elamites and Babylonians. Babylonia was even more affected by those raids and by growing Assyrian pressure from the north. Marduk-nadin-ahhe, Nebuchadnezzar I's younger brother (*c.* 1099–1082 BC), was defeated by the Assyrian Tiglath-pileser I and soon disappeared amidst nomadic raids.[43] After those raids, Babylonian history becomes less certain: there are very few contemporary records or inscriptions, and even names of kings are restored mostly from later chronicles.

Among them, there was the king Mar-biti-apla-uṣur (*c.* 984–979 BC),[44] apparently of Elamite descent. He is referred as ŠÁ.BAL.BAL Ù(?) [*El*]*amti*^{ki} in Dynastic Chronicle V.13.[45] Brinkman translates this as 'remote(?) descendant of Elam',[46] and notes that 'it is unlikely that an Elamite king would have such a Semitic name' and that he 'does not seem to have been regarded as a foreign oppressor by later ages'.

The most significant evidence for the NE IA language is found in Tal-i Malyan (ancient Anshan) in the Elamite South-East (modern Iranian Fars province). The excavated monumental building there (EDD) served as a palace or a royal temple.[47] Four levels belong to the period between *c.* 1100 and 950 BC: IVA and B, and level III A and B. More than 500 tablets with text, mainly in Elamite, were found in these levels.

The analysis of the texts on these tablets was done by Stolper[48] and Steve.[49] According to Steve, the language there is different from the language of Elamite royal inscriptions of Kutir-Nahhunte, Shilhak-Inshushinak and Hutelutush-Inshushinak and is closer to the 'classic Neo-Elamite'. Both researchers note the increased use of logograms (60 vs. 31 for Shutrukid texts, some of them used for the first time in Elam), the first use of some syllabic signs, including *šá* sign in parallel with *ša*, some new logograms, and a wider use of broken spellings (i.e. CV_1-V_2C).[50] The use of logograms for standard Babylonian month names is attested in Tal-i Malyan for the first time,[51] and Basello suggested that it had been 'introduced as a consequence of the cruel capture of Susa by Nebuchadnezzar'.[52] These linguistical and grammatical changes in the language, and paleographical changes in the script, were used by Steve as an additional argument to consider the Tal-i Malyan period as a part of Neo-Elamite.

There are two tablets that contain the names of otherwise unknown Elamite rulers. M-603 is a record of an outlay of some commodity; on lines (8) and (9), there are records:[53]

(8) [10]+5 *su-gir šu-ut-ruk ša* [x]

(9) 6 *su-gir ak-šir* KI+MIN *ša* [x]

It is unlikely that this record is referencing Shutruk-Nahhunte since there is a clear *ša* sign after the name,

[37] EDD sector is a part of the Tal-i Malyan site excavated by University of Pennsylvania expedition in 1972–1976, see Carter (1996, 2).

[38] Carter (1996, 16).

[39] *EKI* 40, 41, 45, 46, 47, 54, 59.

[40] Translation after Malbran-Labat (1995, 135) with minor corrections.

[41] Proposed in Quintana (2004, 2021). Its pros and cons were analysed in detail in Khramov (2023).

[42] Roaf (2017, 99).

[43] Brinkman (1968, 121–123).

[44] Grayson (2000, 143) – *ABC* 18 v 13.

[45] Grayson (2000, 14).

[46] Brinkman (1968, 165).

[47] Carter (1996, 15).

[48] Stolper (1984, 19–21).

[49] Steve (1992, 12–13, 36–39).

[50] Stolper (1984, 20).

[51] Basello (2000, 18).

[52] Basello (2000, 25).

[53] Stolper (2013, 408).

despite Quintana's[54] suggestions. KI+MIN on the line 9 means 'ditto', 'the same'.[55] It was found above IVA, in a bin in the level IIIA. Stolper believes it is not much younger than IVA administrative tablets (certainly less than 100 years), so it could be dated to the 10th or 11th centuries BC.

M1157 apparently contains a distribution order, and at the end of the tablet there is a line:[56] (17) *sugir ak-šir*-[y]. It was found in a IVA context, in an EDD building dated preliminarily to the *c.* 11th century BC. Elamite *sugir* certainly means 'king' and is just a different spelling for *sunki*, used in most Elamite royal inscriptions.[57] In *EKI* 4C, it is used as a variant of *su-un-ki-ik* (of Anshan and Susa). In *EKI* 72, it is used as a title for three previous kings; together they are addressed with the plural *su-un-kip* (variant *su-kip*). Therefore, there are three new characters for the Elamite Dark Age: Shutruk-X and kings named Akshir-X and Akshir-Y. These tablets might either refer to some local chieftains or the Anshan-based kings of Elam after Hutelutush-Inshushinak. I prefer the 'local rulers' hypothesis for two reasons. First, there are two *sugir*s in M-603, so that they could be rulers of different local areas. The second and most important reason is the complete absence of any geographical names outside the local area in all Tal-i Malyan corpus,[58] suggesting a strictly local sphere of influence.

The EDD area function changed from a monumental building (level IV) to a ceramic production area (level IIIB) and then to a domestic structure (level IIIA).[59] The latest structures on this level are dated before 900 BC. After that, the location was mostly abandoned. The levels above contain only fragments of surfaces on top of older buildings, burials and structures that belong to the Sassanian period *c.* AD 400.[60]

The Tal-i Malyan layers could be aligned with Susa archaeological material: EDD 2 (IV A) is parallel with VR II 11; EDD IIIB/ IV A could be compared with VR II level 10.[61] However, the site of Tal-i Malyan was completely abandoned by the middle of the *c.* 10th century BC at the latest.

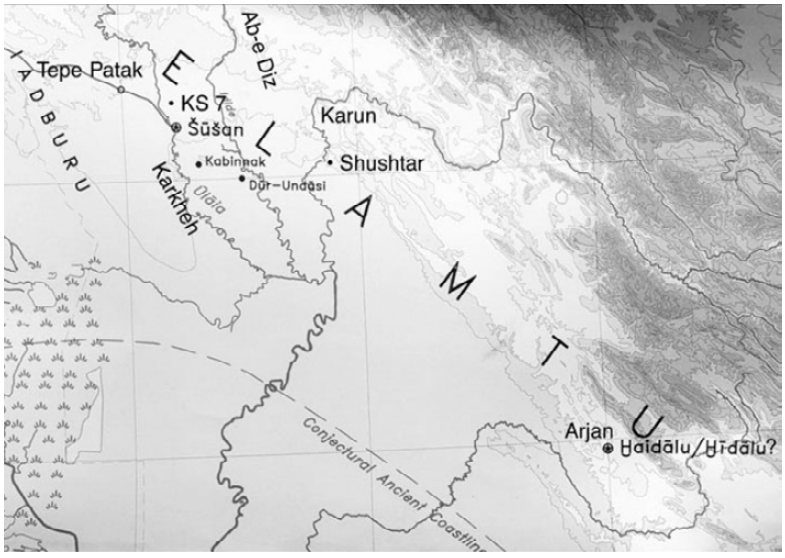

Figure 3. Map of Elam during the Neo-Elamite period (after Potts [2005] with kind permission of the author; originally from Pappola and Porter [2001]).

The second half of the *c.* 10th century BC signifies the lowest point in the Elamite Dark Age. The stratigraphical gaps in Susa, fractalization and contraction of other Susian centres show the process of de-urbanization, increased pastoralism, and possibly tribal growth in Susiana and Khuzestan. At this point, Chaldeans and Arameans moved into Elamite territories along the Ulaya River.[62] The activity in Tal-i Malyan stopped, and there is no sign of sedentary population in the Kur River basin until Achaemenian times.[63] There are only occasional burials in Tal-i Malyan and its surroundings, left by nomadic people.[64] To the southeast, in the territory of Anshan, the Persian tribes had entered Fars at this time.[65]

Neo-Elamite IB Period

The end of the *c.* 10th century is a turning point in the history of Elam. In the north-east, Assyria emerged as an active power under Aššur-dan II (*c.* 934–912 BC), and gradually turned into the dominant player and the main threat to Elam. While there is still a complete absence of the local texts from Elam between *c.* 950 and 750 BC, mentions of Elam begin appearing in Babylonian and Assyrian sources.

The first mention of Elam after the start of the Dark Age is already related to the Assyrians. In 814 BC, Elamite troops delivered military support to the

[54] Quintana (2021).
[55] Stolper (2013, 400).
[56] Stolper (2013, 412).
[57] *ElWb* (1987, 1098).
[58] Stolper (1984, 15).
[59] Carter (1996, 51).
[60] Carter (1996, 56).
[61] Carter and Stolper (1984, 164).
[62] Carter (2007, 140).
[63] Potts (2016, 252).
[64] Potts (2016, 252).
[65] Carter (1994).

Babylonian king Marduk-balassu-iqbi (818–813 BC) against the Assyrian Šamši-Adad V (823–811 BC) in the battle of Dur-Papsukkal.[66] The battle was lost, but the Babylonian king escaped with his life and even with most of his troops. The fact that the people of the neighbouring city of Der fled to Elam suggests that Elam was perceived as a single country.[67] Later, there is a mention of an Elamite ambassador with his staff at the Assyrian court at Nimrud in 784 BC.[68] The letters also mention a wine ration for Elamite court employees in the time of Adad-nirari III (810–783 BC). Finally, a document from the time of the Babylonian king Nabu-šuma-iškun (760–748 BC), mentions him sending his own people to Elam on two occasions,[69] perhaps, in return for military support.

Archaeological evidence of this period in Susa is represented by levels 8 and 9 in Ville Royal II. They likely belong to the time between 900 and 750 BC. Ceramics are represented by new classes: incised square and truncated conic boxes.[70] They also contain vases identical to those found at Choga Zanbil in the latest level before 646 BC. De Miroschedji calls them 'Neo-Elamite classic' and dates them to the 8th–7th centuries BC.[71] However, the presence of the full-hipped naked women figurines and hump-backed bulls with parallels at Kabnak, Dur-Untash and Anshan,[72] makes the dating of 10th–8th centuries BC more likely. Monumental structures for this period are almost completely absent.

Most activity during NE IB was happening outside of the old Elam centres. The new cities of Madaktu and Hidalu were playing a prominent role,[73] while Susa became more of a spiritual and cultural centre.[74]

By the end of the Dark Age, Madaktu was the main royal city of Elam.[75] According to de Miroschedji,[76] the most probable location of Madaktu is Tepe-Patak on the Susa-Deh-Loran Road, on the bank of the Dowarij River. He identifies it with Badake on the Ulaya River in the time of the *Diadochoi* wars, described in Book XIX of Diodorus Siculus.[77] De Miroschedji suggests that Diodorus mistook Dowarij for Ulaya. Potts suggested Tape-Senjar (KS-007), north of Susa on Karkeh (ancient Ulaya)[78], as a good candidate for Madaktu. However, the letter from Bel-Ibni to Assurbanipal (ABL 281)[79] tells that the Elamite king Huban-haldash abandoned Madaktu and crossed the Ulaya river, escaping the Assyrians. Thus, Madaktu should be to the west of the Ulaya river. Since KS-007 is on the eastern bank of the Ulaya (see *Figure 3*), I prefer the classical identification of Madaktu with Tepe-Patak.

The second new centre, Hidalu/Hidali, was outside of the reach of Assyrian invasions. It had its own ruler, sometimes attested as a 'junior' or vassal king.[80] Assurbanipal's annals mention that the 'entrance of the land Hidalu' is near Huhnur,[81] modern Tepe-Bormi[82] in the Ram-Hormuz area. It means that Hidalu is either in the eastern part of Ram-Hormuz, or further east in Behbahan. According to Henkelman, 'the imposing site of Tol-e Homayun near the old city of Arja' in Behbahan is a reasonable candidate for Hidalu.[83]

Unfortunately, there have been no official excavations in the potential locations of Madaktu and Hidalu. The material evidence for NE IB comes mainly from outdoor sanctuaries of Malamir in the Izeh Valley and Kurangum, in the mountains of SE Khuzestan. The Malamir outdoor sanctuary has two relief compositions – Kul-e Fara and Šikaft-e Salman. The oldest reliefs in Šikaft-e Salman can be dated back to the *c.* 12th century BC,[84] but the earliest relief at Kul-e Fara (KF IV) was most likely carved in the 9th century BC, right during the Dark Age.[85] Dating the reliefs is notoriously difficult and is done based on stylistic parallels, composition, garments of figures, etc. The inscriptions provide the *terminus ante quem* for all reliefs. Both groups of reliefs have inscriptions of Hanni, a local leader (*kurtu*), dated to the 7th or 6th century BC.[86] Interestingly, in his inscriptions, Hanni declares the allegiance to his lord, king Shutur-Nahhunte, son of Indada.[87]

[66] RIMA 3, A.0.103.1, iv 38, Gorris and Wick (2018, 326).

[67] SAA 3 41: 5–18, http://oracc.museum.upenn.edu/saao/corpus.

[68] CTN 3.145, iv 13, iv 26, Gorris and Wick (2018, 326).

[69] Glassner (2004, 307, 309) – 'Document concerning Nabu-šuma-iškun'.

[70] Carter and Stolper (1984, 182), de Miroschedji (1978, 227).

[71] De Miroschedji (1978, 227).

[72] Carter and Stolper (1984, 257, Fig. 11).

[73] Wick (2019, 2).

[74] Carter (1994, 74), Stolper (1984, 47).

[75] Potts (2016, 263).

[76] De Miroschedji (1986, 213–215).

[77] Potts (2005, 168–169).

[78] Potts (2005, 170–171).

[79] Translation of this letter in Gerardi (1987, 211), cf. also Potts (2001, 15).

[80] Cf. two client kings, installed by Assurbanipal after the victory over Elam: one in Madaktu and Susa, and another in Hidali – *Encyclopaedia Iranica*, s.v., Hidali (https://iranicaonline.org/articles/hidali).

[81] Wick (2019, 4, n. 8).

[82] Mofidi-Nasrabadi (2018, 115–118).

[83] Henkelman (2017, 07).

[84] Gorris and Wick (2018, 342), Alvarez-Mon (2020, 463).

[85] Alvarez-Mon (2015, 2–4).

[86] Potts (2016, 296).

[87] Tavernier (2004, 41).

Reconnection

By the middle of the 8th century, Elam re-connects to the World scene, and its rulers appear again in the Mesopotamian annals. It emerges as a centralized polity, strong enough to challenge the Assyrians. Most scholars consider this time as the start of the Neo-Elamite II period.[88] In Susa, temple construction activity resumes, royal inscriptions re-appear, new buildings are detected in Choga Zanbil; Elamite kings actively interact with Assyrians and Babylonians. However, our understanding of the period is based on two separate accounts running in parallel: one set of data is based on Mesopotamian chronicles and annals, while the other is based on Elamite royal inscriptions.

The first mention of Elam after the Dark Age is found in Babylonian Chronicle 1 during the reign of Nabu-naṣir (743 BC):[89]

> (i 9–i 10) in the fifth year of Nabu-naṣir, Huban-nikash (I) took the throne in Elam.

The Chronicle then mentions the battle against Sargon II, king of Assyria, near the city of Der:

> (i 33) In the second year of Marduk-apla-iddina (II) Huban-nikash (I), king of Elam
>
> (i 34) did battle against Sargon (II), king of Assyria, in the Der area,
>
> (i 35) he effected an Assyrian retreat, (and) inflicted a major defeat upon them.
>
> ...
>
> (i 38) In the fifth year of Marduk-apla-iddina (II), king Huban-nikash died.
>
> (i 39) for [26] years Huban-nikash ruled Elam.

After the death of Huban-nikash I, the next king, called Ishtar-hundu in the Chronicle, ruled in Elam during the entire time of the 'Sargonid war' and the first years of Sennacherib of Assyria:[90]

> (i 40) [Ishtar-hund]u, son of his (Huban-nikash I) sister, took the throne
>
> ...
>
> (ii 32) The first year of Aššur-nadin-šumi: Ishtar-hundi, king of Elam
>
> (ii 33) was seized by Hallushu (Elamite Hallutush-Inshushinak?)
>
> (ii 34) For eighteen years Ishtar-hundu ruled Elam.
>
> (ii 35) Hallushu, his brother, ascended to the throne of Elam.

The text therefore dates the reign of Ishtar-hundu to *c.* 717–699 BC and allows him to be equated with Shutur-Nahundi mentioned in Assyrian king annals. In the Sargon (Khorsabad) annals, the Elamite king in the description of the campaigns around 710 BC is called *šu-tur-na-ḫu-un-di,*[91] in the Sennacherib annals from the first Elamite campaign (704 BC), the Elamite king is called: *šu-túr-*^{d}*na-ḫu-du.*[92]

On the ground, no royal inscription of Huban-nikash I has been found, but several inscriptions of a king (or kings) with the name Shutur- and Shutruk-Nahhunte were discovered during excavations in Elam. The list below contains some of them.

1. *EKI* 71 A+B – inscriptions on alabaster horns and bricks found close to the Pinigir temple on Susa Acropolis. The first line reads as: *ú* ^{m}*šu-tur-*^{d}PÍR, *šá-ak* ^{md}*ḫu-ban-im-me-na-gi-ik-ki ....*: 'I, Shutur-Nahhunte, son of Huban-immena ...'.[93]
2. Sb 6708[94] – a faience knob from Susa Acropolis: *ú* ^{m}*šu-tur-* ^{d}PÍR *šá-a*[*k* ^{md}*ḫu-b*]*an-im-me-na-k*[*i* EŠŠANA] ^{ÁŠ}*an-za-an* ^{ÁŠ}*su-*[*sun...*]: 'I, Shutur-Nahhunte, son of Huban-immena, king of Anshan and Susa'.[95]
3. *EKI* 72 (= *IRS* 57) – three clay slabs from Susa containing the text: *ú* ^{m}*šu-ut-ru-uk* ^{d}PÍR *ša-ak* ^{md}*ḫu-ban-im-me-na-gi-ik-ki...*: 'I, Shutruk-Nahhunte son of Huban-immena ...'.[96]
4. *EKI* 73 A, B, C – bricks from a stele from Susa: ^{m}*šu-ut-ru-uk-*^{d}PÍR *šá-ak·* ^{md}*ḫu-ban-im-me-na-gi-ik-ki...*: 'Shuturuk-Nahhunte, son of Huban-immena...'.
5. *EKI* 74 – *Shutruru* stele, possibly a land grant.[97] The king is mentioned as *su-gir* ^{m}*šu-ut-ru-uk-*^{d}*nah-ḫu-un-te* on the reverse line 3 (restored) and on the lines 12–13, and then without the title: ^{m}*šu-ut-ru-uk-*^{d}*naḫ-ḫu-un-te* on the line 18.
6. *EKI* 75 – Hanni inscriptions from Malamir, line 14: SUNKIR ^{m}*šu-tur-*^{d}PÍR *šá-ak in-da-da-ri-na...*: 'King Shutur-Nahhunte, son of Indada...'.

V. Scheil, the initial publisher of the inscriptions (1) and (3)–(5),[98] attributed all of them to the same king from the Assyrian annals. The names Shutruk- and Shutur-Nahhunte were deemed to be close enough to be accepted as variants, especially since they used the same patronym (son of Huban-immena). This was widely accepted by the research community until Lambert[99]

[88] Potts (2016, 250–251), Gorris and Wick (2018, 325).

[89] Grayson (2000, 71–74) – *ABC* 1, i 9–10.

[90] Grayson (2000, 75, 77) – *ABC* 1, i–ii.

[91] RINAP 2, 1: 333.

[92] RINAP 3/1, 1: 7.

[93] König (1965, 146).

[94] Sb 6708 – Louvre collection number.

[95] Amiet (1967, 36).

[96] Transliteration and translation from Malbran-Labat (1995, 135).

[97] Henkelman (2013, 372).

[98] Scheil (1904, nos. 84–86).

[99] Lambert (1967, 48).

pointed out that Shutur and Shutruk are structurally different names. But right away, Amiet expressed some reservations: 'one can think of a brother, predecessor, or coregent, because in any case, the two characters must be considered as substantially contemporary, if they do not merge. The discovery of the pommel-figurine inscribed in the immediate vicinity of the enamel temple suggests that both participated in the embellishment of this building'.[100] Ultimately, the question becomes: what is more probable – a variation in a single king's name or two brother kings with names that are so similar.

The difference between the names Shutur and Shutruk is discussed in detail by Waters.[101] He shows that, contrary to the claim by Grillot,[102] they share the same root. The spelling *šu-tur-uk* (instead of *šu-ut-ru-uk*) is attested in PN *ḫu-ban-šu-tur-uk* on the Ururu bronze tablet.[103] The shift from harmonic spelling (as in *šu-ut-*) to abrupt spelling (as in *šu-tur-*) in Elamite is a known process and could explain the change.[104] Variations in king names' spellings are known in Elamite: for example, the Elamite Onomasticon registers the form *ki-tin-*^{d}*ḫu-ut-ru-taš* and assumes that it addresses the same person as *ki-tin-*^{d}*ḫu-ut-ra-an*,[105] whereas there is no reliable example in Elamite sources of two brothers named with similar names. Of course, the possibility cannot be ruled out, but with the data currently available, the option of merging two names into one seems preferrable.

EKI 72 contains the following text:[106]

> I, Shutruk-Nahhunte son of Huban-immena, beloved servant of the Great God and Inshushinak, for *sugir* Hutelutush-Inshushinak, *sugir* Shilhina-hamru-Lagamar, *sugir* Huban-immena – three kings in total (PAP 3 *su-un-kip*).

Hutelutush-Inshushinak and Shilhina-hamru-Lagamar are well attested at the end of the 12th and in the 11th century BC. If Shutruk-Nahhunte in *EKI* 72 is the king mentioned in Mesopotamian documents, then Huban-immena II, his father, should be active no earlier than middle of 8th century BC. It would require a break of about 250 years between the first two kings and Huban-immena II. Quintana suggested,[107] based on *EKI* 72, that Shilhina-hamru-Lagamar ruled right after Hutelutush-Inshushinak, and then introduced three additional Elamite kings right after him – Huban-immena II, Shutur-Nahhunte and Shutruk-Nahhunte II – at the end of the 11th and the beginning of the 10th century BC. He attributes Elamite inscriptions (1)–(5) to these kings. I analyzed this proposal in a separate study,[108] and showed that it is feasible. However, instead of two kings, I assumed that there was one king, Shutruk-Nahhunte, while Shutur-Nahhunte was just a variation of that name. His reign could be dated to the second half of the 11th or even the beginning of the 10th century BC. Then the king mentioned in Sargon's Annals and Babylonian Chronicle, Shutur-Nahhunte, who ruled in 717–699 BC, should be a different person, unattested in the Elamite records.[109]

The next king, Hallushu, overthrew Shutur-Nahhunte and seized the throne in the first year of Aššur-nadin-šumi.[110] On the Elamite site, there were several copies of the inscription of the king Hallutush-Inshushinak, son of Huban-tahra (*EKI* 77). Traditionally, he was considered the same person as Hallushu,[111] since Hallushu could be a hypocoristic form for Hallutush-Inshushinak, as it is shown by Tavernier.[112] This identification is questioned[113] on the base of numerous and profound changes in the Elamite language between inscriptions of Shutruk-Inshushinak II (*EKI* 71–74) and Hallutush-Inshushinak (*EKI* 77).[114]

However, if *EKI* 71–74 are re-dated to the *c.* 10th century BC, as proposed above, there are no problems with timing *EKI* 77 at beginning of the 7th century BC. Therefore, Hallushu of the Babylonian Chronicle 1 and Hallutush-Inshushinak from *EKI* 77 could be the same person. This identification re-establishes the synchronism between Mesopotamia and Elam at the end of the Dark Age.

Conclusions

The Elamite Dark Age starts at the time of the Ulaya battle. This event concludes the Middle Elamite 'imperial' epoch and starts the Neo-Elamite (NE) period. Its first phase, NE I, coincides with the Dark Age in Elam. It can be split into two parts – NE IA and NE IB.

[100] Amiet (1967, 37).

[101] Waters (2000, 113–114).

[102] Grillot (1984, 188).

[103] Tavernier (2004, 37).

[104] Stolper (1984, 20).

[105] Zadok (1984, 15).

[106] Translation from Malbran-Labat (1995, 135) with minor corrections.

[107] Quintana (2021, 64).

[108] Khramov (2023).

[109] Quintana (2021, 4, 16).

[110] See Grayson (2000, 75, 77).

[111] Cf. König (1965, 168, n. 10), Malbran-Labat (1995, 129), Stolper (1984, 47).

[112] Tavernier (2014, 62).

[113] Vallat (1995), Tavernier (2004, 14; 2014, 61).

[114] Malbran-Labat (1995, 137) – use of an ideogram for 'Inšušinak'; Tavernier (2004, 5–6): use of GAM/BE as a determinative for masculine person, use of *-h* in the locative suffix instead of *-k-*, etc.

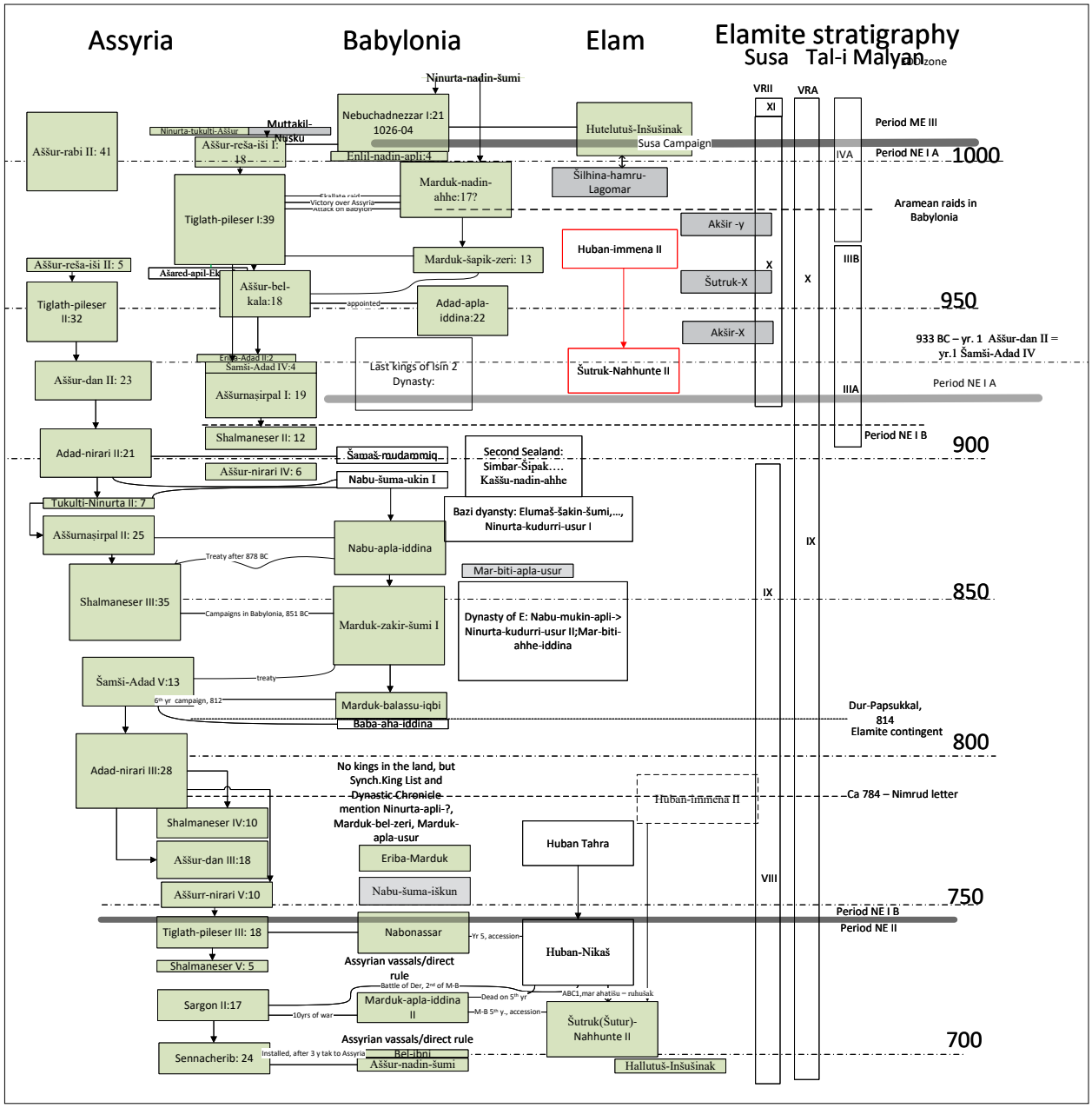

Figure 4. Alternative Dark Age reconstruction

The first part, NE IA is characterized by significant depopulation of the western part of the Susiana plain and the migration of people towards the east of the Dez River.[115] The activity in Tal-i-Malyan fades by the end of the 10th century BC, and the activity in Susiana is significantly decreased. At this point the NE IA period ends, and NE IB begins.

New cities, Madaktu and Hidalu, became the centres of the political life. The main characteristic of the second half of the Dark Age is the involvement of Assyria in the northeast and the appearance of Iranian tribes on the southeast, in the modern Fars province of Iran. The second part of the Dark Age and the NE IB period ends with the accession of Huban-nikash I to the Elamite throne in 743 BC.

One of the goals of this paper was to investigate the compatibility of the *CoD* approach with Elamite data. The main text uses the traditional Middle Chronology (MC); the version that uses the *CoD* approach is presented in the Addendum. The absence of the synchronisms and the scarcity of the data allows to accommodate both scenarios for that period. This can be seen when comparing the diagrams 1 and 4: while the Assyrian and Babylonian columns are completely different for the traditional MC and the *CoD* approach, the changes in the Elamite column are minimal. On the other hand, the change of the dates for the last Shutrukids in Elam does not affect the Mesopotamian events at all.

[115] See de Miroschedji (1990, 58–60), Carter (2007, 140–145).

Addendum: Alternative Reconstruction

The alternative reconstruction of the Dark Age in Elam is compatible with the chronology based on the concept introduced in *CoD*. It assumes that the Dark Age is mostly the artifact of the data interpretation by historians, who ignore the possibility of simultaneous reigns, repetitions, wrong reign lengths and other potential errors. The result is the ultra-short *CoD* chronology, represented in *Figure 4*.

The Elamite Dark Age spans from the sack of Susa by Nebuchadnezzar I to the ascent of Huban-nikash I to the Elamite throne. The switch to the ultra-short *CoD* chronology does not affect the flow of events in Elam directly, since there are no synchronisms during this period. Both options for the end of the Shutrukids dynasty could be accommodated with the *CoD* approach. The absolute dates are reduced by 110–125 years for the start of the period: the sack of Susa should be dated to *c.* 1000 BC. The subdivision of Neo-Elamite I into NE IA and NE IB remains relevant, and is dated to the end of the 10th century BC.

In the traditional chronology, the timespan for the Dark Age is about 370 years. Accepting of the *CoD* approach cuts this gap to about 250 years, and all this time is taken from NE IA.

The reconstruction of the transition between Middle and Neo-Assyrian periods in the diagram is based on Fig. 2 in R. Wallenfels, 'Middle Assyrian History...', elsewhere in these Proceedings, pp. 63–79. It presents 'an alternative restructuring of the late Middle- and early Neo-Assyrian chronology: yr. 1 (98) Aššur-dan II = 933! BCE = yr. 1 (91) Šamši-Adad IV (*1052 BCE)'. I have added synchronistic links to Babylonian and Elamite kings.

The reconstruction of the Babylonian Dark Ages, compatible with the *CoD* approach, is built using only the documents contemporary to the events: *kudurru*s and Assyrian royal inscriptions.[116] Therefore, it could be considered reliable for the *CoD* approach.

[116] Here are the documents used in the reconstruction:
- Simbar-Šipak inscription, B.3.1.1, RIMB 2, 73: Simbar-Šipak, the first king of the 2nd Sealand Dynasty, mentions in his record that in the time of Adad-apla-iddina Aramaeans destroyed Sippar and took the throne of Enlil. He restored the city and made a new throne.
- *Kudurru BBSt* 9: In year 2 Ninurta-kudurri-uṣur, Arad-Sibitti slew a slave of Buruša, and they went to the court (i: 1–5). Then, in year 5 Nabu-mukin-apli, Arad-Sibitti gave some plots to the daughter of Buruša as dowry and bridal gifts (i: 1–17). In year 25 Nabu-mukin-apli, the deal was confirmed by the son of Arad-Sibitti (i: 18–24). The credit payments between year 2 Ninurta-kudurri-uṣur (I) and year 25 Nabu-mukin-apli are discussed in that record. The same *kudurru* also mentions Ninurta-kudurri-uṣur and Mar-biti-ahhe-iddina, sons of the king Nabu-mukin-apli, as witnesses (col. v: 30–32).
- *Kudurru BBSt* 36 (from Sippar): The stele (i: 13) mentions Simbar-Šipak, king of Babylon; then, at i: 25, it says that during the reign Kaššu-nadin-ahhe the offering to Shamash in Sippar were discontinued. At i: 29, it mentions that Eulmaš-šakin-šumi restored them. Then, it talks about the victory of Nabu-apla-iddina over Suteans and his restoration work (ii: 18–iv: 21).
- Annals of Adad-nirari II: he vanquished Šamaš-mudammiq and conquered his country from Yalman to the Dur-ilani River. (RIMA 2, A.0.99.4, 37–34; A.0.99.2, 26–29).
- Annals of Aššurnaṣirpal II: he captured the cavalrymen of Nabu-apla-iddina, king of Karduniash. Aššurnaṣirpal II was a grandson of Adad-nirari II (RIMA 2, A.0.101.1, i: 29).

Abbreviations

ABC	Grayson 2000
BBSt	King 1912
CoD	James *et al.* 1993
CTN 3	Dalley and Postgate 1984
EKI	König 1965
ElWb	Hinz and Koch 1987
IRS	Malbran-Labat 1995
RIMA 2	Grayson 1991
RIMA 3	Grayson 1996
RIMB 2	Frame 1995
RINAP 2	Frame 2021
RINAP 3/1	Grayson and Novotny 2012
SAA	The State Archives of Assyria (Helsinki)
Susa 1992	Harper *et al.* 1992

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Discussion 2: Assyria and Post-Kassite Chronology

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In the *CoD*-like rearrangement of the period under discussion (see *Figure 1*), the regnal dates of the late Middle Assyrian line of kings, together with the intimately associated post-Kassite Isin II[1] and following Sealand II, Bazi, and Elam Dynasties per the 'Dynastic Chronicle' (*ABC* 18),[2] echoed by the late 'Synchronistic Chronicle' (Assur 14616c),[3] plus the next 3 kings in the conventional chronology of Babylonian kings ('Undetermined or Mixed Dynasties' nos 1–3)[4] per the entitlement monument *BBSt* 9,[5] are lowered by 110–125 years (see elsewhere in these Proceedings, pp. 63–79). This line of post-Kassite Babylonian rulers would now appear to be more or less contemporary with another line of Babylonian kings, nos 4–9, prior to the decades-long interregnum between the deportation of (no. 9) Baba-aha-iddina by the Assyrian king Šamši-Adad V in 811 BCE and the accession to the Babylonian throne by the later Sealand dynast (no. 13) Eriba-Marduk, *c.* 770 BCE.[6] A Late Babylonian clay tablet copy of a now-lost throne inscription of Simbar-Šipak,[7] the founder of the Sealand II Dynasty, evidences his friendly relations with an unnamed Assyrian king at Ashur (*Baltil*) who returned property of Enlil, which must have been looted from one of his temples in Karduniash by an earlier Assyrian king, likely Tiglath-pileser I in any chronology.[8]

In the presently proposed *CoD*-like ultra-low chronology, this benevolent Assyrian king may now be identified as Adad-nirari II (r. 910–891 BCE) whose defeat of the king of Karduniash, Šamaš-mudammiq,[9] presumably in or after 910 but prior to 901 BCE,[10] would have made it possible for Simbar-Šipak to establish at about the same time an independent kingdom based far to the south in the Sealand that would remain, unlike its more northerly Babylonian neighbours in Karduniash, free of Assyrian interference.[11] Having a friendly rebellious Babylonian province to the south of Karduniash would certainly have been in Adad-nirari II's best interests in his wide-ranging war with Šamaš-mudammiq. Further, it would have been advantageous to have a friendly kingdom situated between Karduniash and Elam. If Simbar-Šipak began his 18-year-long reign in or prior to *c.* 901 BCE, his and the following Bazi and Elamite dynasties, which add up to at least 47 years, would roughly parallel the reigns of Nabu-šuma-ukin I and Nabu-apla-iddina in Karduniash, the latter king being a contemporary of a hostile Assyrian king Aššurnaṣirpal II (r. 883–859 BCE). Note Aššurnaṣirpal II's belittling passing reference to 'Sibir (Simbar-Šipak) king of Karduniash',[12] whose respective reigns in this chronology were probably separated by no more than a decade! The end of the Sealand II – Bazi – Elam Dynasties at about this time may now be connected to the new relationship between Shalmaneser III (r. 858–824 BCE) and a new king of Karduniash, Marduk-zakir-šumi I. Shalmaneser III claimed to have conquered Chaldea and the Sealand and eliminated Marduk-zakir-šumi I's rival, the latter's brother, one Marduk-bel-usate, prior to 851 BCE.[13] The remaining royal line of Nabu-mukin-apli and his two successors (per *BBSt* 9) may now be seen as survivors somewhere in the Sealand, contemporary with Marduk-zakir-šumi I, who appears to have had a long-enough reign in Karduniash to have entered into a treaty with the Assyrian king Šamši-Adad V (r. 823–811 BCE).[14]

There is of course no evidence whatsoever to directly link the last king of this line, Mar-bit-ahhe-iddina, with any following monarch, other than his relative placement (name restored) in Babylonian King List A

[1] See Brinkman (1968, 37–77).

[2] Grayson (1975, 139–144); Glassner (2004, 126–134, no. 3), known from two Neo-Assyrian and two Neo-Babylonian copies.

[3] See conveniently, Oppenheim (1969, 273).

[4] Brinkman (1977, 339).

[5] Paulus (2014, 622–636; NMA 1), dated yr. 25 Nabu-mukin-apli.

[6] Cf. Brinkman (1968, 213–220).

[7] Frame (1995, 72–73; RIMB 2, B.3.1.1).

[8] Grayson 1991, 43 (A.0.87.4: 44–51).

[9] The late Neo-Assyrian 'Synchronistic Chronicle' (Assur 14616c) has Šamaš-mudammiq as contemporary with Adad-nirari II as well as his father Aššur-dan II (r. *c.* 933–911 BCE).

[10] Grayson (1991, 72–73; RIMA 2, A.0.99.1 148: 26–29).

[11] Goetze (1965, 134–135) in his *editio princeps* assumed him to 'have been one of the successors of Aššur-bēl-kala about who little is known. Not that it really matters...'.

[12] Grayson (1991, 208; RIMA 2, A.0.101.1: ii 84).

[13] Grayson (1996; RIMA 3, A.0.102.59: 45–47).

[14] Parpola and Watanabe (1988, 4–5, no. 1; Rm 2, 427).

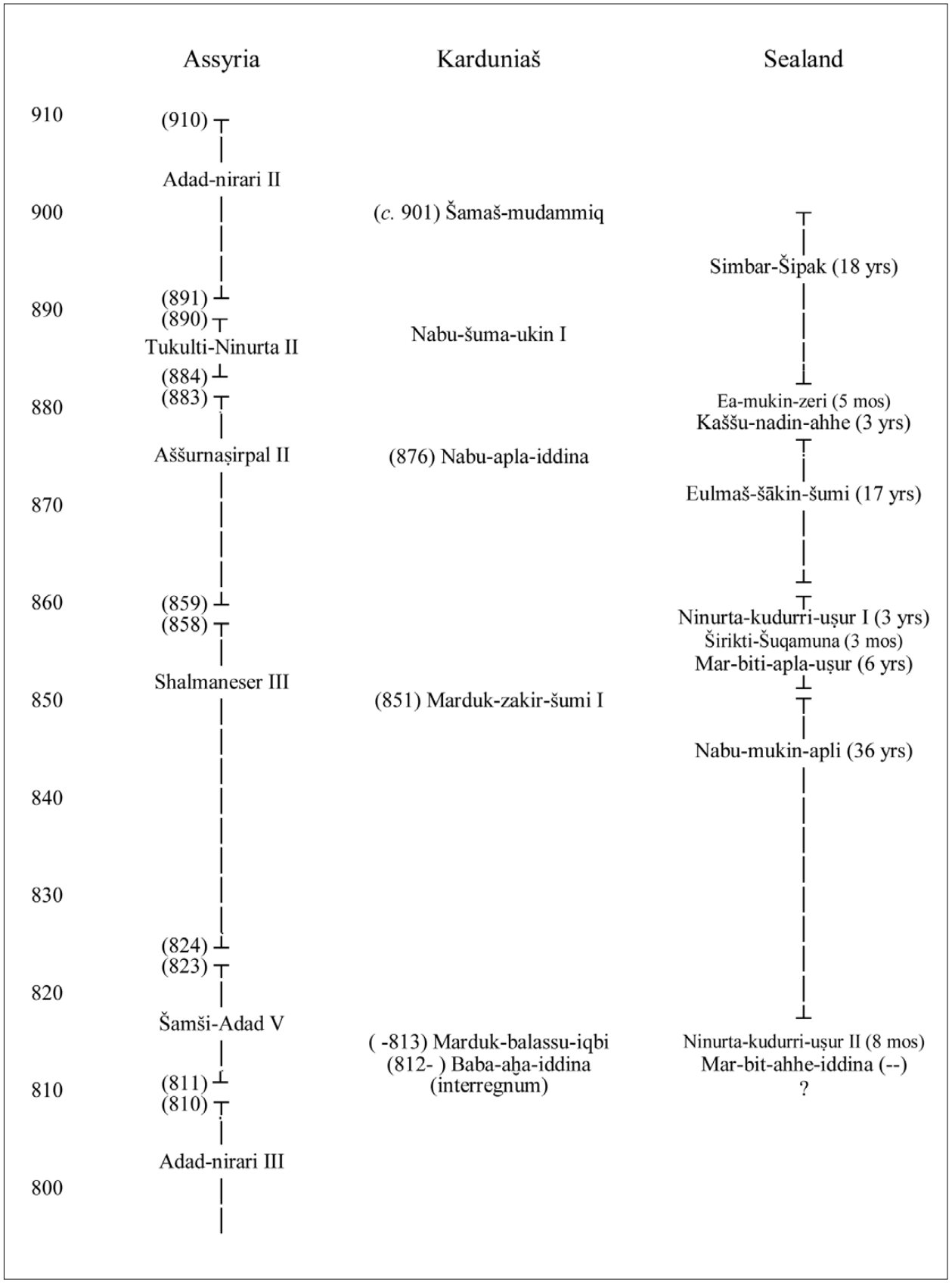

Figure 1. A CoD*-like chronology of Assyria and Babylonia (Karduniash, Sealand) during the early first millennium BCE.*

(iii 14),[15] which, as has been shown elsewhere in these Proceedings (pp. 13–22), has at least two unequivocal substantial overlaps, each on the order of nearly a century in length, between three ostensibly successive dynasties (Old Babylonian – Sealand I – Kassite). The mention of a Šamaš-mudammiq among the witnesses named in *BBSt* 9 (i 22–23) may now be seen as a possible descendant of that king rather than, as previously proposed, an ancestor.[16] The now-implied connection between the rise and fall of Šamaš-mudammiq, Adad-nirari II and the end of Isin II remains to be thoroughly investigated. Finally, although the statement in the 'Religious Chronicle' (*ABC* 17 ii 14),[17] noting that on 26/III/7 [Simbar-Shipak(?)] 'day turned to night and *there was* a fire in the sky', might well be interpreted as a description of a full or annular solar eclipse,[18] current retrocalculations do not provide support for such an event being visible at this time in southern Iraq in either the conventional or ultra-short chronology.[19] There is insufficient data with which to proceed further.

Abbreviations

ABC	Grayson 1975
BBSt	King 1912
RIMA 2	Grayson 1991
RIMA 3	Grayson 1996
RIMB 2	Frame 1995

[15] Grayson (1983, 92); see conveniently, Oppenheim (1969, 272).

[16] E.g. Brinkman (1968, 177, n. 1088).

[17] Grayson (1975, 133–138).

[18] Grayson (1975, 134–136 [ii 1–29]). Although quite broken, the preserved portions of the tablet, with [Nab]u-šum-libur, conventionally the last king of the Isin II Dynasty, referred to in col. i:16, and Nabu-mukin-apli, the first king of the 'Undetermined or Mixed Dynasties' named throughout cols iii–iv, col. ii would appear to have covered the reigns of (unnamed) kings in the intervening Sealand II, Bazi and Elam Dynasties; only Simbar-Šipak and Eulmaš-šakin-šumi enjoyed reigns long enough to be represented by the preserved dates (e.g. Grayson 1975, 134).

[19] http://eclipse.gsfc.nasa.gov/JSEX/JSEX-key.html (accessed 12 Feb. 2024). Cf. the total solar eclipse calculated to have been visible at Babylon on 22 May 948 BCE, which in the currently proposed ultra-short chronology would have likely fallen early in the reign Adad-apla-iddina (r. *c.* *1068–*1047/ *c.* 949!–928! BCE) or later in the reign of his predecessor Marduk-šapik-zeri (r. *c.* *1081–*1069/*c.* 962!–950! BCE). No reference to such an eclipse at this time has been recognized in the preserved ancient literature. Cf. Brinkman (1968, 68, n. 345), who questioned whether the reference was to an eclipse rather than some atmospheric phenomenon.

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Dating Hartapu: The Troubled Relationship between Archaeology and Texts

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This paper aims to analyse and contextualise a recently discovered inscription from Türkmen-Karahöyük in southern Anatolia. The inscription is in Luwian hieroglyphic and refers to a certain 'Great King' Hartapu and his military exploits against the Mushka (a people often identified as the historical Phrygians). It has provisionally been dated to the 8th century BC. This date, however, presents us with a number of problems, most notably in terms of historical context. For if this Hartapu was indeed a powerful ruler in the 8th century BC, why do we not hear about him in contemporary Assyrian texts (which do mention other kings in the area)? And how could this be squared with the fact that the Phrygian kingdom flourished precisely at a time when Hartapu claims to have defeated various kings and supposedly ruled over a significant swathe of land? These and other problems warrant a re-examination of the evidence, especially now that the 8th century BC date has already been adopted as a reference point for the interpretation of archaeological data from Türkmen-Karahöyük. This paper highlights several methodological pitfalls and discusses the possibilities and limitations of interpretations of the currently available historical, archaeological, linguistic and epigraphic evidence.

Introduction

The centuries between the collapse of the Hittite empire, conventionally date to around 1200 BC, and the rise of the Phrygian kingdom in the historical 8th century BC, remain an ill-understood period in Anatolian history. Consequently, the importance of the stele that is discussed in this paper is immediately clear. Found near the newly discovered archaeological site of Türkmen-Karahöyük, the stele bears an inscription (TÜRKMEN-KARAHÖYÜK 1) that identified it as the work of a 'Great King' Hartapu. Since a king with this very same name was already known from other stelae in the region and was associated with the mighty kingdom of Tarhuntassa, the seat of a viceroy during the Hittite Empire period, it has been proposed that Türkmen-Karahöyük was in fact the site of this long-lost kingdom. Moreover, apart from naming Hartapu, the stele also referred to a military victory over the Mushka, a people commonly identified with the Phrygians from later, classical sources. The stele therefore is of tremendous historical importance, but since its discovery in 2019, the exact date and historical context of the stele have been questioned.

The *editio princeps* of the stele by Goedegebuure *et al.* (2020) suggests an eighth-century BC date, and this date has now been adopted by archaeologists working at Türkmen-Karahöyük. The step is a logical one, but not without risks: for if the eighth-century BC date turns out to be incorrect, all subsequent interpretations and hypotheses that build on it must be rejected, too. That this is not a hypothetical danger may be illustrated by the archaeological report on the site by Osborne *et al.* (2020), published in the same issue of *Anatolian Studies* as Goedegebuure's study.

Figure 1. Map showing the location of Türkmen-Karahöyük and other key sites mentioned (U. Zerbst).

The Contribution of Survey Work

Although the site remains unexcavated, some general trends in the development of the site can be surmised based on survey results. Osborne *et al.* indicate that the

Figure 2. The recently discovered inscription of Hartapu (TÜRKMEN-KARAHÖYÜK 1; photo by Fadime Arslan and James Osborne, from Goedegebuure et al. *2020, 30).*

site reached its largest extent (of some 125 to 150ha) in the Hittite Empire period. They also suggest that, following the collapse of the Hittite kingdom, Türkmen-Karahöyük retained much of its habitation during the conventional 12th through the historical 7th centuries BC. If this were true, it would have been one of the largest cities in the world at that time. The pottery distribution at the site is presented as evidence for this scenario. Mapped during the 2019 survey, the material is not always easily identifiable. Indeed, Osborne *et al.* (2020, 13) note that:

> The difference between the Early (ca. 1200–900 BCE) and Middle Iron Age (ca. 900–600 BCE) pottery in this region is almost impossible to discern. Pieces that can only belong to the Late Iron Age (ca. 600–300 BCE) are more readily identifiable, and, when collection units that produced ceramics restricted to the Late Iron Age are mapped, it is apparent that settlement at Türkmen-Karahöyük retracted to roughly 50 ha. *It follows, therefore, that the Late Iron Age witnessed the start of Türkmen-Karahöyük's gradual decline* [emphasis added] and that its maximum extent lasted from the Late Bronze Age until the Middle Iron Age.

This conclusion does not hold. As the authors already indicated, the archaeological evidence – i.e., the distribution of Late Iron Age pottery – indicates that the settlement had *already* shrunk to its much-diminished size at the inception of the Late Iron Age. Unless one wants to argue for a single, sudden depopulation right at the end of the Middle Iron Age, the only plausible conclusion is that settlement at the site had already decreased in the previous period: that is, the Middle Iron Age and possibly even the – in light of the almost indistinguishable pottery – Early Iron Age. This, of course, is difficult to reconcile with the notion of a Great King Hartapu, who was able to defeat the mighty Phrygians. A critical review of the arguments for an eighth-century date of the stele (which, in the absence of a good archaeological context, must be established based on palaeographic and linguistic assessments) is therefore imperative. This is something that will have to be done by specialists, and indeed, some Hittitologists, such as Oreshko (2020) and Adiego (2021), have already expressed doubts regarding the dating and the reading/understanding of parts of the text. As an archaeologist, I am not qualified to judge all of the (linguistic and epigraphic) assessments presented in those studies, though some observations – such as the point made by Adiego (2021, 15–16) that some of the signs that were presented as 'late' in fact seem to have decidedly archaic features – seem to ring true (but see Goedegebuure forthcoming, for a rebuttal of some of these criticisms).

The KIZILDAĞ 1 Conundrum

Where I can judge the evidence, however, the arguments for an eighth-century date seem remarkably thin, and often cascade into a string of assumptions. One example is the inscription's comparison with the KIZILDAĞ 1 inscription of king Hartapu which flanks the only known representation of a king of this name (see *Figure 3*). This inscription has already been known for several years, and both Hawkins (2000, 429) and Oreshko (2017,

48) argued that it belonged to the conventional 12th century BC. However, it is increasingly thought that the accompanying relief of a seated king was probably contemporaneous with the inscription. Yet the style of the relief seems like Assyrian examples from the 8th century (Goedegebuure *et al.* 2020, 41; Hawkins and Weeden 2021, 391), and thus it has been suggested that the KIZILDAĞ 1 inscription must date to that era as well. If the newly discovered stele from Türkmen-Karahöyük dates to that same period (the 8th century BC), such a suggestion would seem to make sense. Yet there are various methodological problems with these arguments, not least because the association between the inscription and the relief is far from certain. Hawkins (2000, 434), for instance, was quite explicit: 'it may be necessary to assign the inscriptions and the figure to two widely differing dates', while Hawkins and Weeden (2021, 393) have taken recourse to the idea that there were two kings called Hartapu (I and II), while acknowledging the problem this raises: 'Why should an 8th century Hartapu echo the inscription of a 12th century namesake (possibly his actual ancestor)? Could the memory in this area have extended back more than 400 years?'

In addition, given the dearth of Assyrian reliefs prior to the 9th century, we should not be too confident in assigning *post quem* dates to given iconographical features: we simply know too little of the development of sculptural traditions even in Assyria itself, let alone in remote places such as southern Anatolia, prior to Ashurnaṣirpal II. Certain features may have developed well before they are archaeologically attested. On top of all this, the figure on the throne, whilst undeniably 'Assyrian-inspired', displays a number of peculiarities (such as the upward-curled shoes) that seem more Anatolian, whereas other, 'Assyrian' features (such as the hairstyle and tripartite beard, as well as the position of the fingers holding a cup) are, according to d'Alfonso and Pedrinazzi (2021, 146), 'peculiar to the representation of Ashurnaṣirpal II and Shalmaneser III'; both these Kings ruled in the 9th (not the 8th) century BC. Dating the KIZILDAĞ 1inscription based on the accompanying relief is thus fraught with numerous problems and uncertainties.

In addition to these iconographical considerations, there are historical problems with assigning the new inscription at Türkmen-Karahöyük to the 8th century BC. For example, Hartapu's designation as a 'Great King' – a highly coveted title that had previously been used by the Hittite emperors – suggests that he ruled over an extensive swathe of land. Yet this seems difficult to reconcile with our understanding of the political geography of 8th century BC Anatolia (which was dominated by the kingdom of Phrygia [which reached the zenith of its power in the second half of the 8th century], with Assyria exercising some sort of control over various minor kingdoms in southern Anatolia): put simply, there is no space on the map for another eighth-century BC Great King.

This problem might be solved by assuming that the title of Great King was, by this time, merely an obsolete heirloom with no connection to any real power or territorial rule. But this again boils down to special pleading, for we have no evidence for a similar flippant use of royal titulary elsewhere at that time – or any time before or after the 8th century BC.[1] More importantly, while suggesting that Hartapu was, despite his claims to Great Kingship, merely a petty chief, this contradicts his claims that he conquered the land of the Mushka. One might argue that this victory did not really amount to much more than a successful raid (as per Goedegebuure *et al.* 2020, 41), but this – though not impossible – is again special pleading and only permissible if there are overriding arguments to do so. Even if one were to argue that the 'conquest' merely was a successful raid into Phrygian territory, such an endeavour still seems completely at odds with the capacities of a petty king. The Phrygians reached the pinnacle of their power precisely under Mita/Midas, in the late 8th century BC, and would have surely retaliated. If Hartapu were indeed capable of inflicting a defeat on Mita, moreover, one would – as Goedegebuure *et al.* concede – expect references to him in contemporary Assyrian sources; *quod non*.

Suggesting that this raid may have taken place just before Mita's reign, in the earlier 8th century BC, and before Tiglath-pileser III invaded Tabal (Goedegebuure *et al.* 2020, 41; see now also Massa and Osborne 2022, 98), is hardly more plausible. Whilst there are no Assyrian sources on the region from the early 8th century BC, the early eighth-century BC inscription of Yariri from Carchemish does refer to the region and mentions only the *Musha, Mushka* and *Sura*. And although the identification of these three terms is not wholly secure, the general consensus seems to be that these are terms for the Lydians (Greek *Mysioi*), the Phrygians (Greek *Moschoi*, Assyrian *Muški*)[2] and possibly Urartu

[1] In fact, the only other king in the wider area who is known to have borne the title 'Great King', Wasusarma (ruled *c.* 740–730 BC), seems to have done so with good reason: in the so-called Topada inscription, he claims control over several vassal kings (though he was, at same time, also listed as a tributary to the Assyrian king Tiglath-pileser III; Bryce [2010, 143–144] suggests that his eventual removal from the throne may have been the result of his flaunting of imperial titles).

[2] Though there may have originally been a distinction between the Mushka and Phrygians, the two ethnonyms appear to have become virtually indistinguishable by the reign of Mita at the latest. Before that time, though, it is likely that the Phrygians and Mushka represented different peoples; the latter are most likely situated in the region east and southeast of Tabal (Wittke 2004; Kopanias 2015, 216). I owe this observation and the references, to Peter James. In a recently published paper, James (2021-2022) argues for a ninth-century BC date for Hartapu.

(Hawkins and Weeden 2016, 14; Rollinger 2006; though Simon [2012, 172] argues convincingly against this identification) or Tyre; apparently, and probably tellingly, omitting the supposedly mighty kingdom of Hartapu. The easiest explanation for such an omission is, of course, that there was, at that time, no such powerful state in southern Anatolia.[3] This is corroborated by an earlier reference from the time of Shalmaneser III (837 BC) to no less than 24 (clearly petty) kings of Tabal (Bryce 2010, 141).

We are thus faced with a conundrum. If one argues that Hartapu ruled in the late 8th century BC, one must accept that a king who clearly ranked amongst the most powerful rulers of his time – powerful enough to invade the realm of king Mita – was not mentioned in the otherwise well-informed contemporary Assyrian annals. If one on the other hand argues for an early eighth-century date of Hartapu, then one must assume that, despite his grandiose title, Hartapu was merely one of the myriads of unnamed kings ruling a minor kingdom in southern Anatolia. Yet at the same time he was apparently able to raid the territory of a next-door emergent superpower with impunity. The latter scenario seems just as unlikely as the first, if only because Phrygia was, already by the late 9th century BC, a powerful kingdom and almost certainly under the control of a single ruler (Muscarella 1995, 96; see Kealhofer *et al.* 2019, 511 for the most recent ^{14}C dates).

The Wider Historical Picture

There are other problems with placing Hartapu in the 8th century BC. The realisation that Phrygia must have already been a more or less united kingdom with a single ruler, forces one to disassociate the 13 vanquished kings mentioned in lines 3 and 4 from Hartapu's victory against the Mushka. Yet there is nothing in the inscription that suggests that Hartapu's victory and resettlement of these 13 kings was not an integral part of his campaign against the Mushka. Quite to the contrary in fact, for the text indicates that these kings came forth *while* Hartapu was campaigning in the land of Mushka (as Goedegebuure *et al.* themselves note, the literal translation is 'the enemy came down into the land' – there is no need to translate this as 'the enemy descended upon (*his*) territory' [emphasis added]). Indeed, disassociating Hartapu's conquest of the country of the Muška (line 1), with his defeat of 13 otherwise unidentified kings (line 3) in battle (line 2) who were subsequently settled in ten strong fortresses (line 4), raises more questions than answers. How could an otherwise unattested, and thus presumably minor king, be able to raid with impunity an emergent superpower, and then defeat a coalition of 13 Tabalian states? Instead, when arguing from the inscription itself, it seems most logical to consider the 13 kings as rulers of the Muška, who were defeated in Hartapu's campaign against this land, and subsequently settled as vassals 'under ten strong-walled fortresses'. Whether these kings were being held in or controlled from these fortresses or given the command (as vassals) over these fortresses, is open to interpretation, but as far as the inscription itself is concerned, the most logical reading indicates that Hartapu was not only able to defeat the Muška, but also that they, at that time at least, were not ruled by a single king but by no less than 13 leaders.[4]

This, of course, does not at all fit the eighth-century context proposed in Goedegebuure *et al.*, and instead rather suggests a much earlier period; a period of tribal leaders and petty kings, who were on the move or, as the inscription has it, were 'coming down into the land'. The most plausible historical setting for such a situation, as far as I can see, is the conventional 12th century BC; i.e. the period immediately following the collapse of the Hittite empire and, not wholly coincidentally, also the period to which the Hartapu from the KIZILDAG 1 inscription and various other inscriptions (Meriggi 1964, 52; Hawkins 2000, 434) were originally thought to belong. If the TÜRKMEN-KARAHÖYÜK 1 inscription does indeed belong to the 12th century BC, it would confirm suspicions of the survival of a Hittite rump-state at Tarhuntassa into the Iron Age. I note in this respect that Hawkins (2000, 429) already suggested that the 'visiting Great King' Ir-Teshup referred to in the stele from Karahöyük (Elbistan) was a king of Tarhuntassa, rather than a king of Carchemish: we may now be able to gain a better understanding of that king's royal line. A 12th century date would also shed new and important light on the development of Luwian

[3] Although it cannot wholly be excluded, the identification with Tabal is extremely unlikely. In his inscription, Yariri boasts of the various languages and scripts he has mastered. Seeing that the script at Carchemish was Luwian hieroglyphic and thus not distinct from the script in use at Tabal, there would be little reason to boast of mastering this language. Younger Jr. (2014, 181) suggests that Sura may refer to (the alphabetic script used at) Tyre. Assyria has also been proposed, though one would then have to explain why that country is listed twice in the KARKAMIS A15b §19 text.

[4] Peker (2020) suggests that the fortresses belonged to the respective kings and were sacked during Hartapu's campaign, though I find such a reading less convincing (if only because we would then have to account for 13 kings ruling 10 fortresses). The impression is rather of (mobile) war bands with their respective leaders, who were subsequently settled in fortified (settled – and thus more easily controllable) places. Being trained as a classical archaeologist, I cannot help but note that the 'mighty fortresses' in the inscription may be compared to later Homeric tradition (*Homeric Hymn to Aphrodite* V.111–112), which describes Phrygia as a land 'rich in fortresses', ruled by one Otreus (cf. Homer *Iliad* 3.186), where Mygdon is mentioned as the co-ruler with Otreus over the 'multitudes' of Phrygian warriors on campaign near the River Sangarius in central Anatolia.

Figure 3. The KIZILDAĞ 1 relief with the only known representation of Hartapu (from Rossner, E. P., 1988. Die hethitischen Felsreliefs in der Türkei*. Nördlingen).*

Hieroglyphic writing traditions. Rather than unique examples of a local tradition 'that adhered more than anywhere else to the traditions of the empire period', as Goedegebuure *et al.* suggested, the inscription's idiosyncrasies could now easily be explained in the context of a transitional phase.

Concluding Remarks

By way of conclusion, I should like to make three points. First, that the archaeological evidence suggests that Türkmen-Karahöyük's heydays were the Late Bronze and Early Iron Age (and not, as we argued above, the Middle Iron Age). Textual evidence, moreover, seems to support this notion. The numerous petty states that are mentioned in both Assyrian and local (Luwian hieroglyphic) texts, moreover, indicate that whatever overarching power that may have existed throughout the Early Iron Age had collapsed by the 8th century BC. In the absence of any unequivocal evidence to the contrary, I would therefore be inclined to date the newly found inscription from Türkmen-Karahöyük to the Early Iron Age – most probably in the conventional 12th century BC. Such a date requires no special pleading and fits the evidence that is currently at our disposal. There are, of course, other solutions to explain the inscription's idiosyncrasies. One way to explain some of these might be to assume that the inscription stems from the period shortly after the fall of the Hittite Empire, but was reworked at some later date. This is not impossible, for there are clear differences in the execution of line 1 and the rest of the text. Yet it would raise numerous questions, most notably why a given king would want to alter an earlier text and when (the latter becomes especially important when we realise that the site was in decline during the Middle Iron Age). Another solution might be to accept that something is terribly wrong with our (conventional) understanding of Late Bronze and Early Iron Age chronology, and that the cultures and reigns that are now associated with 12th century BC belong, instead, to the 10th or 9th century BC. The effects of such a major adjustment would be immense, as the ripple effect of such a modification would force us to reconsider the chronologies of essentially all other parts of the ancient world (including, most worryingly, the sequence of kings of Egypt and their absolute regnal dates, which is generally – though not universally – considered to be relatively secure).

Second, and on a more methodological note, I should like to stress that special pleading only ought to be permissible if it facilitates our understanding of how various *undisputed facts* may relate to each other – and not as a way to weave various *assumptions* into a single narrative. Perhaps this view is a reflection of my own ignorance (as an Aegean prehistorian dabbling in the field of Luwian inscriptions) but the use of terms such as 'expertly archaizing' to explain the use of signs that were hitherto only found in secure Bronze Age contexts in apparent Iron Age contexts (Hawkins and Weeden 2021, 391) raise more than a few red flags – especially given our less than satisfactory understanding of the chronological sequence at the site (Carchemish) where these signs were inscribed. Similarly, whilst one cannot *a priori* exclude the possibility that line 1 of the inscription from Türkmen-Karahöyük is a pastiche of an earlier text (KIZILDAĞ 4) – the carving of this line does indeed appear markedly different from the rest of the text – it seems inherently implausible that the content of this finely carved first line has no real relation to the rest of the text (*pace* Hawkins and Weeden 2021, 392–393). We must assume that the text was meant to be read by someone, and simply using a random line mentioning an earlier, illustrious, namesake before continuing, *in medias res*, with an entirely different,

contemporary story hardly makes sense (as indeed, Hawkins and Weeden seem to admit when they note that this was 'made for whatever reason'). Rather than resorting to a historically inexplicable, and (to my mind at least) elaborate and unprovable scenario, it would seem prudent to review the entire paradigm that has spawned the various conflicting dates. When our current idea of the development of the Luwian Hieroglyphic script forces us to assume the existence of otherwise unattested local, archaizing (as per Goedegebuure *et al.*) or even 'expertly archaizing' (*supra*) signs, and now even forces us to account for, essentially, two entirely different dates (a pastiche and later addenda) within a single text, one cannot help but wonder whether our entire understanding of the development of Luwian Hieroglyphic writing might not be deeply flawed.

Third, and perhaps most importantly, I hope to have highlighted not only how important it is to take into account evidence from neighbouring fields, but also how wary one must be when taking given 'facts' from a field that is not one's own (in this case: the supposed eighth-century date for the inscription) as a point of departure for interpreting new evidence (the archaeological data). This is an important point, for 'facts', once they have entered academic debate (and prestigious publications), are only rarely revisited, yet they often serve as lynchpin for entire archaeological paradigms. The discoveries made by Osborne, Massa and Bachhuber offer us a rare chance to revisit our current paradigms and significantly add to our understanding of southern Anatolia's 'Centuries of Darkness'. Let us proceed with caution and not jump to pre-existing conclusions.

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The Hittite New Kingdom and Its Relationship with the Neo-Hittite Kingdoms: Gap or Continuity?

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In the standard Near Eastern chronology over two centuries elapsed between the end of the Hittite New Kingdom and the emergence of the Neo-Hittite kingdoms. Yet there appear to be no certain rulers available to fill these centuries, though Imperial styles, names and titles recur in the royal houses of Neo-Hittite times suggesting continuity rather than a break. At the same time the archaeological, iconographical and epigraphical evidence suggests that the two periods were actually successive. Lowering the chronology of the Hittite New Kingdom by some 250 years would remove the hiatus and produce a more rounded model for the origin of the Neo-Hittite kingdoms.

Problems with the Standard Model

While it is ultimately Egypt that provides the dates for the Hittite New Kingdom, based at Hattusa (Boğazköy) in central Anatolia, it is mainly Assyria that controls those of the 'Neo-Hittite' (partly Aramaean) kingdoms that succeeded it during the Iron Age – at Carchemish, Aleppo, Hamath and Unqi in Syria, and Malatya/Melid and Tabal in southeastern Anatolia (see *Figure 1*).

The use of the two systems has often led to inexplicable anomalies and chronological tension in these regions – regarding their art, archaeology and history. Controversies began in the 19th century, when most scholars were content with a first millennium BC date for the archaeological remains of the Hittites, including those of the New Kingdom on the central Anatolian plateau. This position was confounded by the discoveries of the archives at Tell el-Amarna, Egypt, in 1887, and Boğazköy in 1906. These,[1] together with

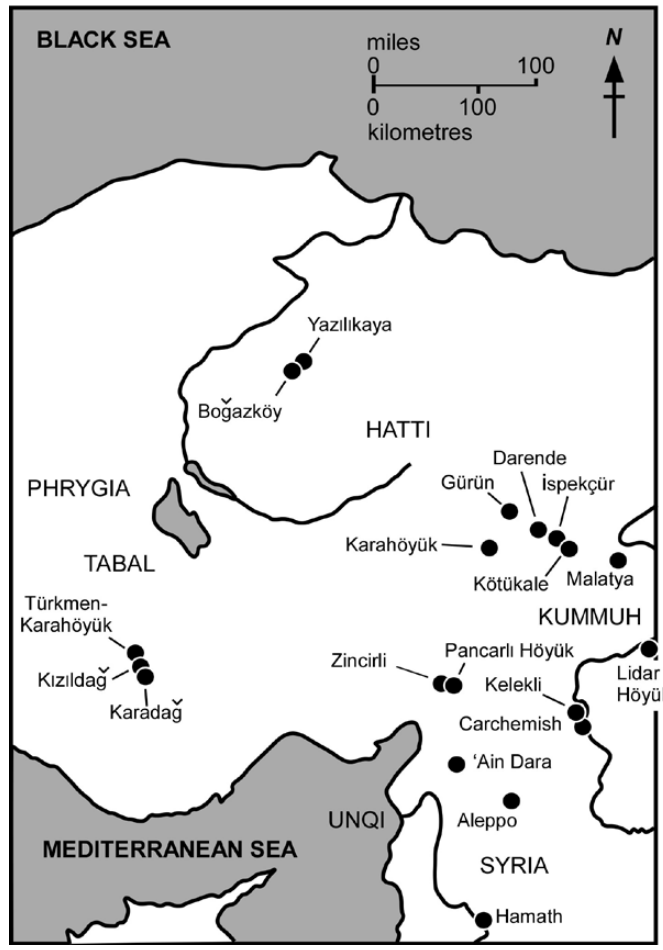

Figure 1. Map of the Hittite and Neo-Hittite sites mentioned (U. Zerbst).

[1] Suppiluliuma I was contemporary with the Egyptian King Ḫuriy[a] and the latter's father: *EA* 41. Krauss (1978, 67, n. 2) and Miller (2007, 279–282) identify Ḫuriy[a] (and his father) in *EA* 41 with Neferkheperura Akhenaten (and Amenhotep III) of Dynasty 18. Moran (1992, 115, n. 2) and Rainey (2015, Vol. 2, 1386) opt for either Akhenaten, Tutankhamun or Smenkhkara. In the 'Deeds of Suppiluliuma I' (*CTH* 40), an Egyptian queen, the widow of Pharaoh Nibkhururiya, asks Suppiluliuma I for a son of his to marry; her husband had died, and she had no son (KBo 5.6 iii 7–15: Güterbock 1956, 94). Nibkhururiya

the war records of Ramesses II of Dynasty 19, which describe his struggles with the Hittites,[2] showed incontrovertibly that the Hittite New Kingdom was contemporary with the 18th and 19th Dynasties in Egypt and hence belonged to the second millennium BC (see *Figure 2*), since the conventional chronology of Egypt dates the reign of Ramesses II to *c.*1279–1213 BC (Kitchen 1987, 52).

Tudhaliya I or II[a]	1400–1350
Arnuwanda I	
Hattusili II?[b]	
Tudhaliya III	
Suppiluliuma I	1350–1322
Arnuwanda II	1322–1321
Mursili II	1321–1295
*Muwatalli II	1295–1272
*Mursili III	1272–1267
Hattusili III	1267–1237
Tudhaliya IV	1237–1209
Arnuwanda III	1209–1207
Suppiluliuma II	1207–?

[a] Bryce (2012, 336, note 1 in his Appendix III): 'It is uncertain whether there were one or two early New Kingdom rulers of this name.' Scholars now propose only one early ruler, Tudhaliya I, with the former Tudhaliya III being labelled Tudhaliya II: Weeden (2022, 569–570).

[b] The existence of Hattusili II is dubious: Bryce (1998, 153–154); Weeden (2022, 570, n. 162).

Figure 2. Conventional chronology for the Hittite New Kingdom (after Bryce 2012, 310 [Appendix III]). The kings listed had their capital at Hattusa in central Anatolia, with the exception of those marked with an asterisk, who temporarily used Tarhuntassa in southern Anatolia as their base.

is either Neferkheperura Akhenaten (so Krauss 1978, 36–40; Parker 2002; Miller 2007; Gabolde 2015, 68–70, 549, n. 34), or Nebkheperura Tutankhamun (Bryce 1990; Murnane 1990, 23, n. 114, 133 and n. 120; Dodson 2020, 79–80).

[2] Ramesses II fought against Muwatalli II at the Battle of Qadesh, in year 5 of Ramesses II – Treaty of Ramesses II with Hattusili III: K*RI II* 227.3–4; K*RITA II* 72 (64). The 'Apology of Hattusili III' (*CTH* 81: Otten 1981, 16–17 [KUB 1.1+, ii 69]) and an edict of Hattusili III (*CTH* 86: Ünal 1974, 20–21 [KUB 21.17, i 14–17]) mention Muwatalli II's conflict with Egypt, but they do not identify the Egyptian king by name. Hattusili III concluded a peace treaty with Egypt in year 21 of Ramesses II – Treaty of Ramesses II with Hattusili III: K*RI II* 225–232; K*RITA II* 71–76 (64); cf. the Hittite version of the treaty (*CTH* 91): Beckman (1999, 96–100 [15]).

This backwards shift in time of the Hittite Empire introduced over two centuries between its demise and the emergence of the Neo-Hittite kingdoms in southeastern Anatolia and in Syria. As the leading expert on hieroglyphic Hittite inscriptions, David Hawkins (1974, 67), long ago wrote regarding the 12th to 10th centuries BC: 'During these three centuries, such archaeological remains as have been discovered in Syria float in a chronological vacuum.' Fifty years later and despite considerably more research and excavation, there is still no convincing evidence for the reality of the conventional two centuries between the end of the Hittite New Kingdom and the emergence of the Neo-Hittite kingdoms. Architectural remains, sculptures, inscriptions and even kings have continued to be moved backwards and forwards in time within this vacuum. James *et al.* (1991, 129–132) reviewed the archaeological anomalies at both Carchemish and Malatya – including the lack of clear archaeological strata at these sites to bridge the 'Dark Age' gap. This paper updates those questions with the discussions and discoveries that have been made in the last thirty years or so. To review all the relevant sites would be impossible in a short paper, and only some of the more striking cases are presented below.

Tabal: King Hartapu

A conspicuous case concerns the ongoing controversy over the dating of King Hartapu, a ruler in Tabal in southeastern Anatolia. His inscriptions at Kızıldağ and Karadağ describe him as a Great King, Hero, and the son of a Great King Mursili – thought by some to be the ephemeral New Kingdom ruler Mursili III (also called Urhi-Teshub).[3] Mursili III is conventionally dated to the 13th century, and the inscriptions have Imperial Hittite terminology. Yet one inscription is accompanied by an eighth- or possibly ninth-century Assyrianising relief of the seated King Hartapu, suggesting that he ruled several centuries later (see *Figure 3* in Jorrit Kelder, 'Dating Hartapu: The Troubled Relationship between Archaeology and Texts', in these Proceedings, p. 137). As David Hawkins (1992, 269) noted:[4]

> Altogether then it would appear that an unbridgeable gulf has been opened between the inscriptions of Hartapus and his adjoining representation. The former are best dated to the early 12th. century B.C., the period immediately

[3] For a survey of scholars who identify Hartapu's father with Mursili III (or Mursili II), see Oreshko (2017, 49, n. 11). Oreshko himself (2017, 59–62) and Goedegebuure *et al.* (2020, 40–42) reject the identification of Hartapu's father with Mursili II or III.

[4] Cf. Oreshko (2017, 48): '... there proves to be a chronological gap of about 300 years between the paleographical and the art-historical datings of the Hartapu monuments, which to this day remains without a satisfactory explanation.'

Figure 3. Lawrence and Woolley at Carchemish. The relief of a war chariot, from the Long Wall of Suhi II, dates to the late 10th century BC and shows clear Neo-Assyrian influence (Liddell Hart Centre for Military Archives, King's College, London, Liddell Hart Collection, ref: LH 9/13/71/3).

> following the fall of the Hittite Empire, the latter to the later 8th. century B.C., the period of Tiglath-pileser III and Sargon, when Assyrian influence in Anatolia was strongest. Thus it seems that the inscriptions and the figure may be separated by as much as four centuries.

The discussion has been rekindled by the recent discovery of a new inscription at nearby Türkmen-Karahöyük of a Great King, Hero, Hartapu/Kartapu, son of Mursili (TÜRKMEN-KARAHÖYÜK 1: Goedegebuure *et al*. 2020; see Kelder in these Proceedings, p. 134, *Figure 2*). The identity in names makes it almost certain that he was the same king as that known from nearby Kızıldağ and Karadağ. As Kelder outlines in these Proceedings (pp. 133–139), the eighth-century date for Hartapu offered by the publishers of the new text clashes with the conspicuous Imperial style of his titulary and with other indications that would argue for a 12th–11th-century BC date; he also stresses problems for the archaeology of Türkmen-Karahöyük caused by an eighth-century dating for the new inscription. The Assyrianising iconography of the Kızıldağ relief can date it to the early 9th century, but this still leaves a gap of two to three centuries between this and immediate post-Imperial times (see James 2021–2022). Hawkins and Weeden (2021) recently had recourse to the solution that there were two kings named Hartapu, one early and one late.[5] The solution is awkward and points again to the wider problem in chronology. Certainly a higher date than the 8th century for Hartapu is required,[6] but consideration also needs to be given to the more fundamental question of the date of the Imperial and immediate post-Imperial periods.

Carchemish

Moving from southeastern Anatolia to northern Syria, the most important of the Neo-Hittite states was at Carchemish on the Euphrates, once the seat of a cadet branch of viceroys that was descended from the Imperial

[5] Weeden (2023, 928–929) notes that the dating of the king/s named Hartapu is still disputed, and he argues that at the least two inscriptions of Hartapu cannot be later than the 12th century.

[6] In TÜRKMEN-KARAHÖYÜK 1, Hartapu claims to have defeated the land of Mushka. As shown in James (2021–2022), the publishers of Türkmen-Karahöyük (Goedegebuure *et al*. 2020) relied on historical misunderstandings in their dating of the inscription to the 8th century BC – regarding the Mushki, their first appearance in the region of Tabal and their alleged identity with the Phrygians of King Midas' days. The publishers were unaware that the Mushki were present in the Tabal/Cilicia region *long before* the 8th century.

Figure 4. Stela KARKAMIŠ A4b (© J. D. Hawkins 2000, Pl. 1).

family at Hattusa (see below). Here the dating of its sculptures has been a subject of major disagreement since the site was excavated by a British team led first by David Hogarth, then Reginald Campbell Thompson and finally Sir Leonard Woolley and T. E. Lawrence, a.k.a. 'Lawrence of Arabia', in the early 20th century. Some sculptures show clear Neo-Assyrian influence (see *Figure 3*).

But others, such as the reliefs on the 'Herald's Wall' and 'Water Gate', clearly exhibit the style of the Hittite Empire and, accordingly, Woolley dated them to the 13th century BC. Several scholars firmly objected – notably Henri Frankfort, the then-doyen of ancient Near Eastern art. He pointed out that some of these sculptures betrayed the influence of Neo-Assyrian art and hence should be no earlier than the 10th–9th centuries. The influential scholar Sir Max Mallowan (1972, 63) agreed:

> The chronology and sequence dating of the rich series of sculpture discovered at Carchemish remains a problem, even after 60 years of investigation, but it is generally recognised that Leonard Woolley exaggerated the antiquity of some of the orthostats and it is no longer possible to assign any of them to the second millennium B.C. On the contrary, many critics will now support Frankfort's view that none of this particular series of sculptures could have been executed without an awareness of neo-Assyrian art.

Figure 5. Some of the 39 figurines from the Gold Tomb at Carchemish (© Trustees of the British Museum).

Nevertheless, Mallowan had to accept the force of some of Woolley's arguments – in particular a stratigraphic one concerning a massive corner-stone from the Water Gate depicting a winged lion of Imperial style which could only have been 'inserted at the cost of demolishing the whole gateway front' (Woolley in Woolley and Barnett 1952, 248, as recognised by Mallowan 1972, 79). Therefore, Woolley argued, this relief cannot be 'secondary' but must belong to the same building phase as the other sculptures found there which he identified as Late Bronze Age. To resolve the issue, Mallowan allowed that this and some other reliefs were 'archaic' (rather than belonging to the Imperial period), which is often a diplomatic way of admitting that an artwork is anomalous in terms of one's preferred dating scheme.[7]

The relationship of the Herald's Wall and Water Gate reliefs to the next sequence of sculptures from the mid-10th to 9th centuries still remains far from clear – despite the recent re-excavation of the site, between 2011 and 2020 (see Marchetti 2012; Marchetti *et al.* 2019–2020). The construction date of the Water Gate remains uncertain,[8] and the Herald's Wall is now

Figure 6. Deities from the Bronze Age frieze at Yazılıkaya near Boğazköy. On the left is the sun god and to his right the moon god (© Wikimedia Commons).

definitely dated to the time of the Country Lord Katuwa, at the close of the 10th century,[9] though the Imperial tendencies still do not find a convenient explanation.

Another anomaly from the early excavations was the discovery of stela KARKAMIŠ A4*b* (Hawkins 2000, Vol. 1/1, 80–82). Found in an apparent Iron Age context, it was erected by one Arnu-[x] the son of a local ruler named Suhi for a king named Ura-Tarhunza (see *Figure 4*). The stela was briefly discussed in James *et al.* (1991, 135–137) and since then another, with a parallel text with extra information and erected by Suhi himself, has been discovered (see below). On both, Ura-Tarhunza is given superior titles to Suhi and full Imperial titulary, and the iconography is clearly Late Bronze in style. Hawkins (1974, 71) necessarily described it as 'archaic-looking', as it is known from other documents that the inscription's dedicator, Suhi I, lived in the 10th century.

[7] Mallowan (1972, 79) proposed the late 10th century for the oldest orthostats, while Gilibert (2011, 27–29) dates them to the early or mid-11th century. She tacitly admits that the Gate was originally built during the Late Bronze Age (2011, 25, 29): 'The Water Gate ... followed an archaic scheme, and its oldest building phase probably dated to the second millennium BCE.' And, when discussing that there would have been four building phases of the Gate: 'The remodeling of a second millennium gateway, with the addition of carved orthostats (among them, Carchemish 3–5.8 but also Carchemish 10–12). Date of the event: early-/mid-eleventh century BCE.' Her use of 'archaic' echoes that of Mallowan and seems to downplay the idea that some of the material may be originally LBA in date. Her reluctance seems to come from her preference to date most of the early activity at the Water Gate to the Early Iron Age.

[8] Marchetti in Marchetti *et al.* (2019–2020, 268): 'The Water Gate seems to have been (re)built in this period [of Late Bronze II], if not in Iron I.'

[9] Marchetti in Marchetti *et al.* (2019–2020, 280).

Carchemish and Yazılıkaya

A further riddle from Carchemish, this time concerning small finds, arose when an Iron Age tomb was excavated – which from the stratigraphy and relationship to the Assyrian levels was no earlier than the 8th–7th centuries. The tomb was richly endowed: numerous fragments of ornamental ivory and gold were found, leading to it being referred to by the excavators as the 'Gold Tomb'. Most distinctive were a group of 39 figurines, made of lapis or steatite, set in gold cloisons and depicting a pantheon of Hittite gods and goddesses (Woolley and Barnett 1952, 252–257 and Pl. 64, and see *Figure 5* here). They bear an undisputable similarity to the procession of deities carved during the Imperial period at the rock sanctuary of Yazılıkaya near Boğazköy: the ones from the tomb are clearly miniature reproductions of the deities from the frieze at Yazılıkaya (see *Figure 6*).

Yet, as Güterbock (1954, 113) states: 'How did carvings of the thirteenth century get into a tomb of the seventh?' Woolley (in Woolley and Barnett 1952, 256–257) felt that they were part of an artistic tradition which had survived at Carchemish since the Bronze Age. Güterbock (1954, 114; cf. Mellink 1954, 250) preferred to see them as 'heirlooms', although he saw the difficulty with this that 'there was no family continuity between the kings of the Empire and the Late Hittite rulers of Carchemish' – as was then understood to be the case. The figurines in the Gold Tomb demonstrate clear continuity in both art and religious beliefs between the Imperial and Neo-Hittite eras, but the length of time involved on the conventional chronology (at least four centuries) does seem surprising.[10] As an outcome, the date of the Gold Tomb has recently been questioned by the new Turco-Italian excavations at the site (Marchetti 2015, 365–366):

> ... around the approximate location of Woolley's 'Gold Tomb', we exposed an LB II level (F.4116 and F.4118) in which one red slipped bowl, placed upside down (Fig. 6), and a horse skeleton cut into pieces were found (Fig. 5). They may be interpreted as funerary offerings related with the Gold Tomb, seemingly a further hint about the early date of that burial, but more evidence is necessary yet.

The suggestion that the Gold Tomb burial was of LBA date is, however, merely an inference and Woolley's arguments for dating the tomb to the Neo-Assyrian period have not been refuted yet, so that the problem of apparent continuity between the Imperial (Late Bronze) and Neo-Assyrian periods remains.

'Ain Dara

Before moving on to Malatya, where the most conspicuous problems arise, a brief look at the site of 'Ain Dara is in order for its intrinsic interest. 'Ain Dara in northern Syria boasts some magnificent sculpted lions in its Iron Age temple (see *Figure 7*).

Figure 7. Lion in the Iron Age temple at 'Ain Dara (© Wikimedia Commons).

Not a 'Neo-Hittite' site in the usual sense, the art and architecture of 'Ain Dara largely followed a native Syrian tradition, related to that of nearby Aleppo, where the dating depends on its Hittite finds. 'Ain Dara's temple has often been compared in dimensions and size to that of King Solomon's as described in the Hebrew Bible (Zwickel 2015, 150; Garfinkel and Mumcuoglu 2019) – pointing to a date in the 10th century, but as noted by Stone and Zimansky (1999, 3), summarising the early excavations, such a late date for 'Ain Dara's temple is at variance with its Imperial features:

> The temple's embellishments, executed in a style that clearly owes much to the artistic traditions of the Hittite Empire, appear to offer paradoxical grounds for dating. Some regard them as 'Late Hittite' or 'Syro-Hittite', belonging to a time relatively early in the Iron Age, shortly after the collapse of the Hittite Empire, *c.* 1200 B.C. ... . In his most recent publications, the excavator argues that some of the sculptures may belong to the Empire itself, but others were carved as late as the mid Eighth Century, B.C. ... . Even a consensus that the 'Ain Dara sculptures are 'early' Neo-Hittite art would not give them a precise date, since the absolute chronology of the sequence is a matter of controversy.

[10] To be fair, Yazılıkaya is an open outdoor site and anyone, even centuries later, might have visited it and been inspired to create the Carchemish miniatures. But even with this possibility we still have to allow for continuity in religion over a long period of time.

Mirko Novák (2012, 41) gives a more recent summary:

> Was the temple a product of one of the Luwo-Aramaean principalities, which constituted themselves after the collapse of Hittite Empire around 1200 B.C.E.? Or was it already built during Imperial Hittite domination over Northern Syria in the Late Bronze Age? Do the reliefs date to the 9th and 8th cent. B.C.E. as supposed by the excavator or do they represent the very early stage of the Neo-Hittite art as argued by W. Orthmann? Or may the time-span of their production even cover both the late Imperial Hittite and the Neo-Hittite Periods as style and iconography indicate ...

The conundrum is redolent of the Hartapu problem and many others.

Malatya: Imperial descendants

Inasmuch as Hartapu has sometimes been considered to provide a link between the Imperial and Neo-Hittite periods, a much firmer relationship was brought into play when Kuzi-Teshub was discovered. In 1985 two seal impressions were found at Lidar Höyük (see *Figure 8*), which lies on the Upper Euphrates, about midway between Carchemish and Malatya. Though fragmentary, the reading of the hieroglyphic inscription is now agreed on – '(King) Kuzi-Tešub, King of the land of Karkamiš, (of) (King) Talmi-Tešub, King of the land of Karkamiš, the son, recognized by the god(s)' (trans. Hawkins 1988, 100). As Talmi-Teshub was viceroy of Carchemish under Suppiluliuma II (*CTH* 122.1: d'Alfonso 2007), the last known Great King of the Imperial period, Kuzi-Teshub had to be dated to the early to mid-12th century (Bryce 2012, 84, 101, 302).

Kuzi-Teshub's royal descendants at Carchemish are presently thought not to have been identified (see *Figure 10*), but he is recognised as the ancestor of a line of local rulers at Malatya/Melid in eastern Anatolia. These associations have raised severe difficulties with the local genealogical and archaeological information. Kuzi-Teshub was the grandfather of Runtiya[11] and Arnuwanti I,[12] both local rulers ('Country Lords') of Malatya. Arnuwanti II, a later Country Lord of Malatya, was a grandson of Arnuwanti I.[13] Because

Figure 8. Sealing of Kuzi-Teshub from Lidar Höyük, on the Upper Euphrates. The Imperial style iconography depicts the Storm God Teshub standing on two 'mountain men' or atlantes,[a] *surrounded by 'fillings' such as a rosette, along with the hieroglyphic inscription fragmentarily repeated in the cuneiform inscription around the edge (© J. D. Hawkins 1995b, Pl. Ia [c]).*

[a] For this motif of *atlantes* supporting gods or kings, familiar from Hittite iconography, see James (1995, 196–200; 2015, 243).

of the genealogical relationship with Kuzi-Teshub, Arnuwanti II should be dated to the late 12th–early 11th century (Bryce 2012, 104, 303). Yet the palaeography of Arnuwanti II's inscriptions and their accompanying sculptures are generally dated to *c.* 900 BC.[14]

[11] Inscriptions of Runtiya: Hawkins (2000, Vol. 1/1, 296 [GÜRÜN §1b]): 'Runtiyas(?), grandson of Kuzi-Tešub, the Great King, the Hero of Karkamiš'; 300 (KÖTÜKALE §1b): 'Runtiyas, grandson of Kuzi-Tešub, Great King'.

[12] Inscription of Arnuwanti II: Hawkins (2000, Vol. 1/1, 302 [İSPEKÇÜR §1]): '(I am?) Arnuwantis the King, grandson of Kuzi-Tešub the Hero.'

[13] Inscription of Arnuwanti II: Hawkins (2000, Vol. 1/1, 305 [DARENDE §1]): '(I am?) Arnuwantis, King Arnuwantis's grandson.'

[14] Genge (1979, Vol. 1, 178) dates the İSPEKÇÜR stela about the year 900 BC and the DARENDE stela about 925 BC. See also Hawkins (2000, Vol. 1/1, 296): 'The reading of the grandfather's name in the genealogy as *ku-zi*-TONITRUS-*sa*$_5$, and the identification, based on the titles "Great King, Hero of Karkamiš", with Kuzi-Tešub son of Talmi-Tešub, king of Karkamiš *c.* 1200 B.C., ostensibly dates GÜRÜN (also KÖTÜKALE) to the later 12th century B.C. İSPEKÇÜR is unexpectedly early palaeographically, particularly when GÜRÜN is compared with the archaic KARAHÖYÜK (Elbistan), discovered less than 60km. across the mountains to the south. In particular GÜRÜN shows the signs *za* and *ia* differentiated from *zi* and *i*, which KARAHÖYÜK does not. An expedient for evading this high date would be to interpret INFANS.(*NI.*)NEPOS ... not as usual, "grandson", but generally descendant. Alternatively, the palaeographic discrepancy between KARAHÖYÜK and GÜRÜN + KÖTÜKALE might be explained by assigning them to contemporary but different epigraphic traditions of the west (Tabal) and of the east (Karkamiš).' Similarly on p. 302 (on İSPEKÇÜR). See further, Wallenfels ('Middle Assyrian History and Culture', in these Proceedings, p. 65, n. 10).

Similar problems exist for another descendant of Kuzi-Teshub. Most of the Lion Gate sculptures at Malatya bear inscriptions of a ruler whose name can be partially normalized as *PUGNUS-mili, who could be *PUGNUS-mili I or II (Hawkins 1988, 103; 2000, Vol. 1/1, 287–288). *PUGNUS-mili I was a son of Kuzi-Teshub,[15] and *PUGNUS-mili II was a son of Arnuwanti I.[16] The now conventional dates for *PUGNUS-mili I and II (the 12th century), a fallout from the discovery of Kuzi-Teshub, clash with the stylistic date for the *PUGNUS-mili reliefs on the Lion Gate, which is the 10th–9th centuries BC.[17] So Hawkins (1988, 102–103):

> This identification of Kuzi-Tešub as the actual grandfather of two kings of Melid [i.e. Runtiya and Arnuwanti I], which follows from the literal interpretation of the inscriptions' genealogies, gives some unexpected and perhaps problematic results for the chronology of Neo-Hittite sculpture and inscriptions. ... The literal interpretation of the genealogical information will thus date the two pieces to the fourth generation after Kuzi-Tešub, i.e. not later than the mid-XIth century B.C. This would be at least a century earlier than would be envisaged by the most recent and reliable chronology of neo-Hittite sculpture. ... The author of the Lion Gate could be identified with either [*PUGNUS-mili I or II], which would date the Lion Gate to the early or late XIIth century B.C., a date up to two centuries earlier than recent opinions favour.

The problem has still not been resolved. Recently Hawkins and Weeden (2016, 9) wrote (emphasis added):

> It has ... been clear since the discovery of the seal of Kuzi-Teššub, king of Karkamish and son of the last known Hittite viceroy at Karkamish, that some kind of continuity exists between the end of the Late Bronze Age Hittite Empire and its Iron Age successor state in northern Syria. *Everything else, however, is entirely unclear*.

In conclusion, the problems hinge around the fact that both iconographical and epigraphical evidence show that the Hittite and Neo-Hittite periods were actually successive, whereas the conventional chronology separates them by over two centuries.

The Conventional Gap between the Imperial Viceroys and Neo-Hittite Kings of Carchemish

The conventional reconstruction not only entails the problem of introducing a separation of two centuries despite apparent continuity, it also faces the problem that there are not sufficient kings attested to fill these two centuries, as the next section, on the rulers of Carchemish, shows.

The viceroys of Carchemish (during the Hittite New Kingdom)

The Hittite New Kingdom kings installed viceroys at Carchemish.[18] Several synchronisms relate the latter to their overlords in central Anatolia:

Piyassili (with the Hurrian name Sharri-Kushuh), son of Suppiluliuma I, was installed by Suppiluliuma I,[19] and he died in year 9 of Mursili II (Annals of Mursili II: Goetze 1933, 108–109).

Shahurunuwa, son of Piyassili, was contemporary with Muwatalli II: he appears as a witness in a treaty between Muwatalli II and Talmi-Sharruma of Aleppo (*CTH* 75: Beckman 1999, 95 [14, §15]).

Ini-Teshub I, son of Shahurunuwa, was contemporary with Hattusili III and Tudhaliya IV: he appears as a witness in a treaty between Hattusili III and Ulmi-Teshub of Tarhuntassa (*CTH* 106.II.2: Beckman 1999, 113 [18B, §15]), and he also appears as a witness in a treaty between Tudhaliya IV and Kurunta of Tarhuntassa[20] (*CTH* 106.I.1: Beckman 1999, 122 [18C, §27]), and as a witness in an edict of Tudhaliya IV releasing Ammistamru II of Ugarit (*CTH* 108: Beckman 1999, 182 [37, §1]).

Talmi-Teshub, son of Ini-Teshub I, was contemporary with Suppiluliuma II (*CTH* 122.1: d'Alfonso 2007).

The time setting for the viceroys of Carchemish results from these synchronisms with the Great Kings of the Hittite New Kingdom. In turn, the chronology for the Hittite New Kingdom depends on its synchronisation with the Egyptian New Kingdom (see *Figure 2* above). As a result, the viceroys are dated to the late 14th – early 12th centuries.

[15] Inscriptions of Runtiya and Arnuwanti II: Hawkins (2000, Vol. 1/1, 296–297 [GÜRÜN upper inscription §1b and lower inscription §1], 300 [KÖTÜKALE §1b], 302 [İSPEKÇÜR §1]).

[16] Inscription of Arnuwanti II: Hawkins (2000, Vol. 1/1, 305 [DARENDE §1]).

[17] Orthmann (1975, 430 [353]): 'Der Block gehört zu den Reliefs des Löwentores von Arslantepe, in denen die Ikonographie der hethitischen Großreichszeit fast unverändert fortlebt, obwohl sie kaum vor dem 10. Jahrhundert v. Chr. entstanden sein können'; Genge (1979, Vol. 1, 174): within the limits of 925–875 BC.

[18] For the identifications of the viceroys of Carchemish, see Klengel (1992, 120–128).

[19] Treaty between Suppiluliuma I and Shattiwaza (*CTH* 51): Beckman (1999, 45–46 [6A, §§10–12]); treaty between Shattiwaza and Suppiluliuma I (*CTH* 52): Beckman (1999, 50–51 [6B, §§4–6]); a report of Hattusili III (*CTH* 83.1.A): Parker (2002, 53 [KUB 19.9, i 18]).

[20] Ulmi-Teshub of Tarhuntassa and Kurunta of Tarhuntassa are the same person: van den Hout (2014); Weeden (2022, 590 and n. 254).

The Neo-Hittite rulers of Carchemish: The Suhi and Sangara dynasties

At Carchemish, the following Neo-Hittite rulers are known for the Suhi dynasty (Bryce 2012, 302):

> Suhi I
>
> Astuwalamanza, son of Suhi I
>
> Suhi II, son of Astuwalamanza
>
> Katuwa, son of Suhi II
>
> Suhi III?, perhaps a son of Katuwa[21]

The Suhi dynasty was succeeded by that of Sangara. The relationship of Sangara to the preceding kings of the Suhi dynasty is unknown (Bryce 2012, 93). Sangara evidently founded a new line, the dynasty of Sangara. Like the members of the Suhi dynasty, these rulers called themselves Country Lords (see the stela of Kubaba: Marchetti and Peker 2018).

The time setting for the Suhi and Sangara dynasties is relatively well established (see *Figure 9*), on the basis of the synchronisms between Sangara and the Assyrian kings Ashurnasirpal II[22] (883–858 BC) and Shalmaneser III[23] (from year 1,[24] Nisannu 858/857 BC, until year 10,[25] Nisannu 849/848 BC). These set the reign of Suhi I (who comes at least three generations before Sangara) in the 10th century.

The conventional gap between Talmi-Teshub and Suhi I: The Kuzi-Teshub dynasty

Given that Talmi-Teshub was a contemporary of Suppiluliuma II, his son Kuzi-Teshub must have reigned shortly after the collapse of the central Hittite polity at Hattusa (see above). The fact that Kuzi-Teshub's grandson Runtiya at Malatya referred to him as 'Great King' (see n. 11 above), a title which was formerly held only by the Hittite Emperors, shows that he continued

Suhi I	probably 10th century
Astuwalamanza	probably 10th century
Suhi II	probably 10th century
Katuwa	probably 10th or early 9th century
(Suhi III?)	
Sangara	*c*. 870–848

Figure 9. Conventional chronology for the Suhi dynasty and Sangara (after Bryce 2012, 90–93, 302 [Appendix II], without Suhi III).

the Imperial tradition of Hattusa, reigning as Great King of Carchemish. Hawkins (1988, 104) concluded that:

> The combination of his own seal and his position in the Malatya genealogies imply that as king of Karkamiš, he not only survived the collapse of the Hittite Empire, but was able to expand his power at least as far as Malatya. In recognition of this, he would have been in a position, when the dynasty of Hattusa disappeared, to claim the vacant titles of his third cousin once removed, Suppiluliumas II.

The placements of Kuzi-Teshub in the early to mid-12th century and Suhi I in the 10th century yield the apparent 'ghost' centuries in the conventional chronology between the Hittite New Kingdom (the Imperial viceroys of Carchemish) and the Neo-Hittite kingdom of Carchemish. To cover this period, five rulers have been proposed who can *possibly* be situated between Kuzi-Teshub and Suhi I (see *Figure 10*).[26]

Ir-Teshub?	later 12th century
Ini-Teshub II?	late 12th–early 11th century
Tudhaliya?	probably 11th or 10th century
Sapaziti[a]	probably later 11th or 10th century
Ura-Tarhunza	probably later 11th or 10th century

[a] Until the discovery of the stela KH.11.O.400 (see n. 36 below), the name was read as '[x]-paziti'.

Figure 10. Conventional chronology for the supposed successors of Kuzi-Teshub (after Bryce 2012, 84–90, 302 [Appendix II]).

[21] Suhi III was recently added to the list: Peker (2016, 48–49); Marchetti and Peker (2018, 97–98). His existence is, however, debated: Weeden (2023, 959 and n. 182).

[22] Grayson (1991, 217 [RIMA 2 A.0.101.1, iii 65, 'Sangara, king of the land Ḫatti']); cf. Grayson (1991, 345 [A.0.101.80], 349 [A.0.101.90]).

[23] Grayson (1996, 9–10 [RIMA 3 A.0.102.1, ll. 55', 67']), 16, 18, 23 (A.0.102.2, i 43, 53; ii 19, 27, 82–83), 38 (A.0.102.6, ii 69), 46–47 (A.0.102.8, ll. 30', 35'), 53 (A.0.102.10, ii 46, 52), 66 (A.0.102.14, l. 85), 76 (A.0.102.16, ll. 67', 72'), 142 (A.0.102.70).

[24] Grayson (1996, 14, 16–17 [RIMA 3 A.0.102.2, i 14 'in my accession year [and] in my first regnal year', i 43, 53 'Sangara, the Carchemishite']).

[25] Grayson (1996, 37 [RIMA 3 A.0.102.6, ii 55–56, 'In my tenth regnal year I … burned the cities of Sangara, the Carchemishite']); *idem* in Grayson (1996, 53 [A.0.102.10, ii 45–46], 66 [A.0.102.14, ii 85]).

[26] Cf. Weeden (2013, 9), who adds King Maza/i-Karhuha, a contemporary of a king named Tudhaliya Labarna (identified with the Great King Tudhaliya of Carchemish), as a possible successor of Kuzi-Teshub (cf. Weeden 2013, 7–8; Hawkins and Weeden 2016, 10; Weeden 2023, 932) and makes Ura-Tarhunza contemporary with Suhi I.

However, both their identification as kings of Carchemish and their chronological positions are disputed.

Ir-Teshub. Ir-Teshub is only attested in one inscription, from Malatya: '(To) the Storm-God of the land POCULUM (this) stele Armananis, Lord of the Pithos-Men, dedicated, at the time when Ir-Teshub, the Great King, came to the land POCULUM' (trans. Hawkins 2000, Vol. 1/1, 289 [KARAHÖYÜK §§1–2]). Ir-Teshub does not explicitly appear as a king of Carchemish but merely as a king of a place related to Malatya.[27] Ir-Teshub might be a king of Carchemish,[28] an idea which Hawkins and Weeden (2016, 10) think 'is certainly worthy of consideration', though Tarhuntassa[29] and the area of Malatya[30] cannot be ruled out. Simon (2013) has, moreover, proposed that Ir-Teshub should be identified with Ini-Teshub II.

Ini-Teshub II. Ini-Teshub II is attested in inscriptions of Tiglath-pileser I as a king of Hatti,[31] most probably – though not necessarily – at Carchemish.[32]

Tudhaliya. The sole attestation for this Great King of Car[chemi]sh is a fragmentary inscription (Hawkins 2000, Vol. 1/1, 76, 82 [KARKAMIŠ A16*c*]), with an uncertain restoration of the king's name (Hawkins 2000, 82; Bryce 2012, 88). Not only the name's reading but also the time setting of Tudhaliya's reign are unsure. Given that a daughter of Suhi II married a king named Tudhaliya,[33] the relevant king Tudhaliya might be the contemporary of the Neo-Hittite Country Lord Suhi II (see also *Figure 11* below).[34] Thus, Tudhaliya 'I' (the conventional predecessor of Sapaziti) would be identical with Tudhaliya 'II' (the contemporary of Suhi II).[35]

Sapaziti and Ura-Tarhunza. Two inscriptions of Suhi I from Carchemish were erected in honour of Ura-Tarhunza, called 'Great King, Hero, King of the land of Carchemish'. One is the chronologically awkward stela KARKAMIŠ A4*b* (see above), while a second stela (KH.11.O.400), with much the same inscription, was found in 2011.[36] This improved some readings, including the name of Ura-Tarhunza's father: 'Sapaziti, Great King, Hero'. KH.11.O.400 further revealed that Ura-Tarhunza was contemporary with Suhi I.

The above survey shows that only Ini-Teshub II and Sapaziti remain as probable candidates for the conventional two centuries between Kuzi-Teshub and Suhi I. This absence of rulers cannot be ascribed to a power vacuum following a supposed massive invasion of 'Sea Peoples' in the time of Ramesses III and their alleged destruction of the site. The modern Turco-Italian excavations report no traces of burning or destruction at the end of the Late Bronze Age. As stressed by the team's Egyptologist, Marco Zecchi (2014, 105):[37]

> ... at present there is no archaeological or epigraphic evidence that Karkemish was devastated by an invasion, so it is likely that the text of Medinet Habu refers to an invasion or sacks by the Sea People in the territory of northern Syria under the Hittite empire, rather than to a destruction of the city itself.

[27] Hawkins (2000, Vol. 1/1, 291 [§2]): 'The recognition of this verb [PES$_2$ + *RA/I*] is historically important: the clause does not state that the named Great King *ruled* in (Karahöyük-)Elbistan, simply that he *visited*. This leaves the way open to supposing that the Great King belonged to the western branch of the claimants of that title, and that the inscription KARAHÖYÜK itself belonged to an epigraphic tradition of the west, not one local to Malatya' (italics by Hawkins).

[28] Bryce (2012, 86): 'The statement that he was visiting POCULUM-land must mean that he came from somewhere else. From where? The titulature "Great King" indicates his importance, and it is possible that he was one of the early rulers of Carchemish.'

[29] Hawkins (2000, Vol. 1/1, 283; Vol. 1/2, 429) considers both Carchemish and Tarhuntassa to be possibilities and prefers the latter. Weeden (2023, 930–931) proposes the Konya region (the region of Tarhuntassa) or Carchemish.

[30] Weeden (2013, 8): 'Although Bryce suggested Karkamish, the location of the text suggests links with the area of Malatya further north.'

[31] Grayson (1991, 37 [RIMA 2 A.0.87.3, ll. 26–28]): 'Finally, upon my return I became [*lord*] of the entire land Ḫatti ... (and) [imposed] upon Ini-Tešub, king of the land Ḫatti, hostages, tax, tribute, and (impost consisting of) cedar beams'; *idem* in Grayson (1991, 42–43 [A.0.87.4, ll. 28–30], 53 [A.0.87.10, ll. 33–35]); Frame (2011, 130 [CUSAS 17 68, ll. 16–18]): 'I imposed (the payment of) cedar beams and tribute upon Ini-Tešub, king of the land of Ḫatti.'

[32] Hawkins (1973, 153 [§3.3]); Bryce (2012, 87): '"Hatti" in these contexts is almost certainly to be equated with the kingdom of Carchemish. If so, then Ini-Teshub can probably be included in the royal "dynasty" established by Kuzi-Teshub which ruled Carchemish for at least two centuries.'

[33] Inscription of Suhi II: Hawkins (2000, Vol. 1/1, 93 [KELEKLİ §§1–2]): 'I (am) Suhis the Ruler[... Karka]miš Country-Lord, Astuwatamanzas's[... so]n(?). And when king Tudhaliyas shall take to himself (in marriage) ... my dear daughter.'

[34] Hawkins (1995a, 82–83; 2000, Vol. 1/1, 93 [§2]). Hawkins (2000, Vol. 1/1, 333) considers the possibility that King Tudhaliya on Suhi II's stela might be a king of Kummuh rather than of Carchemish.

[35] For the ongoing discussion whether there were one or two kings of Carchemish named Tudhaliya, see Weeden (2023, 943–944).

[36] Dinçol *et al.* (2014b); Peker (2016, 14–17). The stela was later copied, as KARKAMIŠ A4*b*, by Arnu-[x], 'the ruler Suhis's son, the priest of Kubaba' (trans. Hawkins 2000, Vol. 1/1, 80 [§6]), who is identified as a son of Suhi I: Hawkins (1995a, 80); cf. Bryce (2012, 88).

[37] Cf. Hawkins and Weeden (2016, 10): 'Despite the declaration of Ramesses III to the contrary, it does not appear archaeologically to be the case, nor is it apparent from external textual records, that Karkamish was destroyed around the end of the Late Bronze Age.'

The idea that Carchemish was destroyed is based on a misunderstanding of the Egyptian record of Ramesses III's Year 8 (James, 'Towards a Resolution of the "Sea Peoples" Rubik's Cube', in these Proceedings, pp. 219–248 and James in prep.).

The attestation of only two possible rulers for a continuous government of two centuries shows the frailty of the reconstruction and suggests that the absolute chronology of the Hittite New Kingdom would benefit from being lowered.[38]

The contemporaneity of the lines of Ura-Tarhunza and Suhi I

The reconstruction of the successive line of rulers is, moreover, complicated by the possibility of contemporary rulers. The contemporaneity of the lines of Ura-Tarhunza and Suhi I is such a case in point. Ura-Tarhunza and Suhi I were formerly supposed to have ruled successively but are now considered to be contemporary. The Hittite New Kingdom had instituted a contemporary viceroyalty at Carchemish, and a similar principle of parallel reigns seems to have continued there during the Neo-Hittite period. Two branches, which may have been related through intermarriage,[39] ruled at Neo-Hittite Carchemish: the house of Ura-Tarhunza, using Imperial titles, and the house of Suhi I, the 'Country Lords'. As Hawkins (1995a, 84) explained it:[40]

> At a certain date, not necessarily earlier than Suhi I, a line of Country-Lords seems to have split off from that of the Great Kings, and to have assumed political power in the city without either appropriating the title Great King or removing its hereditary holders. These Country-Lords, while never at this date called 'king', certainly behaved like kings, if we may judge from their surviving building, sculpture and inscriptions.

This view has matured since the discovery of the new inscription of Suhi I, KH.11.O.400. Dinçol and colleagues (2014b, 151) concluded in their article:

> The relationship between the Great King and the author of the stele [KH.11.O.400] is now more easily understood, though a complete view depends on the interpretation of the word guessed as 'kinsman'. But it looks most probable that the Great King and the Country Lord were contemporaries, perhaps in a relationship akin to sovereign and vizier (and it would in fact not be surprising to find as vizier a member of the royal family).

An analogy, though rather remote, might be made with Great Britain, where the monarchs held sway over much other territory (than England) while the Prime Minister was responsible for the day-to-day affairs of state in the heartland. For similar reasons the archaeological and epigraphical record is biased in favour of the Country Lords rather than the Great Kings, as the former proudly exhibited their names, titles and deeds on monuments at the capital, Carchemish.

Slightly modifying the earlier suggestion of Hawkins,[41] one can discern the following line-up between the Great Kings and Country Lords of Carchemish (Peker 2016, 49; Marchetti and Peker 2018, 98):

Great Kings	Country Lords
Ura-Tarhunza	Suhi I
	Astuwalamanza
Tudhaliya	Suhi II
	Katuwa
	Suhi III?

Figure 11. The contemporaneity of the Great Kings and Country Lords of Carchemish.

[38] Cf. Hawkins (1995a, 84): 'In Karkamiš the surviving evidence does not stretch back ... apparently beyond the house of Suhi and their contemporary Great Kings, i.e. not much before c. 1000 B.C., almost two centuries after the time of Kuzi-Tešub. An unattested line of Great Kings may have occupied this blank period: the Ini-Tešub named by Tiglath-pileser I should have been one of them'; Weeden (2023, 942): 'Turning to Carchemish, while it now seems likely that buildings there continued to be used from the Late Bronze to the Iron Age, it is not until the end of the eleventh century BC that historically identifiable individuals are attested for this city.'

[39] Weeden (2013, 9): 'Suhi I appears to call himself the "seed/kinsman(?)" of Uratarhunda, and we have evidence for a marriage between a king Tudhaliya and the daughter of Suhi I's grandson, the "Country Lord" Suhi II [see n. 33 above]. This and Suhi I's appellation may indicate that there was a history of intermarriage between the "Country Lords" in Karkamish and its "Great Kings".'

[40] Cf. Hawkins (2000, Vol. 1/1, 76): 'The curiosity is the co-existence of a Great King and a Ruler [in KARKAMIŠ A4*b*], whether or not they exercised authority in Karkamiš contemporaneously', 78: 'The evidence however does not show, nor is it easy to suggest, how the two dynasties, one with titles "Great King, King of Karkamiš" and the other with "Ruler, Country-Lord of Karkamiš", might have related to each other.' And see already James *et al.* (1991, 135): 'The titles they [i.e. the Suhi dynasty] gave themselves were restricted to the relatively modest tag: "Lord of the country of Carchemish". Hovering alongside them, a presence which Hawkins has increasingly noted during his collation of all available Neo-Hittite inscriptions, is another line of kings who seem to claim greater titles.'

[41] Hawkins (1995a, 83), before the discovery of KH.11.O.400, synchronised Ura-Tarhunza with Astuwalamanza instead of with Suhi I.

The double line seems to have ended with Katuwa, who was contemporary with Ura-Tarhunza's grandsons (Hawkins 2000, Vol. 1/1, 103–104 [KARKAMIŠ A11*b+c*, §§1, 4, 30]).[42]

The Ura-Tarhunza and Sapaziti question

The reconstruction of the line(s) of rulers of Carchemish is further complicated by the possibility of the identity of some persons. It is, for instance, quite possible that the Great King and King Ura-Tarhunza and the viceroy King Talmi-Teshub are the same individual. The question has to be asked regarding the status and identity of Ura-Tarhunza – even from the little information we have (KARKAMIŠ A4*b* and KH.11.O.400). He must have been a very powerful individual: Suhi I paid him great respect by erecting a monument for him and appears to have gone to pains to associate himself with Ura-Tarhunza as a kinsman. Ura-Tarhunza's descendants were also powerful as they were mentioned at Carchemish by Katuwa (KARKAMIŠ A11*b+c*). All these point to Ura-Tarhunza being a true heir to Imperial power at Carchemish. Perhaps most importantly his name can also be read as Talmi-Teshub, as recognised by Hawkins (1974, 71, emphasis added):

> In the case of A 4 b ... some progress has been made, at least with the readings of the names. Bossert was certainly right in correcting Barnett's reading of the author's name from GREAT-*pa* to GREAT.THUNDER, as was Güterbock in reading the father's name as X-*pa-ziti* (for Barnett's *Pa-ī-da*). These names are now listed by Laroche [1966: 198 (1441)], but in the case of GREAT.THUNDER he hesitates between the Luwian reading (*Ura-Tarhundas*) and the Hurrian (*Talmi-Tešub*), *which would coincide with the name of the last king of Carchemish of the Hittite Empire dynasty*. However, in view of the Luwian character of the father's name, the former is surely to be preferred.

However, it seems reasonable to think that the ideograms for names on hieroglyphic inscriptions, especially those with theophoric elements, could be read bilingually, and it is clear that dual royal names, one Luwian and the other Hurrian, became increasingly common during the Imperial period. Piyassili, son of Suppiluliuma I, was also named (Hurrian) Sharri-Kushuh (see p. 148 above). Piyassili's son Shahurununuwa apparently also had a Hurrian name, [x]-Sharruma (Devecchi 2010, 14–15). Piyassili's nephew the Great King Muwatalli II had the Hurrian name Sharri-Teshub, while his two sons were named in Hurrian respectively Urhi-Teshub (Mursili III)[43] and Ulmi-Teshub[44] (de Martino 2023, 90). Muwatalli II's nephew Tudhaliya IV was also known as (Hurrian) Tashmi-Sharruma (Güterbock 1956, 121; de Martino 2023, 94).

The weight of this evidence for double names, much of which was drawn attention to by Güterbock in 1956, has been overlooked. It seems that it was almost *de rigueur* for the royalty of the Imperial period, the family of Suppiluliuma I, to have dual names. This also means that the possible counterargument that Ura-Tarhunza could not have been Talmi-Teshub as their respective fathers' names, Sapaziti and Ini-Teshub, do not match would be a specious one. Given that the first two viceroys bore dual names (Piyassili = Sharri-Kushuh and Shahurununuwa = [x]-Sharruma), the next two viceroys would likely have done as well (i.e. Ini-Teshub = Sapaziti and Talmi-Teshub = Ura-Tarhunza). The double names would also explain why Sapaziti and Ura-Tarhunza are poorly attested, as they would have mainly issued material under the names Ini-Teshub and Talmi-Teshub.[45]

A possible second objection – that Ura-Tarhunza had the titles Great King and King, whereas Talmi-Teshub is only attested as King – is easily countered. He may have assumed the title 'Great King' only after the end of the Hittite New Kingdom. The sources belonging to the preceding period, when the Great King of Hatti Suppiluliuma II was still alive, would have called him 'King' (Talmi-Teshub), while the later sources call him 'King' as well as 'Great King' (Ura-Tarhunza).

The problem of status is more difficult for the identification of Ura-Tarhunza's father Sapaziti with Ini-Teshub I, given that Ini-Teshub I reigned in the days of the Great Kings Hattusili III and Tudhaliya IV and thus cannot have used the title of 'Great King' himself, whereas Sapaziti is attested as 'Great King' (KARKAMIŠ A4*b* and KH.11.O.400). Yet here the counterargument is not imposing either. For one thing we only have the most limited information on Sapaziti, and have none of his own inscriptions. The two inscriptions mentioning Sapaziti were not commissioned by him personally but by the Country Lord Suhi I and the latter's son. As Suhi I seems to have been aggrandizing himself by association, he may have easily exaggerated Sapaziti's

[42] Weeden (2013, 9): '... Katuwa ... seems to claim that the city had been "empty" and that he had, according to the most recent interpretation of the verb, "acquired it by a legal transaction" from the "grandsons of Uratarhunda". After that the line of the "Great Kings" and their title disappear entirely from the repertoire of titles at Karkamish.'

[43] Both his Hurrian and Hittite names were used officially: Güterbock (1956, 121).

[44] Ulmi-Teshub is a.k.a. Kurunta of Tarhuntassa: see n. 20 above.

[45] Weeden (2023, 945) proposes a geographical explanation for the paucity of their material: '... the physical imprint left by the Great Kings Sapaziti, Ura-Tarhunza, and Tudhaliya on Carchemish is so very faint that one could reasonably argue that they in fact resided somewhere else entirely.'

Chronology	Great Kings of Hatti	Viceroys of Carchemish	Egypt
1st half 11th cent.	Suppiluliuma I	Piyassili	Akhenaten
	Arnuwanda II		
	Mursili II		
2nd half 11th cent.	Muwatalli II	Shahurunuwa	Ramesses II
	Mursili III		
1st half 10th cent.	Hattusili III	Ini-Teshub I (= Sapaziti)	Ramesses II
	Tudhaliya IV		
	Arnuwanda III		
mid-10th cent.	Suppiluliuma II	Talmi-Teshub (= Ura-Tarhunza)	
	Country Lords of Carchemish	Great Kings of Carchemish	
mid-10th cent.	Suhi I	Ura-Tarhunza (= Talmi-Teshub)	
2nd half 10th cent.	Astuwalamanza	Kuzi-Teshub	
	Suhi II	Tudhaliya	
1st half 9th cent.	Katuwa	Ura-Tarhunza's grandsons?	
	Suhi III?		Assyria
mid-9th cent.	Sangara		Ashurnasirpal II Shalmaneser III

Figure 12. Revised scheme for the rulers of Carchemish.

position for his own benefit and have assigned him the same status as Sapaziti's son, Ura-Tarhunza. The city of Carchemish strongly developed as a centre in the days of Ini-Teshub I,[46] which could have justified calling him a 'Great King' in later times.

Concluding Remarks

The synchronisations and identifications for the rulers of Carchemish proposed in the preceding sections result in the sequence reconstructed in *Figure 12*.

The contemporaneity of Ura-Tarhunza (Talmi-Teshub) with Suhi I situates the Hittite New Kingdom immediately before the Neo-Hittite kingdoms. The two periods are clearly successive – with no archaeological gap. The only solution seems to come from a considerable lowering of the Hittite New Kingdom chronology, with its last incumbent as viceroy at Carchemish now ruling as Great King and overlapping with the Suhi dynasty of Country Lords. The 'Great Kings' continued to rule at Carchemish, alongside those with the more modest title of 'Country Lords'. Not long ago the Finnish Hittitologist Sanna Aro (2013, 246) made this telling point:

> Can we bridge the gap between Suppiluliuma II and the Suhi-Katuwa dynasty, a period of 200–300 years? ... the present picture given in various contributions that more or less eliminate the previously postulated gap of the Dark Ages is overly optimistic.

[46] Peker in Marchetti *et al.* (2019–2020, 267–268).

Aro is correct in this observation. One can see it said many times that the discovery of Kuzi-Teshub somehow resolved the Dark Age problem. This is hardly the case. The discovery of Kuzi-Teshub provided a link between the Imperial and Neo-Hittite periods, but one king does not resolve the Dark Age gap of two centuries.

One thing has long been clear. With the demise and abandonment of the central polity at Hattusa, the Neo-Hittite states – and Carchemish in particular – became of increasing importance. Carchemish was the *primus inter pares* and its Great Kings inherited much of the authority and something of the grandeur that their ancestors once wielded as viceroys during the Imperial period. Kuzi-Teshub, 'Great King' of Carchemish, clearly extended his power over Malatya as well. As raised by the current excavators of Carchemish (Dinçol *et al.* 2014a, 131):

> ... who were these Great Kings and where did their title come from? An obvious possibility is that they claimed to be Kuzi-Tešup's descendants, thus ultimately descending from Šuppiluliuma I. A comparable lineage would be the seven generations of Country Lords in Malatya, also descending from Kuzi-Tešup.

Such a conclusion would seem to be inevitable.[47] As the initial line of Country Lords at Malatya were descended

[47] Gilibert (2015, 141) acknowledges: 'At Karkemiš ... Great Kings and Country Lords coexisted at least until the first half of the 10th century, with both titles being inherited by

from Kuzi-Teshub (the *PUGNUS-mili family), it has been increasingly accepted that the royalty at Carchemish may have been as well. Complete continuity between the royal lines of Imperial times and the Great Kings at Carchemish can be brought about if we bring to bear the attested custom of Hittite kings having dual names: one Hurrian and the other Luwian. Talmi-Teshub, the last viceroy of Carchemish, might be the same person as Ura-Tarhunza, the first Great King of Carchemish. This would confirm the direct descent of the Great Kings of Carchemish from the Imperial Great Kings.

To summarise, the following two examples illustrate how a reduced chronology better explains the archaeological material. (1) The stela of Ura-Tarhunza (erected by Suhi I), the Neo-Hittite Great King at Carchemish, brings us to wider archaeological and artistic problems. The presence of his stela in an Iron Age context at Carchemish has always been seen to be a mystery because of its conspicuous Imperial iconography and royal titles ('Great King' and 'King' Ura-Tarhunza). It therefore used to be deemed to be 'archaic'. But if we lower the date for the end of the Hittite Empire from the late 12th to 10th centuries BC, the presence of Imperial style in Iron Age Carchemish would be explainable. (2) On a more general level, the longstanding debate over the date of the Herald's Wall and Water Gate sculptures at Carchemish can now be seen as unnecessary. Both Leonard Woolley, the excavator, and Henri Frankfort were correct – in their own terms. Lowering the chronology would allow both Imperial Hittite and Neo-Assyrian influence on the sculptures. Many of the relevant issues discussed here were recently summarised by Manuelli (2019, 234):

> For the specific case of the Syro-Anatolian societies, it must be considered that practices of constant renewal of the decorative and ideological equipment of the citadels of several sites from the end of the Late Bronze Age to the Iron Age were common. The renovation of the 11th century BC Storm-God temple at Aleppo with the incorporation of sculpted blocks that date back to the Hittite period is probably the most striking example of this practice. Based on the early dating of the latter, a similar chronological assessment and related reuse of some of the sculptures that adorned the 12th century BC phase of the 'Ain Dara sanctuary has been proposed. Recently, it has also been suggested that the bas-reliefs of the 'South Gate' at Zincirli might have originally been located at the nearby Pancarlı Höyük and later re-used when the site was re-founded at the end of the 10th century BC. Moreover, the development of the 'Water Gate' at Karkemiš from an earlier 2nd millennium BC prototype, including the renovation and refitting of its decorative apparatus, has been suggested.

Of course ancient cities, just like our own, were continually repaired and 'modernised' in a way that would incorporate treasured ancient monuments, friezes, sculptures and smaller things like jewellery, even for hundreds of years. But this cannot be used as a catchall excuse to explain the many anomalies in Hittite archaeology. The argument wears a bit thin over so many sites – especially now that it has become increasingly accepted that there was continuity between the Imperial and local rulers at both Carchemish and Malatya, involving the 'puzzling' problems of synchronising the characters involved such as the Malatyan rulers named PUGNUS.

A lowering of Hittite Imperial chronology by some 250 years in line with the Egyptian chronology – as argued in *Centuries of Darkness* (James *et al.* 1991) and in numerous subsequent studies (see, most recently, Wallenfels 2019; James 2021) – would resolve numerous problems in art history and archaeology and open further scope for examining the relationships between the 'Great Kings' at Carchemish and their apparent ancestors in the royal Imperial line.

Addendum

I would like to stress that I still support the identification of the two Ini-Teshubs, 'I' and 'II' (see above), as argued in James *et al.* (1991, 134–135, 362–363), followed by Wallenfels ('Middle Assyrian History...' in these Proceedings, p. 65). It would seem to be the logical outcome of lowering the dates for Ini-Teshub 'I' to the 10th century BC, though Bieke Mahieu remains uncertain about this. Peter James†

Abbreviations

CTH	Laroche 1971
CUSAS 17	Frame 2011
EA	*Amarna Letters*. (1) Translated by Moran 1992. (2) Transliterated and translated by Rainey 2015.
KBo	Keilschrifttexte aus Boghazköi
K*RI II*	Kitchen 1979
K*RITA II*	Kitchen 2019
KUB	Keilschrifturkunden aus Boghazöi
RIMA 2	Grayson 1991
RIMA 3	Grayson 1996

paternal lineage.'

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SESSION 2

THE LEVANT AND THE EFFECTS OF ASSYRIAN CHRONOLOGY

Introduction to Session 2 – A Dark Age Hidden in Plain Sight?

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The absolute dates for New Kingdom Egypt and the Neo-Assyrian Empire are in tension. The tension is not always obvious but comes into focus when criteria from both cultural centres are employed to date strata or ceramic styles in a third area. The result is often that archaeological periods become stretched to fill what would otherwise be a dark age. I will use two examples to illustrate this, the second of which points clearly to New Kingdom Egypt as the source of the problem.

Megiddo

In 1931 P. L. O. Guy attributed Stratum IV at Megiddo to the reign of Solomon. He did this for three main reasons. Firstly, because Stratum IV contained buildings identified as stable complexes, and according to the Bible Solomon had built at Megiddo, and had built cities for chariots and horsemen (1 Kings 9:15 and 19)[1]. Secondly, because Stratum IV produced some Egyptian amulets which were believed to be from the reign of Shoshenq I, who was thought to be king Shishak of the Bible (1 Kings 14:25–26; 2 Chron 12:2–9) and therefore part-contemporary with Solomon. Thirdly, because the stratum contained both hand-burnished pottery and wheel-burnished pottery and this association seemed to fit, perhaps uniquely, a tenth-century BC date (Guy 1931).

Guy's conclusions were soon picked apart by J. W. Crowfoot (1940) and later by Kathleen Kenyon, both excavators of Samaria. Kenyon (1979, 247) wrote:

> The stratigraphic methods used in these excavations at Megiddo make it difficult to rely on much of the pottery evidence. However, enough can be identified that can be attributed to the building stage of these Stratum IV buildings to show that they can be linked with phases III and IV at Samaria, and must therefore belong to the second half of the 9th century B.C.

Guy's conclusions were beginning to look like 'text-impeded archaeology', to borrow a term that Rupert Chapman uses in his article in this volume, pp. 347–354.

Controversy over the stratum and its buildings continued. W. F. Albright and G. E. Wright re-interpreted Stratum IVB in the south of the city and Stratum VA in the north as a single stratum (VA/IVB) dating from the time of Solomon (Wright 1950). Yigael Yadin's discovery of a six-chambered gate at Hazor, almost identical to Gate 2156 at Megiddo, and his detection of a third example at Gezer (in a fifty-year-old plan from Macalister's excavations), appeared to strengthen the association with Solomon, who is reported (1 Kings 9:15) to have built at all three of these cities (Yadin 1970; 1972, 150–164). Even Kenyon conceded that Gate 2156 at Megiddo was 'possibly of the Solomonic period' (1979, 246, caption to Fig. 73).

Leaving aside further re-interpretations by Yohanan Aharoni (1972) and David Ussishkin (1980), I fast-forward to 1996 when Israel Finkelstein revived Kenyon's ninth-century date (without proper acknowledgement) as part of his Low (or Tel Aviv) Chronology of the Iron Age (Finkelstein 1996). This has proved controversial for several reasons. Notably, with the end of the Late Bronze Age fixed at around 1200 BC by Egyptian chronology, starting Iron Age IIA in the late 10th century BC stretches the Iron I period to an unlikely 300 years. As Peter James has pointed out, this effectively exposes a 'Dark Age' in the archaeology of ancient Israel, one previously hidden by the belief that Solomon's reign belonged in the Iron IIA period, which therefore could not have begun much later than 1000 BC (see the introduction by the editors in this volume, pp. 1-7).

Revisions to our understanding of Megiddo's stratigraphy have continued to lower the date of Stratum IV. As a result of the Tel Aviv University expedition the stratum and its monumental stable complex are now attributed to Jeroboam II in the early 8th century BC (Cantrell and Finkelstein 2006, 656–660)[2]. This date was also presented by Norma Franklin in her lecture at

[1] It should be noted that these two statements are not connected, so there is no suggestion that Megiddo was one of Solomon's cities for chariots and horsemen.

[2] Cantrell and Finkelstein (2006, 656–660) suggest that Megiddo under Jeroboam II was not only a centre for the Israelite chariot corps but also a training centre for imported Egyptian horses that were traded with Assyria. See Na'aman (2023, 183) for a critique of the latter suggestion.

the BICANE conference (2021; already Franklin 2006). Lower dates for contemporary Iron Age IIB strata have recently been proposed by Pieter van der Veen (2020) and in this volume, pp. 291–310.

Here I will move on to another example of gaps concealed by an improbable stretching of Iron I chronology. As it happens, this example also involves alleged connections with Solomon.

Southern Arabah Copper Mines and Tell el-Kheleifeh

Until the 1960s it was widely believed, following the work of Nelson Glueck, that the copper deposits of the southern Arabah had been exploited during Iron Age II by Solomon and his successors. This view was overturned by the results of the Arabah Expedition, begun by Beno Rothenberg in 1959. Over the next ten years a very different picture emerged from the work of Rothenberg and his colleagues. Their excavations appeared to show that the copper industry of the area had been active during three main phases: firstly in the Chalcolithic period and Early Bronze Ages; secondly toward the end of the Late Bronze Age and into Iron Age I; thirdly during the 2nd century AD. The dating of the second phase seemed to be clinched in 1969 by the discovery of a sanctuary devoted to the goddess Hathor, containing inscribed Egyptian items from the 19th–20th Dynasties (Site 200).[3] These finds fixed the date of the second phase to the 13th–12th centuries BC. There appeared to have been no copper mining or smelting in the southern Arabah during Iron Age II, so no involvement by Israelite or Judean kings.

Over 40 years ago I questioned the new consensus, pointing out that the Egyptian finds were in tension with other evidence that points to a later date (Bimson 1981, superseded by Bimson and Tebes 2009). Here I will focus on one aspect of that evidence and refer to some recent developments.

Among the types of pottery found at Timna, including in the Hathor sanctuary, is a painted pottery style which Rothenberg called Midianite, but which is better described (following Parr 1988) as Qurayyah Painted Ware (hereafter QPW) because of its origin at Qurayyah in the Hejaz. The discovery of QPW at Timna in association with Egyptian finds from the 19th and 20th Dynasties provided the first datable archaeological context for this pottery.

However, QPW was also found in contexts which seemed to require a much later date. During 1938–1940 Nelson Glueck carried out excavations at Tell el-Kheleifeh, near the head of the Gulf of Aqabah, believing it to be biblical Ezion-geber (1 Kings 9:26; 22:48). To the earliest occupation period Glueck assigned a four-room building with a surrounding casemate wall. After an uncertain period, this was destroyed and replaced by a solid offset-inset wall with a four-chambered gate. Glueck was struck by the similarity of the four-chambered gate to that excavated by Guy at Megiddo. Guy assigned this Megiddo gateway to Stratum IV which, as we have seen, he dated to the time of Solomon. Glueck therefore found it probable that the builder of this phase at Tell el-Kheleifeh 'was none other than the builder of Stratum IV at Megiddo and of numerous other sites throughout the length and breadth of Palestine, namely King Solomon' (Glueck 1939, 15–16).

Glueck discerned five occupation periods at Tell el-Kheleifeh spanning the 10th–5th centuries BC. His preliminary reports on the pottery included six sherds of QPW, though he failed to distinguish them from Edomite pottery with which QPW shares geometric painted decorations. Glueck attributed these sherds to period IV, which also yielded Edomite inscriptions and many items of pottery showing strong Late Assyrian influence: 'Both the shape and hard metallic ware of many of them are in clear imitation of contemporary seventh-sixth century B.C. Assyrian metal and pottery vessels ...' (Glueck 1969, 53). In Glueck's view (1967, 24) period IV could not have begun before the late 8th century (on Edom during the first millennium BC, see also the article by Tebes in this volume, pp. 323–346).

No final report of Glueck's excavations at Tell el-Kheleifeh was ever published, and many issues remained unclear after his death. Certain aspects of his work at the site were reassessed in the 1980s and the conclusions involved a major revision of his views (Pratico 1993). Glueck's excavation and recording methods were often unreliable. Gary Pratico's analysis of the pottery, including new items collected during a survey in 1980, led to the conclusion that – apart from the six sherds of QPW (see below) – none of it is older than the 8th century BC (Pratico 1993, 35–50).

Tell el-Kheleifeh's two architectural phases cannot be dated independently of the pottery, and pottery assemblages cannot be confidently assigned to them, though Pratico finds it likely that Glueck's period IV pottery corpus should be associated with the offset-inset wall and its four-chambered gate (Pratico 1993, 33–34, 49). If this is correct, the architecture which Glueck believed was Solomonic can be no earlier than the 8th century BC, and the site's entire occupational history must fall within the 8th–6th centuries (with a possible afterglow of uncertain duration). Finkelstein (2014, 130–134) has suggested that Tell el-Kheleifeh was part of a system of Neo-Assyrian strongholds, a view in keeping with the Late Assyrian-influenced pottery of period IV (cf. Pratico 1993, 73). During the BICANE conference Alexander Fantalkin informed us

[3] This is often referred to (following Rothenberg) as a temple, but Avner (2014, 122–123) points out that it does not follow the principles of Egyptian temples and recommends the term 'sanctuary'.

of a parallel situation at Tell Qudadi.[4] He argued that the first phase of that site should be redated from the Solomonic era to the second half of the 8th century BC and be viewed as part of a network of Neo-Assyrian fortresses (Fantalkin and Tal 2015, esp. 191, Table 18).

Where does the redating of Tell el-Kheleifeh's phases leave the six sherds of QPW? Even before Pratico's reappraisal of Glueck's work Rothenberg had rejected the attribution of the sherds to period IV and assumed they were surface finds (Rothenberg and Glass 1983, 76). He further claimed that they 'clearly signify a road station on the way from the Egyptian/Midianite mines in the Arabah to the mining regions of NW Arabia' (Rothenberg 1998, 207). No evidence justifies this conclusion. In Pratico's view,

> Because the Tell el-Kheleifeh records do not provide a reliable field provenance for those pieces, little can be said about their significance. At any rate, it is clear that six sherds, without a reliable stratigraphic context, do not document an occupational horizon. (Pratico 1993, 50; cf. Bimson and Tebes 2009, 89, n. 67.)

Juan Manuel Tebes and I subsequently published a paper which re-examined the situation at Timna and re-opened the dating of QPW (Bimson and Tebes 2009). Tebes writes: 'Overall, the picture of the chronology of the QPW is one of complete dependence on the dates of Iron I Timna. Yet such a model seems to be at odds with the growing findings of QPW in late contexts in the Negev and Edom.' He goes on to list no fewer than ten locations where the pottery has been found in 10th–8th centuries BC contexts; Tell el-Kheleifeh is included as a location where QPW 'overlapped geographically and chronologically with true Edomite ceramics' (Bimson and Tebes 2009, 91–92). Glueck's original attribution of the QPW sherds to period IV appears vindicated by the many other instances of the ware occurring in late contexts.[5]

However, this leaves a serious problem unresolved. In discussing Glueck's late date for the QPW sherds from Tell el-Kheleifeh, Rothenberg and Glass (1983, 76) comment: 'It is hard to accept such a longevity of such a homogeneous pottery, from the thirteenth to the sixth century BC ....'[6] On the other hand, in light of a clearly stratified QPW sherd associated with Late Iron II pottery at Tawilan, Piotr Bienkowski has defended a long period of use for the pottery, pointing out that Negevite ware 'had precisely this long time span' (Bienkowski 2001, 261). But the analogy is not persuasive. Negevite pottery is a crude, hand-made ware and its forms were purely utilitarian. By contrast, QPW is a wheel-made pottery combining simple shapes with sophisticated decorations that show a high level of aesthetic appreciation. It is very unlikely that this pottery could remain 'exceedingly homogeneous' over many centuries (Rothenberg 1998, 201; cf. Singer-Avitz 2014, 138, n. 121).

Since 2012 the Central Timna Valley Project, led by Erez Ben-Yosef, has brought about a second revolution in our understanding of copper mining and smelting in the southern Arabah. New excavations and high-precision radiocarbon tests suggest that peak activity in the Timna region occurred during the 11th–9th centuries BC. 'These new dates fall squarely within the biblical timeframe for the United Monarchy and indicate that the majority of the sites in the valley were not Egyptian' (Ben-Yosef and Greener 2018).[7]

Ben-Yosef and Greener refer to 2 Sam 8:13–14 and 1 Kings 11:15–16, according to which Edom was under Israelite control during the reigns of David and Solomon, and point out that the results of their work 'provide a possible archaeological background for the biblical account of an Edom dominated by Israel. We now understand what would have been at stake for David in this remote region: copper.' Consequently, the Arabah copper industry could have been 'the unwritten backdrop' to Solomon's wealth. Hence in an ironic twist, having been shown the door by Rothenberg, Solomon has been welcomed back to Timna by Ben-Yosef.

[4] Fantalkin did not submit an article for the proceedings, but the results can be found in his co-authored book on the subject with Oren Tal (2015).

[5] See also Tebes (2013, 319) and Ben-Yosef *et al.* (2012, 63) for further listings of QPW in late contexts.

[6] There is no compelling reason to date the Tell el-Kheleifeh sherds later than the 8th century BC, but the argument against QPW's longevity still stands.

[7] Uzi Avner (2014, 125–134) has presented detailed arguments that, even at the time of the 19th and 20th Dynasties, local people rather than Egyptians oversaw the copper industry in the Timna Valley (cf. Ben-Yosef *et al.* 2012, 65). Avner's examination of the technology of the mines and smelting furnaces reveals that – contrary to the view of Rothenberg – none of these can be considered Egyptian. Avner asks: 'If technology of mining and smelting, and organization of work were all local, then what was the role of the Egyptians? In my view they were mainly important customers, who needed large amounts of copper for weaponry, monumental construction and so forth.' (Avner 2014, 141–142.) However, in the light of the *Centuries of Darkness* chronology (James *et al.* 1991), Avner's view requires an important modification. In that chronology Ramesses III is identified as the biblical Shishak, who invaded Judah in the fifth year of Solomon's successor Rehoboam (James *et al.* 1991, 385–386; Bimson 2015). Ramesses III is prominently commemorated in the Timna region, not only by small finds in the Hathor sanctuary but also by an outline for a rock-stele above the sanctuary and by a monumental double cartouche carved into a rock-face by Nahal Roded, a southern approach to Timna (Rothenberg 1972, 201 and 132; Avner 2014, Figs. 16–17). It seems likely that after his successful campaign against Judah he asserted full Egyptian control over the copper industry at Timna (see further Peter James, 'Towards a Resolution' in this volume, pp. 219–248).

Of particular importance here are the low dates produced for QPW by the Central Timna Valley Project. Ben-Yosef's new excavations at Timna Site 30 uncovered two fragments of QPW, both 'from well-dated stratigraphic contexts'. The results of three associated radiocarbon tests place these sherds in the 10th century BC (Ben-Yosef *et al.* 2012, 63; cf. Ben-Yosef 2016, 174–175 and 191 on similar dates from Site 34).[8]

Singer-Avitz (2014) has sought to resolve the controversy over the dating of QPW by arguing that it should all be dated to the 12th–11th centuries BC. This is achieved by adopting the lowest possible dates for a range of LB–Iron I examples and by dismissing finds from much later contexts as misattributions or misidentifications. On late dates for QPW which have the support of radiocarbon results she comments (2014, 137):

> [D]ating by radiocarbon test results, while at the same time ignoring ceramic typology … is methodically incorrect as it creates a new pottery chronology that is inconsistent with other sites and even within the same site. It is important to note that there are serious impediments in the radiocarbon dating method and the procedure of data processing.

Radiocarbon results must certainly be treated with caution – sometimes scepticism – but Singer-Avitz is selective in questioning lower dates for QPW rather than higher ones. This is presumably because she trusts the Egyptian chronology from which LB–Iron I dates are derived. She reports without query the results obtained from charcoal samples during Erickson-Gini's renewed excavations (2005–2011) at Timna Site 2, which 'indicated that the material had been deposited sometime between the late 13th and 11th centuries BCE' (Singer-Avitz 2014, 128 with reference to Erickson-Gini 2014, 58 and Table 1). The risk of charcoal samples producing dates which are too early (the 'old wood effect') has been well-illustrated at Timna by Ben-Yosef's team. Tests were done on charcoal samples and short-lived samples from the same section at Site 30, and the results revealed differences ranging from 50 years to 160 years between dates for the two types of material (Ben-Yosef *et al.* 2012, 52–57).

When addressing the tension between the high and low dates assigned to QPW (Ben-Yosef *et al.* 2012, 63), Ben-Yosef shies away from its most serious implication. He quotes from Bimson and Tebes (2009, 106), written when Rothenberg's narrative was still dominant:

> The dates currently given to mining and smelting operations in the southern Arabah produce a number of chronological anomalies and tensions. Taken together these suggest the need for lower dates for New Kingdom Egypt, which would in turn allow a lower date for the Late Bronze/Iron Age transition.

Ben-Yosef comments in response:

> In support of very low Egyptian chronologies (e.g. James *et al.* 1991), Bimson and Tebes (2009) provide evidence from smelting sites of the southern Arabah that appears to be even more robust in light of the new dates published here. However, regarding the complicated deposition of archaeometallurgical materials and the complex stratigraphy of Site 200, it is much more likely that the pottery identification and stratigraphic discernment of the Arabah Expedition were confused.[9]

The potential for confusion at Site 200 cannot be dismissed out of hand (see Avner 2014, 105–110), but it should be noted that the combination of pottery types found there (Negevite ware, QPW and 'normal' wheel-made ware) was the same as that from Layers 3 and 2 at Site 30, from where some of Ben-Yosef's high-precision dates were obtained. The one difference is that Layer 3 (the earliest level) at Site 30 contained some red-burnished Egyptian pottery which was not found at the other sites.[10] When discussing the dating of Site 30 and Site 2 (both smelting camps) Rothenberg states (1999, 160):

> All other types and kinds of pottery of Layers 3 and 2 of Site 30 were identical with the pottery of Site 2 as well as other, unexcavated Egyptian camps of Timna and the Wadi Amram, and, especially, the Hathor Temple. This fact was of course of decisive significance for the chronology of the Egyptian activities in the Southern Arabah, Timna and Wadi Amram.

In short, the identical combination of pottery types found at these locations was taken to indicate that they were contemporary, and that the dates derived from the Egyptian finds at Site 200 could be applied to all of them.

Ben-Yosef is highly critical of Rothenberg's argument, stating: 'The absolute chronology of Site 30 was based almost exclusively on correlation to the findings of

[8] The only radiocarbon test taken from Site 200 during Rothenberg's excavations (BM117) produced a date of *c.* 960 BC (Bimson and Tebes 2009, 104–105; Tebes 2013, Fig. 2; Avner 2014, Table 1, no. 72). While little weight can be placed on a single result obtained as long ago as 1979, it is at least intriguing that it agrees with Ben-Yosef's recent dates from Sites 30 and 34.

[9] Note that although he refers to James *et al.* (1991), Ben-Yosef does not acknowledge the multiple lines of evidence presented there for a shortened Egyptian chronology.

[10] For all four pottery types see Avner (2014, 135–137).

Site 200 (the Egyptian sanctuary). This is a crucial point, as later research took the results of this dating methodology at face value, without critical evaluation, despite the evidently poor assemblage of material culture.' (Ben-Yosef *et al.* 2012, 37.)

However, while rejecting Rothenberg's absolute dates for the pottery at Site 30, Ben-Yosef does not dispute that Negevite ware, QPW and 'normal' wheel-made ware all occurred there (Ben-Yosef *et al.* 2012, 59–63). He subsequently found the same trio of pottery types at the previously unexcavated (therefore undisturbed) Site 34, which also produced radiocarbon dates from the end of the 11th century BC to the late 10th century BC (Ben-Yosef 2016, 174–175). The renewed excavations at Site 2 by Erickson-Gini produced the same pottery types Rothenberg had found there and at Site 200 (Erickson-Gini 2014, 64, Figs. 13–16). Probes conducted at Site 200 by Uzi Avner to clarify the stratigraphy also produced the same three pottery types along with Egyptian items (Avner 2014, 111–113 and 115, Fig. 7). While it is possible that more detailed analyses of these pottery types could change the picture yet again, the ceramic evidence currently appears to support the contemporaneity of these Timna sites.

In light of the 10th-century BC date for sherds of QPW at Site 30 (see above), Ben-Yosef is inclined to agree with a suggestion by Singer-Avitz (e.g. 2014, 126) that the stratigraphy of Site 200 is mixed and suggests that the QPW sherds from there 'are *not* associated with the Egyptian phase' (Ben-Yosef *et al.* 2012, 63, emphasis original). Thus, he avoids the weighty implications of questioning the dating of Egypt's New Kingdom.

In attempting to dissociate the dating of the Egyptian sanctuary from that of other Timna sites, Ben-Yosef also states: 'During the first 10 years of intensive research in the southern Arabah, including extensive excavations at Site 2, the Arabah Expedition did not report the discovery of *any* Egyptian-related artifacts from this region.' (Ben-Yosef *et al.* 2012, 62, emphasis original.) This overlooks the discovery at Site 2 of a scarab from the time of Ramesses II, which led to that site being dated to the 13th–12th centuries BC *before* the excavation of the Hathor sanctuary in 1969 (Rothenberg 1972, Pls. 46–47; 1999, 158).

Instead of questioning the association of QPW sherds with Egyptian finds at Timna, or the security of late dates for QPW, we should be questioning the Egyptian chronology which has created the tension between the two. There is much more at stake here than the lifespan of a single pottery type. If the LBA/IA transition were no longer set at *c.* 1200 BC – a date ultimately dependent on the chronology of New Kingdom Egypt – a host of conceptual barriers would fall; old problems could be resolved and fruitful new paradigms could be developed. (On the repercussions of lower dates for the LB/IA transition, see especially James in this volume, pp. 219–248).

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An Alternative Paradigm for Israel's Origins in Canaan

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This paper takes issue with the prevailing assumption that biblical Israel emerged in Canaan around 1200 BC. This assumption is shared by several models which seek to explain Israel's origins, the most influential of which will be critically examined. Egyptian sources for Israel's presence in Canaan are discussed and it will be argued that these (both old and new) point to Israel having been present in Canaan much earlier than the end of the 13th century BC. An argument will then be made for the crisis at the end of the Middle Bronze Age as the most likely context for Israel's arrival in Canaan. Finally, some key events in the discussion will be relocated within the Centuries of Darkness *chronology (James* et al. *1991).*

Introduction

> Some scholars date the Exodus to the fifteenth century (e.g. Bimson 1991 ...). Since, however, there is no evidence for Israel in Canaan before the thirteenth century, there is no need to address this approach here. (Faust 2015, 476, n. 10.)

With these words Avraham Faust expresses a widely shared view. My purpose in this paper is to show that a strong case can in fact be made for early Israel's arrival in Canaan before the 13th century BC.

However, the date of the Exodus *per se* will not play any part in this paper since it is a topic which cannot be discussed – even in general terms – without also discussing the chronology of Egypt, which would be too vast an excursion to undertake here. Several proposals have been made which would lower the dates for the New Kingdom by varying degrees (e.g. James *et al.* 1991; Morkot and James 2009; Thijs 2010; James and van der Veen 2015). My preferred option is the case made by James *et al.* (1991) for lowering New Kingdom dates by 245 years, referred to hereafter as the *CoD* (*Centuries of Darkness*) chronology. Commenting on revisionist proposals over twenty years ago, Egyptologist Aidan Dodson remarked: 'It is now apparent to a growing number of scholars that the chronological status quo is no longer an option; far less clear is any incipient consensus as to what precisely will replace it!' (Dodson 2000, 16; similarly, 2012, ix–xii.) More than two decades later there is still no emerging consensus and most scholars still ignore the case for change.

In this paper I will avoid the use of absolute dates as far as possible. Where they are necessary, I will use conventional dates except where making specific reference to the *CoD* chronology.

1. The Rise and Decline of the 13th-Century BC Conquest Model

Over a period of about 20 years W. F. Albright constructed a synthesis of biblical and archaeological evidence for Israel's arrival in Canaan which became immensely influential. Albright frequently modified his views in light of accumulating archaeological discoveries and only a simplified account of his evolving ideas can be given here.

From an early stage Albright held that 'the account of the conquest of Palestine in the Old Testament is highly schematized and contains the record of events spread out in reality over centuries' (Albright 1924, 147). On somewhat slender biblical evidence he concluded that the arrival of the Joseph tribes in Canaan 'was attended with conquest' and placed this 'between 1600 and 1550 B.C., say about three and a half centuries before Joshua' (1924, 148). He placed Joshua in the 13th century BC, believing that the Exodus had happened during the reign of Ramesses II, *c.* 1260 BC (Albright 1920, 64).[1]

[1] In her discussion of Albright (1920), Shlomit Bechar (2022, 8) discerns 'a conundrum concerning Albright's view of the relationship between the Hyksos and the Exodus – on the one hand, he explicitly tells us that the Hyksos and the Israelites are one and the same, and on the other hand he dates the Exodus to approximately 350 years after the expulsion of the Hyksos from Egypt. This disparity cannot be solved.' But this is a misreading of Albright's 1920 paper, in which he does not state that the Hyksos and the Israelites 'are one and the same' but rather that 'We have excellent reasons for combining the Hebrew entrance into Goshen with the Hyksos invasion', and that 'the Benê Ya'qob played an important part in the Hyksos confederation' (Albright 1920, 65). Earlier in the paper, by arguing that the 430 years of Ex. 12:40 were based on 'the era of Tanis ... found on the so called Four Hundred Year Stele', he arrived at a date for the Exodus of *c.* 1260 BC (1920, 64). Albright's argument is convoluted at times but involves no insoluble 'disparity'.

Based on archaeological evidence available at the time, Albright associated the conquest by the Joseph tribes with the destructions of Jericho and Ai (identified with et-Tell), placing both in the 16th century BC (1924, 147–149). All this was to change in 1934 when he began excavating Beitin, which he believed to be biblical Bethel. This excavation coincided with a second season of excavation at neighbouring et-Tell by Judith Marquet-Krause, where results overturned Albright's opinion on the destruction of that site. It became clear that et-Tell had fallen late in the third millennium BC and had been deserted until the 12th century BC (Marquet-Krause 1949). Meanwhile at Beitin Albright's first season uncovered evidence of 'a tremendous conflagration' which ended the Late Bronze Age city in the 13th century BC. He wrote: 'We have never seen indications of a more destructive conflagration in any Palestinian excavation.' Furthermore, the cultural break between the final LBA stratum and the one following was so great 'that no bridge can be thrown across it, and we are compelled to identify it with the Israelite conquest. In reaching this obvious and inescapable conclusion, the writer abandons a position which he has held for eleven years and adopts the low date of the Israelite conquest of central Palestine' (Albright 1934, 9–10, echoed by Wright 1957, 80–81).[2] This change of stance demanded a revised view of Jericho (Tell es-Sultan). John Garstang's excavations there, begun in 1930, produced a confused picture of occupation in the LBA, but it was clear that there had been LB settlement in addition to the MBII city which had initially held Albright's interest. 'This LB occupation was much less important than the MB II city ... but it *existed*', wrote Albright (1934, 11, emphasis original), and he suggested that Jericho and Beitin had both fallen to the Israelites in the 13th century BC. However, further assessments of Jericho's LB pottery forced Albright to revise his opinion yet again, resulting in the view that LB Jericho had fallen 'somewhere between cir. 1375 and cir. 1300 B.C.' (Albright 1939, 20).

On the other hand he was able to add Tell Beit Mirsim (which he thought was probably biblical Debir) to his nascent 13th-century conquest, since his excavations there in 1928 and 1930 seemed to provide 'clear evidence that the latest Canaanite occupation ended with a general destruction and conflagration' in the second half of the 13th century BC (Albright 1935, 10; cf. 1939, 23).

In 1937 James Starkey's excavations at Tell ed-Duweir (biblical Lachish) uncovered a LB stratum (Level VI) containing, among other ceramic items, pieces of a small bowl inscribed inside and out in Egyptian hieratic. The inscription contained three dates in 'year 4' of an unnamed pharaoh. Discussing this intriguing find, Albright concluded that the 'year 4' should probably be assigned to Merenptah, corresponding to 1231 BC in the Egyptian chronology of Borchardt which Albright was then using. Finding it 'highly probable that this bowl belongs to the time just before the last destruction of Bronze-Age Lachish', he dated the destruction 'about the year 1231–30, or a very little later' (1939, 21; cf. 1937, 23–24). Albright went so far as to declare: 'This new evidence is, therefore, of decisive value for the question of the date of the main phase of the Israelite Conquest' (Albright 1937, 24).[3]

By this time Albright's speculations on an earlier entry into Canaan by the Joseph tribes were fading into the background, though they had not been altogether abandoned. In 1935 he continued to maintain that the Joseph tribes 'returned from Egypt to Palestine much earlier than the group led by Moses' and suggested they were among the Ḫabiru of the Amarna Letters and the Shasu against which Seti I campaigned (1935, 15 with n. 13; cf. 1924, 148). But by the late 1930s these ideas were eclipsed by the developing 13th-century scenario. To Albright the destructions of Beitin, Tell Beit Mirsim and Lachish – all by fire and seemingly within the same timeframe (a conclusion that would later be challenged) – made a compelling case. He wrote triumphantly: '[T]here is no doubt that the burden of proof is now entirely on those scholars who still wish to place the main phase of the Israelite conquest of Palestine before the thirteenth century B.C.' (Albright 1939, 23). Eighteen years later Yigael Yadin's excavations at Hazor (Tell el-Qedaḥ) appeared to further strengthen the case by adding a fourth city to the list of those destroyed by fire in the late 13th century (Yadin 1957, 35).

With modifications and elaborations, Albright's synthesis was championed by G. E. Wright (1957, 69, 80–83), J. Bright (1959, 117) and P. W. Lapp (1967). In spite of various weaknesses (notably the failure of Jericho and Ai to fit the 13th-century scenario) it became the dominant version of what is often termed 'the conquest model' (Chaney 1983, 44; Dever 2003, 41) of Israel's origins in Canaan.

However, by the close of the 1970s the weaknesses of the Albrightian synthesis had been laid bare by detailed critiques (Miller 1977; Bimson 1981, 30–73).[4]

[2] Albright's certainty is surprising, given (a) the absence of any hint in Judg. 1:22–26 that the house of Joseph burnt Bethel, and (b) the possibility of a destroyer other than the early Israelites. On the latter see Bimson (1981, 48–51) and sources there. For a salutary warning regarding the interpretation of destruction levels see Franken (1976, 4).

[3] From subsequent finds at Lachish, it is clear that Level VI cannot have ended earlier than the reign of Ramesses III. For details see James and Bimson in prep.

[4] My critique, though not published until 1978 (1st edition of Bimson 1981), was independent of that by Miller, being part of my PhD thesis submitted to the University of Sheffield in 1976.

It has become increasingly clear that there was no single wave of destruction in the late 13th century BC (Bimson 1991, 5–13) and some places mentioned in the conquest narratives were not even occupied at that time (cf. Dever 2003, 56–57). Hence the 13th-century conquest model has fallen out of favour to be replaced by a variety of sociological models for understanding the emergence of ancient Israel in Canaan.[5]

Finkelstein and Silberman (2001, 83) describe the scholarly consensus as having 'finally abandoned the conquest story', referring to failed attempts to locate it convincingly in the late 13th century BC (cf. Mullins 2015, 518–19). But it is important to remember that this dating has never been more than an assumption supported by questionable evidence (cf. Hoffmeier 1997, 34). The failure of the 13th-century conquest model has become an argument against a historical conquest *per se*, and this in turn has led to the biblical narratives being widely regarded as 'irrelevant as a direct historical source' for the history of early Israel (Finkelstein 1995, 351).

Hoffmeier (1996, 33) rightly observes that the arguments of the Albright school, by taking 'a moderately conservative maximalist position relative to the biblical narratives', have been detrimental to the debate. Wright, for example (1957, 80–83), presented the case for a wave of destructions around 1220 BC in a way that went beyond the claims of the biblical text. Consequently, 'The conquest model has become something of a straw man that ostensibly represented the biblical record, the latter being guilty by association with the former' (Hoffmeier 1997, 36). This has gone hand-in-hand with a minimalist position being widely adopted toward the biblical materials (e.g. Coote 1990; Davies 1992; Thompson 1999). Overwhelming priority is now generally given to a combination of archaeology and socio-historical analogies in studies of Israel's origins in Canaan.

The approach I take here is the alternative advocated by Anthony Frendo (2004, 41):

> I think that it is high time that we go back to basics, and seriously reconsider the issue of the emergence of ancient Israel in Canaan in the light of all the available evidence without unjustly preferring one type of evidence to another. Thus, the biblical evidence, the archaeological evidence, and other extra-biblical evidence should all be given their due weight. It appears that currently the biblical evidence is being treated as the underdog ....[6]

Allowing the biblical evidence 'due weight' does not mean taking an uncritical approach to the conquest narratives. The Albrightian conquest model was arguably 'doomed from the beginning because of its literal, simplistic reading of Joshua' (Younger 1999, 179). The aim of this paper is not to resuscitate it but to argue for an earlier context in which Israel's arrival in Canaan makes far better historical and archaeological sense.

2. A Contextual Approach to the Conquest Accounts

The relevant biblical narratives must be contextualised in cultural and literary terms. Several studies have helpfully embedded the book of Joshua in its ancient Near Eastern context (Niehaus 1988; Younger 1990, 2008; Hoffmeier 1994; 1997, 36–43; 2003; Hess 1997a). Younger (1990) provides the most wide-ranging and detailed comparative study, encompassing Assyrian, Hittite and Egyptian conquest accounts. By analysing Joshua 9–12 in this broad context Younger demonstrates that it 'utilizes similar literary and ideological aspects to the ancient Near Eastern conquest account ...' (1990, 237). As he summarizes in his Conclusion (1990, 265):

> This study has shown that one encounters very similar things in both ancient Near Eastern and biblical history writing. While there are differences (e.g. the characteristics of the deities in the individual cultures), the Hebrew conquest account of Canaan in Joshua 9–12 is, by and large, typical of any ancient Near Eastern account. In other words, there is a common denominator, a certain commonality between them, so that it is possible for us to speak, for purposes of generalization, of a common transmission code that is an intermingling of the texts' figurative and ideological aspects.

This common transmission code involves figurative language, stereotyped syntagms and hyperbole. In the case of the Joshua narratives these include: 'all Israel', 'the whole land', 'in a single campaign', 'left no survivors', 'totally destroyed all who breathed', etc. Younger points out that an account which uses such phrases 'is not meant to be interpreted in a wooden, literal sense'; indeed (ironically), 'The use of hyperbolic syntagms, such as "no survivors", argues against the notion of a complete, total conquest' (Younger 1990, 247).[7]

[5] It should be noted that several scholars have continued to support an Exodus and Conquest in the 13th century BC (e.g. Hess 1993; Kitchen 1998; 2003, 159–312; Hoffmeier 2007; Pitkänen 2010, 40–51, 162–169, 182–184).

[6] Also note Faust's comment, although made in a different context, that '[f]uture studies should ... pay more attention to the biblical texts ...' (2006, 232, n. 9).

[7] Josh. 10:20 provides a clear indication that such literary conventions were used by the biblical authors. It reads: 'When Joshua and the Israelites had finished inflicting a very great slaughter on them, until they were wiped out, and when the

Figure 1. Prism of Tiglath-Pileser I. Figurative language and hyperbole, as found in the book of Joshua, has been compared with typical expressions also found in Egyptian and Assyrian war annals (photo: Wikipedia Commons).

In short it would be a serious error to defend the historicity of a passage such as Josh. 10:40–43, which portrays a concerted and sweeping Israelite victory over all southern Canaan in a single campaign which left no survivors. Indeed, other passages in the book of Joshua (13:1–6; 15:63; 17:12) provide a very different perspective on Israel's entry into Canaan, listing cities and areas of the land that could not be conquered during Joshua's lifetime or later. These passages are in conformity with Judg. 1:19, 21, 27–36, often misguidedly portrayed as contradicting the book of Joshua (cf. Younger 1990, 242–243).

On the other hand it would be wrong to conclude on the basis of the language and ideology of the conquest accounts that no battles occurred and no victories were achieved; that is not how scholars read most Assyrian, Hittite or Egyptian conquest accounts, which also employ hyperbole and stereotyped language in the service of propaganda and ideology (Younger 1990, 266; Hoffmeier 2003, xxvi–xxvii; 2005, 20–22).

It has also been noted that the verbs used in Josh. 1–11 to recount Israel's attacks on Canaanite cities (*lk̲d*, 'take/capture'; *lḥm*, 'fight'; *nk̲h*, 'strike/smite'; *ḥnh*, 'besiege') do not, without further qualification, imply destruction (Hoffmeier 1997, 34–35; Frendo 2004, 46–47). In those narratives only three cities – Jericho, Ai and Hazor – are said to have been destroyed by fire. Josh. 24:13 speaks of Israelites living in cities they had captured, while Judg. 1:27–36 lists cities where Israelites lived alongside Canaanites. The point is well-made by Hoffmeier (1997, 36): 'A careful reading of the text of Joshua suggests a far more modest military outcome than those advanced by twentieth-century biblical scholars either supporting or critiquing the conquest model.'

Perhaps influenced by the overstatements of Wright and others, Redford (1992, 264) satirizes the biblical conquest narratives in the following terms: 'Cities with massive fortifications fall easily to rustic nomads from off the desert.' This is a caricature of what we learn from a careful reading of the text. As Malamat (1979; 1982) has shown, where relevant details are provided the biblical accounts rarely portray the Israelites attempting direct assault or siege tactics against Canaanite cities. Instead, indirect approaches are employed, the two main tactics being 'covert infiltration' and 'enticement' (Malamat 1979, 46–47). According to Josh. 7:2–5, a straightforward assault against Ai was a failure; a second attack succeeded through enticement and ambush (Josh. 8:3–29). According to Judg. 1:22–26, Bethel was taken through covert infiltration.[8]

In the case of Jericho, Weinfeld argues for the historicity of Josh. 2 on the basis of parallels from the early second millennium BC. He provides examples of similar reconnaissance and infiltration by spies, and notes that in a set of instructions to Hittite tower commanders 'we find that it is forbidden to build an inn (*arzana*) in which prostitutes live near the fortress wall, apparently because of the kind of danger described in Joshua 2.' (Weinfeld 1993, 142–143, quoted by Hess 1997a, 66–67.) Nur and Burgess (2008, 216–223) point out that Josh. 3:14–17 and 6:20 are understandable in light of the seismicity of the Jericho area, which could have given the Israelites an advantage over the city and its defenders. I will consider the archaeological context

survivors had entered into the fortified towns, all the people returned safe to Joshua in the camp of Makkedah.' (NRSV) Unless we assume hyperbolic language in the first half of this verse, we are faced with contradictory statements clumsily placed next to each other by an author or redactor. Hoffmeier (2003, xxv) mentions a similar case from the Poetical Stela of Thutmose III, which boasts that none of the enemy had survived but goes on to report thousands of prisoners of war.

[8] Joshua's forces are said to have besieged (*ḥnh*) only two cities, Lachish and Eglon (Josh. 10:31–32, 34–35). Both sieges are said to have been brief and successful, which may reflect the transmission code identified by Younger; on the other hand, according to Josh. 10:1–26 the armies of these cities had already been defeated in open battle, lending some feasibility to the claims in the later verses.

of the destructions of Jericho and Ai (and Hazor) later in this paper.

Many scholars who are dismissive of an Israelite conquest treat the book of Joshua as a late text which is unlikely to contain any historical information from the time it purports to describe. It is often argued that the book of Joshua was a creation of the 7th century BC at the earliest (e.g. Weinfeld 1972; Van Seters 1990; Na'aman 1994a, 281). The methodology which underpins this late dating is seriously flawed. As Hoffmeier has shown with regard to Van Seters' argument for the seventh-century date, the parallels on which it rests are drawn exclusively from Neo-Assyrian royal inscriptions while analogies from the second millennium BC are ignored. This is a highly selective application of the comparative method, skewed to produce the desired outcome (Hoffmeier 1994, summarized in Hoffmeier 2003, xxiii–xxvii).

Hoffmeier's (1994, 2003) more comprehensive analyses of parallels between Egyptian and biblical battle accounts, from the structural level down to individual motifs, lead him to conclude (2003, xxvii): '[T]he similarities in the two corpora of literature may even suggest that books like Joshua date earlier than the seventh century ... or, minimally, that the Hebrew author(s) drew on earlier (Late Bronze Age) sources.' Relevant here are the studies by Richard Hess (1994, 1996, 1997a) of non-Israelite personal names, boundary descriptions, treaties and other genres of West Semitic texts. Hess has identified numerous features in the book of Joshua which are best explained by tracing their origins to the second millennium BC (see also Pitkänen 2003 *passim*).

3. Israelite Origins without a Conquest

The Albrightian conquest model was not entirely to do with 13th-century destruction levels, though they played a major role in its formation. Albright himself led the identification of the subsequent Iron I highland culture as 'Israelite' (Albright 1937, 25). The decades around 1200 BC have since become central in the search for Israel's emergence, notwithstanding the demise of the conquest model.

As noted above, the conquest model has largely been replaced by other explanatory models which draw on a combination of sociological insights and ethno-archaeological data (for useful summaries see Chaney 1983: 41–52; Finkelstein 1988, 295–314; Dever 2003: 41–54; Mullins 2015). In spite of contrasting approaches these models all assume a connection between the emergence of Israel and the Iron Age I culture of the central hill-country. This assumption has been detrimental to a correct understanding of what the Iron I culture represents (see Bimson 2015 and below under: **In the *Centuries of Darkness* Chronology**).

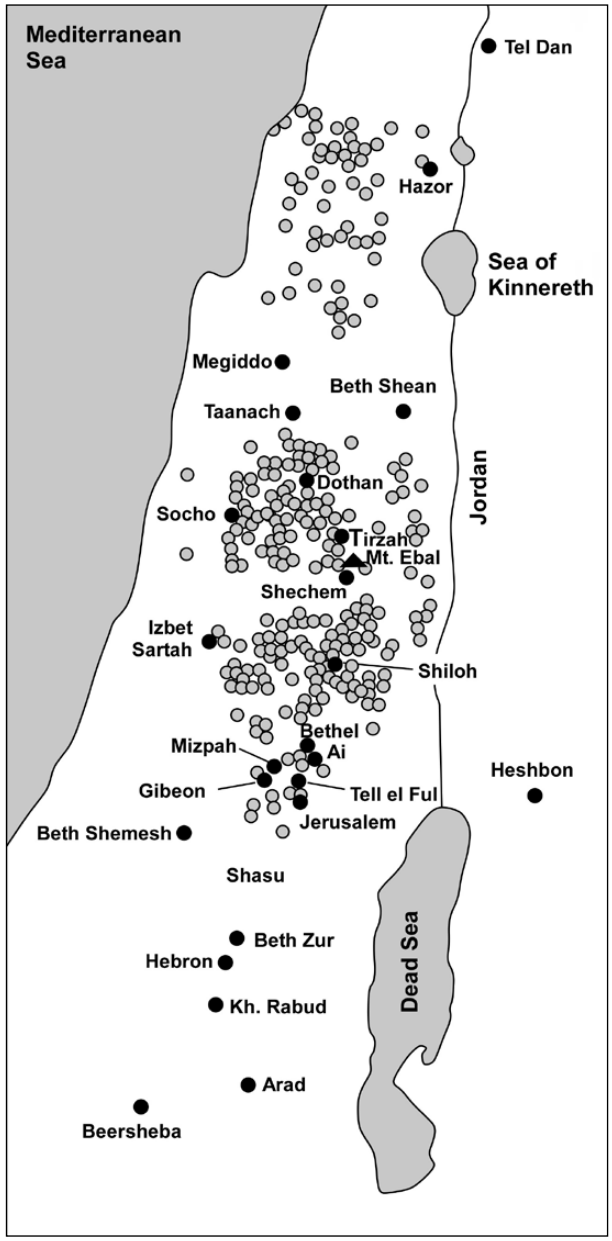

Figure 2. Map with prominent Late Bronze Age sites (black dots) and early Iron Age settlements (grey dots) in the central hill country (U. Zerbst).

The central hill-country had been only sparsely settled during the Late Bronze Age but during the Iron I period it became intensively colonized. Hundreds of small settlements appeared, stretching from the northern Negev to Galilee but with the greatest concentration in the central highlands. This is seen as a process extending over roughly two centuries, from the late 13th century to the 11th century BC, with the main increase in density occurring during the 12th century (Finkelstein 1988, 348, 353; Faust 2006, 169).

Various aspects of the material culture found at hill-country sites during Iron I (e.g. four-room houses, collared-rim store-jars, agricultural terraces and lime-plastered water cisterns) have been associated with

Israelite settlement. It has become clear, however, that these features cannot be viewed simplistically as Israelite innovations (Frendo 2004, 47–49), and debate over their significance has become deeply nuanced and complex (see Finkelstein 1988, 275–285; 1995, 364; Esse 1991; Dever 2003, 102–107, 113–117; Faust 2006, 71–84, 191–205).[9]

In spite of many decades of debate and analysis, major issues around the emergence of Israel in Canaan remain unresolved. As some theories have fallen out of favour three models have been left jockeying for pole position.

An indigenous (Canaanite) origin theory is championed by W. G. Dever, who argues that the people who settled the central hill-country in the Iron I period were mostly refugee Canaanites who had previously been settled in the lowlands (2003, 167–189). He uses the term 'agrarian frontier reform model' to describe the process which, in his view, produced the change in settlement patterns (2003, 188). In effect this is Dever's modification of the older 'peasants' revolt model'.[10]

It is important to Dever's case that he finds continuity between Canaanite culture at the end of the Late Bronze Age and the Iron I culture of the hill-country settlements. 'All the various Iron I regional assemblages develop out of general continuity with the last stages of the Late Bronze Age Canaanite culture .... The continuity is best seen perhaps in the local pottery' (Dever 2003, 218). In this cultural continuity Dever finds support for his view that the Iron I settlers, although new to the hill-country, were not new to Canaan. 'They were displaced Canaanites. For the most part, they came from various elements of Canaanite society who decided to settle the hill-country frontier' (Dever 1992a, 53). Dever calls these settlers 'proto-Israelites' and provides a lengthy defence of the term (2003, 194–200).

Other scholars have interpreted the Iron I hill-country villages as the result of groups from a pastoralist seminomadic background adopting a settled lifestyle. This school finds a cautious but eloquent defender in Faust (2006; more succinctly Faust 2015), who describes it as 'basically the original "Peaceful Infiltration school," although in a more sophisticated form' (2015, 473). The 'Peaceful Infiltration' model of Israel's emergence in Canaan began with the work of Albrecht Alt (1925), and with modifications has been advocated by Weippert (1971), Fritz (1981, 1987), Aharoni (1982, 167–180) and Rainey (1991, 60; 2003, 180–184).[11]

In its classic form the theory proposes that groups of seminomads with herds of sheep and goats migrated into the central highlands, seasonally at first but eventually settling. Their sedentarization was a drawn-out, peaceful process, but as the upland population grew and had to expand into the plains and valleys, conflicts arose with Canaanite cities. It was from such conflicts that the battle accounts in Joshua eventually developed.

On the basis of New Kingdom Egyptian texts, the seminomadic pastoralists from whom Israel originated 'are now identified mainly with the Shasu' (Faust 2006, 176–177). Shasu is a generic Egyptian term for (semi)nomads and is ethnically non-specific, though a second name can specify a tribal identity or geographical location.[12] Shasu feature in many Egyptian texts from the 18th–20th Dynasties, usually as the targets of Egyptian military action. The texts consistently portray them as unruly and hostile but they appear to have been a nuisance rather than an organised foe. Shasu were widespread, being attested in the central hill-country, Sinai, the Negev, Transjordan and as far north as Lebanon-Syria (Giveon 1971; Görg 1979; Astour 1979, 20–24; Ward 1992; Hoffmeier 2012, 118–119). Scholars who suggest the Shasu were a component of early Israel include Giveon (1971, 269–271), Weippert (1979, 32–35), Redford (1986, 199–200; 1992, 272–273), Rainey (1991, 2001, 2008) and Faust (2006).

However, scholars supporting this view do not make a simple equation of Shasu with Israelites; rather the argument is that the early Israelites were one Shasu group among several and that they probably migrated to the central highlands from Transjordan (Faust 2006, 176–177, 183, 184).

A variant of the seminomadic origin school is what Faust (2006, 177–178; 2015, 471–472) terms 'the local nomads school'. Its main proponents are Finkelstein (e.g. 1988, 336–351; 1995, 351–362) and Bunimovitz (1994), who disagree on important points but essentially see the Iron I settlement of the central hill-

[9] Faust's defence of the four-room house as an Israelite ethnic marker (2006, 71–84) provides a good illustration of this. He questions whether some of the four-room houses found outside Israelite settlement areas are in fact of that type, and suggests others lie in areas which 'can quite reasonably be regarded as Israelite'. He accepts that there are 'examples of true four-room houses outside the supposed Israelite territory', but he assigns most of these to a stage 'prior to the final crystallization of ethnic groups in the region'. When this house type, or its prototype, emerged in the late 13th and early 12th centuries BC, it was one type among many and used by several ethnic groups. During the 12th and 11th centuries its plan became increasingly uniform and 'it became very dominant in the settlements of what became Israel'. Faust calls this the 'canonization of the four-room house', a process which made it 'ethnically significant' by Iron Age II.

[10] See Mendenhall (1962), Gottwald (1979) and Chaney (1983) for theories which, in spite of major differences, fall roughly under this heading.

[11] The model's origins and development into the 1960s are usefully traced by Weippert (1971, 5–62). For a brief critique see Chaney (1983, 41–44).

[12] E.g. *š3sw n idwm*, 'Shasu band of Edom', in Pap. Anastasi VI, 55–56; see conveniently Hoffmeier (2012, 118).

Figure 3. Second stela of Seti I from Beth-Shean mentioning unruly ʿApiru *in the central Jordan Valley (photo: Wikipedia Commons).*

country as a snapshot of a long-term cyclical process in which periods of nomadization were followed by periods of re-sedentarization. At the end of the Middle Bronze Age, Canaan's urban systems suffered a collapse and the number of settlements decreased dramatically (a topic I will return to under: **Early Israel and the Middle Bronze Age Collapse** and **Causes of the MBIIC Destructions**). In Finkelstein's view this decline in the sedentary population did not result from invasions, war or pestilence but from a significant part of the population abandoning permanent settlements and becoming seminomadic pastoralists. Three centuries or more later, a different set of political and economic triggers reversed the process and the Iron I villages were the result. Finkelstein (1988, 345) refers to Egyptian sources mentioning the Shasu as evidence for 'significant groups of pastoralists' in Canaan during the Late Bronze Age.

Faust finds the local seminomads theory 'a near impossibility', chiefly on the grounds that the end of the LBA saw population movements over a wide region and the central highlands of Canaan cannot have been unaffected (Faust 2006, 177–178; cf. Na'aman 1994a, 238–239). Rosen (1992, 2009) is critical of the cyclical process theory. Dever rejects all attempts to derive the early Israelites from pastoralist seminomads (2015, 403) but reserves his strongest criticisms for Finkelstein (e.g. Dever 1992a, 79–82; 2003, 84, 153–166).

Faust in turn is unpersuaded by the Canaanite origin school (Faust 2006, 178–182; 2015, 472–473) and concludes a critique of Dever's arguments for LBA–Iron I cultural continuity by stating: 'The first Israelites may have been "local" in a loose meaning of the term, but they were most likely *not* settled Canaanites' (2006, 181, emphasis original).[13]

Clearly the debate has produced nothing like consensus but there has been a measure of convergence. All sides see the Iron I settlers in the highlands as containing, or later incorporating, indigenous elements. Hence Faust can say, on the one hand, that the first settlers were chiefly seminomads from among the Shasu, while also believing that 'many Canaanites became Israelites in the course of the Iron Age' (2015, 473). At one point he portrays Israel's origins as quite mixed (Faust 2006, 186):

> [A]s for Israel's actual origins, it seems as if ancient Israel was composed of peoples who came from various backgrounds: a semi-nomadic population who lived on the fringe of settlement, settled Canaanites who for various reasons changed their identity, tribes from Transjordan, and probably even a group who fled Egypt. In the end it is likely that many, if not most, Israelites had Canaanite origins.

Dever also envisages a variety of groups comprising his 'proto-Israelites', and his list is almost identical to that of Faust (Dever 2003, 181–182).

The following two sections of this paper will serve to put these rival theories in a larger context.

4. Israel in Merenptah's 'Israel Stela' and Reliefs

Ever since its discovery by W. Flinders-Petrie in 1896, and the publication of its hieroglyphic text by Spiegelberg in the same year (Spiegelberg 1896), there has been scholarly debate over the coda of Merenptah's 'Israel Stela' (Cairo stela no. 34025). The inscription is primarily a hymn celebrating Merenptah's victory over the Libyans in his fifth year (conventionally *c*. 1210 BC), but the final lines refer retrospectively to successes in Syria and Canaan. Three defeated city-states (Ashkelon, Gezer and Yano'am) are named, followed by two phrases boasting of victory over 'Israel': 'Israel is laid waste, his seed is not' (as translated by Wilson 1969, 378; Hasel 2008, 52).

[13] The continuity v. discontinuity debate is far from straightforward. In the matter of pottery, by placing the emphasis on *forms*, Dever (2003, 118–125) can argue for overwhelming continuity, while Faust (2006, 179–181) focuses on the pottery *repertoire* and finds significant differences (see especially Faust 2006, 180, n. 12).

Figure 4. The probable depiction of Merenptah's attack on Israel, above the depiction of the siege of Ashkelon on the temple walls at the Cour de la Cachette *at Karnak (photo: A. Schick).*

Disagreement has been rife over several issues, chiefly: the meaning of the term 'Israel', the nature and location of the entity bearing that name, and the stela's implications for understanding biblical Israel's origins.

Several scholars have concluded from the text that Israel was a politically significant entity in Canaan by the time of Merenptah (e.g. Stager 1985a, 86; 1985b; Bimson 1991, 22–23; Hasel 1994, 51 and 54; 2008, 59; Kitchen 2004; Faust 2006, 163). There has been a predictable backlash from minimalist scholars, who find this conclusion inconvenient. Criticisms range from questioning a connection between the 'Israel' of the stela and the people Israel in the Hebrew Bible (e.g. Ahlström 1991, 27–34; Whitelam 1994, 68–76; Thompson 1999, 79), to arguments that the name on the stela should not be read as 'Israel' at all (e.g. Hjelm and Thompson 2002, 13–14). Such arguments have received strong rebuttals from Egyptologists (e.g. Hasel 1994; 2008; Hoffmeier 1997, 27–31; Kitchen 2004) and need not be considered here.

The literary structure of the coda has also been debated. Hasel (1994, 47–51) reviews various proposals before providing his own.[14] Details of this debate need not detain us, but one conclusion is worth noting: '[T]he structure of the hymnic-poetic unit places the entity Israel within the territory of Canaan' (Hasel 1994, 56, n. 11). Unless we envisage two migrations from Egypt to Canaan, widely separated in time, this rules out the theories of Rendsburg (1992) and Bruce (2019) who place the Exodus in the reign of Ramesses III.[15]

The inscription's claim that 'Israel is laid waste, his seed is not' raises two issues that call for clarification. Firstly, the determinative which follows the name 'Israel' is that for a people, not that for a land or city-state (as is used with all other entities named in the coda)[16], and it has often been assumed that this indicates a seminomadic rather than a settled entity. Hence it seemed clear to Albright that Israel at the time of Merenptah, although already 'a strong and dangerous people', was 'not yet settled' (Albright 1939, 22; also Wright 1957, 71; Wilson 1969, 378, n. 18). In response to such misunderstandings Hasel (2008, 53) comments:

> The critical distinction of the determinatives indicates that Israel is a socioethnic entity. The determinative, a man and woman seated over three strokes indicating the plural, says nothing concerning whether this people is settled or seminomadic. Groups that had this designation included those who were sedentary and those who were not.

[14] This writer's argument concerning the structure of the coda (Bimson 1991, 20–23) is obsolete in light of Hasel's critique and his alternative proposal (1994, 49–51).

[15] Bietak posits 'a possible sojourn of Israel in Egypt' during the 20th Dynasty, but suggests this was concurrent with, not earlier than, the beginning of the settlement in Canaan (Bietak 2015, 30).

[16] Thus 'Israel' in the coda should not be understood as a geographical region as argued by Ahlström (1986, 37–43; 1991; 1993, 285–286; Ahlström and Edelman 1985).

Secondly, it has been disputed whether 'seed' (Egyptian *prt*) refers to human progeny or grain. Rainey (2001; cf. Wilson 1969, 378, n. 18) has argued that the phrase 'its seed is not' refers to the extermination of offspring, but Hasel (2003, 20–26) provides a detailed defence of the alternative. Careful examination of the crucial phrase, in the context of the stela and other Egyptian military texts, makes it virtually certain that Merenptah boasts of destroying Israel's grain supplies, leaving it without sustenance. This resolution also throws light on Israel's status at the time: 'From the preceding analysis it is indeed possible to suggest that Israel in 1211–1209 B.C.E. was a settled people, living agriculturally from the land' (Hasel 2003, 36; also 1994, 53–54).[17]

Discussion of Merenptah's Israel must also include a series of reliefs at Karnak which seem to provide a pictorial equivalent of the victories celebrated in the coda of the stela. The reliefs, situated on the exterior west wall of the *Cour de la Cachette*, were long believed to show military exploits of Ramesses II, but in 1978 F. J. Yurco argued that they should be attributed to Merenptah (Yurco 1978, 1986, 1990, 1997). The re-attribution was rejected by Redford (1986) but has since been corroborated by detailed first-hand inspection of the reliefs (Brand 2011).

The reliefs open with four battle scenes which, Yurco argued, show Merenptah's defeat of Ashkelon, Gezer, Yanoam and Israel. Scene 1 shows an attack on a city identified as Ashkelon in the accompanying text. Scenes 2 and 3 show Egyptian forces attacking two more cities, which are not named but logically correspond to Gezer and Yano'am on the stela.[18] If the sequence of the scenes follows that of the names on the stela, Scene 4 portrays Merenptah's routing of Israel (Yurco 1986, 205–210). Sadly, only the lower part of this scene is preserved so details are lacking.[19]

Anson Rainey (e.g. 1991, 2001, 2003) accepted Yurco's attribution of the reliefs to Merenptah but preferred to find Israel in Scenes 5 and 7–8. These show captive Shasu being bound and taken to Egypt with other prisoners. Rainey argued that these scenes portray victory over Israel as related on the stela (though it should be noted that no battle scene actually shows the defeat of the Shasu; cf. Yurco 1991). The identification of Shasu with Merenptah's Israel appealed to Rainey because he advocated the Peaceful Infiltration theory of Israelite origins (cf. Rainey 1991, 60; 2001, 66–68; 2003, 180–184). The scenes depicting Shasu prisoners therefore confirm the identity of the sedentarizing pastoralists.

Yurco (1991) responded briefly to Rainey's arguments and Hasel (replying to Rainey 2001) subsequently provided a more detailed rebuttal (see also Hoffmeier 2005, 245). Hasel demonstrates several 'major problems' with Rainey's interpretation of the reliefs and supports Yurco's (Hasel 2003, 27–36; more briefly Hasel 2008, 57). One of the problems for Rainey's interpretation is that on Merenptah's stela Israel is not referred to as Shasu, and the Shasu in the reliefs are not referred to as Israel but as 'consisting of the Shasu plundered by his majesty' (or 'among those [people] of Shasu plundered by his majesty'; Hasel 2003, 32; see also Stager 1985b, 60*; Yurco 1997, 41).

If Scene 4 shows Merenptah's troops overwhelming Israel, a significant detail is a chariot clearly belonging to the enemy forces (Hasel 2003, 32–34). After speculating that Israel may have formed an alliance with Canaanite city-states, or captured some chariots in raids on them, Yurco (1986, 212) commented: 'The presence of the chariot and its implications, as well as the fact that Merenptah considered the operation against the Israelites important enough to merit a battle scene, suggests that the Israelites at this juncture were rather prosperous.'[20]

Faust acknowledges that, 'Presently, the reliefs do pose a bit of a problem for the identification of Israel with the Shasu ...' (2006, 186). After making this remarkable understatement Faust goes on to state that on balance 'the arguments ... in favour of the semi-nomadic explanation are more sound'. He argues that Merenptah's campaign may have been 'very limited in scope' and that 'it is possible that the Egyptians had only heard of the Israelites and chose to depict them, without really knowing exactly what they were'. But it stretches credulity that the Egyptians would have devoted not merely two phrases on a stela but also a whole battle scene to the defeat of a people they 'had only heard of'.

[17] Faust, after citing Hasel 1994 and 2003 on *prt*, comments: 'This claim was refuted in a very detailed discussion by Rainey (2001, 57–66) ...' (Faust 2006, 185). Unfortunately, there is no acknowledgement that Hasel (2003, 20–26) provided an equally detailed reply to Rainey.

[18] Redford (1986, 199) implies that the other two cities are also named but Yurco is emphatic that they are not (Yurco 1990, 28 and 38 n. 14).

[19] Whether this scene could have included a city has been much discussed; see Brand (2011, 58, n. 45).

[20] To Rainey the chariot is evidence that Scene 4 shows Egypt in battle with Canaanites and not with Israel, as also is the enemy's mode of dress and general appearance. But on the latter see Hasel (2003, 34–36), where he points out that Rainey's argument depends on an assumption that Egyptian artists would have depicted Israel differently from others living within the Canaanite sphere. 'But this assumption fails to take into account the stereotypical nature of "ethnic" detail in Egyptian narrative art.' Clear distinctions were sometimes made, especially between the Egyptian army and conquered enemies, 'But we do not find artists making distinctions in appearance between every ethnic or regional group.' See also Yurco (1997, 41): 'Israelites, as Merenptah saw them, resembled the Canaanites most closely.'

After reviewing the Peaceful Infiltration and Canaanite origin models, and briefly discussing minimalist views, Hasel finds them all wanting in the light of Merenptah's stela. He asks (2008, 59):

> How could a fledgling socioethnic group just emerging in the 12th–11th centuries B.C.E. already have been known as Israel by Merenptah's scribes in the 13th century? In addition to being known and identified, how is it that they are perceived as such a serious threat that they are mentioned alongside major city-states such as Ashkelon and Gezer?

Hasel points out that anthropological studies of ethnicity have identified 'the *external* (*etic*) recognition of a group as one of the indicators of established ethnicity', and that the mention of Israel by Merenptah's scribes is a clear example of such recognition. His conclusion (2008, 59) is worth quoting at length:

> But if this is so, and Egypt recognized Israel as an entity of this sort, how much time was required for the complex processes of ethnogenesis to occur? What duration was required for Israel to actually possess the land? Current anthropological and sociological reconstructions of Israel's origin seem to overlook the complex processes required for this emergence. That is, they do not provide the necessary time or factors needed for such internal and external ethnic recognition to be achieved. In this regard, the Merenptah stela, over a century after its discovery, still cuts through current scholarly reconstructions and rhetoric with a simple declaration: Israel exists as a socioethnic people already located in the land of Canaan by 1209 B.C.E. It remains the task of future study to firmly connect Merenptah's Israel with the realities of the archaeological record in the ongoing debate over the origins of ancient Israel.

Faust is a rarity among scholars in acknowledging that the implications of the stela are at odds with his own findings. He writes (2006, 168):

> But how should we define this 'Israel' [of the stela]? Was it in actuality an ethnic group at such an early stage? After all, I have claimed earlier that the twelfth-century settlers had a totemic conscience that evolved into an ethnic one only during the twelfth century.[21]

In attempting to resolve the dilemma he points out that 'the totemic/ethnic distinction is an artificial one, and that in reality we are dealing with a continuum ...'. Furthermore, 'ethnic groups can, if circumstances change, "retreat" to forms of totemic conscience' (2006, 169, n. 2). But his discussion of the processes which might have caused such a reversal by the socioethnic Israel of the stela is entirely speculative – as it must be in the absence of evidence.

If we could free ourselves from the constraining preconception that Israel emerged in Canaan towards the end of the 13th century BC (a date which is largely a hangover from the rejected 13th-century conquest model), it might be acknowledged more readily that Merenptah's stela refers to an Israel that had been in Canaan for a considerable period before the time of Merenptah.

5. An Earlier Reference to Israel in Canaan?

In 2001 Manfred Görg proposed that a name-ring on a fragmentary Egyptian relief should be read as 'Israel' (Görg 2001; cf. Edel and Görg 2005, 86, 118). In spite of indications that the inscription is earlier than Merenptah's stela, Görg's suggestion initially attracted little attention. Since then, other scholars have supported Görg's proposal with additional arguments (van der Veen *et al.* 2010; van der Veen 2012; van der Veen and Zwickel 2014; Zwickel and van der Veen 2017; see Shanks 2012 for a popular report).

The inscription (ÄM 21687), currently kept in storage in the New Museum, Berlin, was acquired by Borchardt in 1913 and is probably part of a topographical list from a statue pedestal. It shows three Western Asiatic prisoners with name-rings below the torso. From left to right the first two names can be read as Ashkelon and Canaan (or perhaps 'Canaanites'; van der Veen 2012, 42). The third name is damaged but very probably should be read as *'Ešra'el* /*'Išra'el* or *'Išar'el* /*Yašar'el.* This reading of the third name-ring, of which about two thirds survive, was supported by 3-D laser scanning and impressions in 2011, and 'seems to be the most conclusive reconstruction by far of what can be seen on the relief' (van der Veen 2012, 42).

Pieter van der Veen and colleagues (van der Veen *et al.* 2010, 15–20) have provided detailed arguments for the view that this reading is best understood as an archaic rendering of the biblical name 'Israel'.[22] The proximity of this entity to Ashkelon and Canaan reinforces the identification with Israel: 'No known location (especially so near to those two familiar geographical entities) has a name so reminiscent of the biblical name "Israel"' (van der Veen *et al.* 2010, 20).

All three names display archaic orthographic elements, on the basis of which Görg (1974) originally proposed

[21] The term 'totemic' as used here is explained by Faust (2015, 469–470) as referring to 'the "simpler" or "local" forms of identity' in contrast to 'the broader, ethnic one'.

[22] Van der Veen *et al.* (2010) also includes a response to objections by Hoffmeier (2007, 241–242). For replies to more recent criticisms see van der Veen (2022, 26–27).

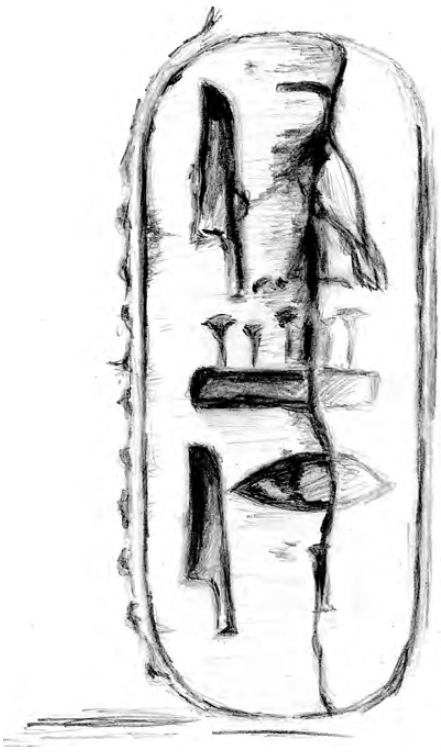

Figure 5a–b. Left: Berlin statue pedestal relief ÄM 21687 with the name rings for Ashkelon, Canaan and likely Israel (courtesy of Dr O. Zorn, Berlin Museum, photo: R. Wiskin); right: line-drawing of the third name ring with plausible reading 'Israel' (P. van der Veen).

dating the relief to the reign of Amenhotep II. Raphael Giveon (1981, *137–*139) preferred the reign of Amenhotep III, a date with which Shmuel Aḥituv (1984, 15, 69) tentatively concurred. Görg subsequently suggested a date in the reign of Ramesses II, chiefly because the names in the inscription correspond to three of the names contained in the coda of Merenptah's Israel stela (Görg 2001). But if this were the case the archaic orthography would require that the names on the Berlin fragment had been copied from an earlier source, one that originated in the first half of the 18th Dynasty (van der Veen *et al*. 2010, 17; Zwickel and van der Veen 2017, 3). Before his death in 2012 Görg returned to dating the inscription around the time of Amenhotep II, a date with which van der Veen is in agreement (van der Veen 2012, 42 and sources cited there).

If the Berlin fragment indeed contains the name 'Israel', using a form which dates from the first half of the 18th Dynasty, it would seem to indicate Israel's presence within Canaan at that early time.

This conclusion gains further support from the fact that in Egyptian topographical lists of the 18th and 19th Dynasties we encounter names which resemble those of the Israelite tribes Asher, Reuben and Issachar. These names have been the subject of earlier scholarly debate but van der Veen (2022) has provided fresh analyses of the relevant texts which strengthen the likelihood that they preserve the biblical names.

While the toponym Asher (*y-ś-r'* with variants) can in places be read as Assur, van der Veen argues that in other instances it makes better sense to read it as the biblical name Asher (2022, 27–29). A case in point is where the name occurs in a list of Seti I at Wadi Abbad. A detailed study of the associated names suggests a location in northern Canaan corresponding with the tribal territory of Asher (Josh. 19:24–31; cf. Josh. 17: 7–11).

The name *t3 š3św r'-b3-n3* (arguably 'Shasu-land of Reuben') occurs in a 19th-Dynasty toponym list at Amara West in Nubia. Van der Veen argues against Astour's (1979) suggestion to locate this and associated Shasu-lands in northern Canaan and Syria. He prefers, with Weippert (2010, 183–184) and others, a location in southern Canaan. A proposed location east of the Dead Sea (van der Veen 2022, 30, Map 3) is in keeping with the territory of Reuben as described in Josh. 13:15–23. According to Num. 32:1–5 the tribes of Reuben and Gad were seminomadic pastoralists (see also Judg. 5:15–16 on Reuben), for whom the Egyptian term *š3św* would be entirely appropriate.

The name *ś3-k3r'*, which occurs in an itinerary from the reign of Amenhotep III, has been compared with the biblical Issachar. Van der Veen (2022, 30–32) tentatively proposes an interpretation of the itinerary which takes a route through the Jezreel Valley, making the identification of the two names plausible (cf. Josh. 19: 17–23).

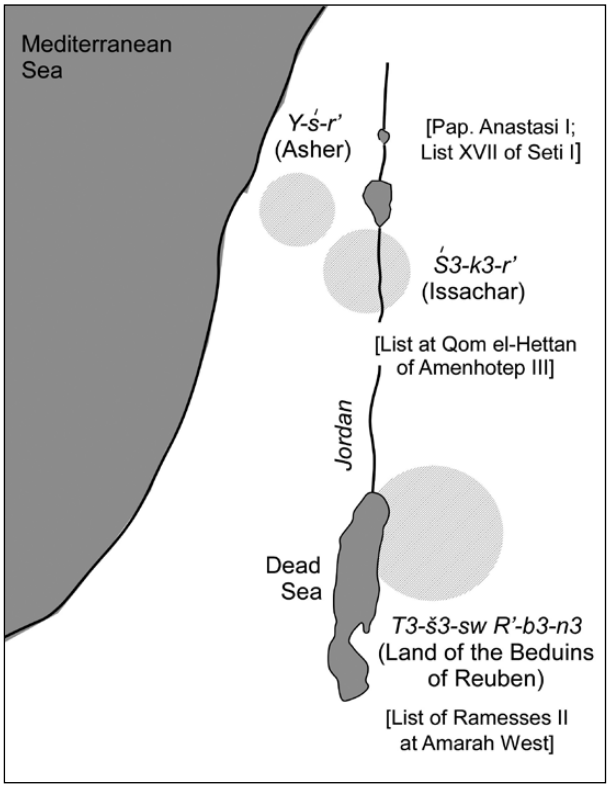

Figure 6. The map shows the location of the toponyms y-ś-r', t3 š3św r'-b3-n3 *and* ś3-k3r' *according to their mention in the relevant Egyptian topographic lists (P. van der Veen and U. Zerbst).*

In summary, it seems very likely that the name 'Israel', and probably the names of three Israelite tribes, are attested in Canaan long before the Israel stela of Merenptah. In theory, of course, it is possible that various Israelite tribes arrived piecemeal over an extended period, as Albright believed over a century ago and others have argued since (e.g. Aharoni 1982, 173–174). But this assumption is unnecessary if there is a plausible archaeological setting for Israel's arrival in Canaan at a suitably early date.

6. Early Israel and the Middle Bronze Age Collapse

In this and following sections I will argue that the most likely context for Israel's arrival in Canaan is what Bunimovitz has described as 'the great crisis at the end of the Middle Bronze Age' (1995, 323). The end of Middle Bronze IIC was marked by the destruction and/or abandonment of numerous cities and smaller settlements. As we will see below, this was probably a lengthy process with multiple contributory factors.

An influential paper by James Weinstein (1981) examined some twenty sites, from Tell el-'Ajjul in the south to Dan in the north, whose destruction or abandonment has traditionally been dated to the 16th or early 15th century BC. He also considered a further ten sites which may have come to an end in the LBIA period. Weinstein concluded that Egyptian campaigns by Ahmose and his successors in the early 18th Dynasty were probably 'responsible for at least the great majority of these devastations' (1981, 5).

Subsequent surveys of the central hill country and of Upper and Lower Galilee have added greatly to the number of sites affected. In the central hill country alone well over 200 small rural sites were abandoned in this 'severe settlement crisis' (for summary and sources, see Na'aman 1984b).

The scale of the crisis is in sharp contrast to the apparent strength of the MBIIC cities. Dever (1987, 153) observes that '[t]he proliferation of massive fortifications is the single most characteristic feature of the fully developed phases of the [MB] period.' The fortification of major towns began in the MBI phase and by the end of MBIIC almost all urban settlements were strongly fortified with a combination of embankments and walls. Dever describes the situation vividly (1987, 154):

> One element is added atop another, in an almost bewildering array and variety, as though each city tried to outdo its neighbors. Not only are all the larger cities fortified, as might be predicted, but even towns and villages as small as 2 to 4 acres are surrounded by city walls .... Indeed, scarcely a single excavated Middle Bronze Age site in Palestine has failed to yield formidable fortifications.

He goes on to note an apparent 'attempt at mass' which implies an element of 'psychological warfare' in the construction of such fortifications.

All this resonates strongly with the biblical tradition that the incoming Israelites were confronted with Canaanite cities that were 'fortified and very large' (Num. 13:28); 'great and fortified up to heaven' (Deut.

Figure 7. The formidable walls surrounding Middle Bronze Age II Hebron (photo and courtesy, J. Chadwick).

1:28; 9:1). In the Late Bronze Age, settlements were fewer and smaller (Bunimovitz 1995, 324–326; Bechar 2022, 11) and not many were fortified (Gonen 1984, 69; Herzog 1987, 272; Mazar 1993, 243). Although the number of settlements grew gradually during LB II (Gonen 1984; Na'aman 1994a, 233), that period never matched the situation at the end of MBIIC. Aaron Burke (2008, xiii) comments on the description found in Deut. 1:28:

> While there is no evident connection between this biblical tradition and Middle Bronze Age fortification systems, there is even less evidence for the fortifications of Late Bronze Age towns in the southern Levant and the data available do not reveal an impressive network of fortified settlements such as those during the Middle Bronze Age. It is curious, therefore, that the biblical tradition regarding fortified Canaanite cities finds what superficially appears to be its historical antecedent in the walled cities of Middle Bronze Age Canaan.

But this situation is not remotely curious if in fact Israel entered Canaan at the end of the MBA.

7. Causes of the MBIIC Destructions

As noted above, Weinstein (1981) argued that a great majority of the MBIIC cities were destroyed by Egyptian military action following the expulsion of the Hyksos. Others have argued for (or simply assumed) a similar cause (e.g. Wright 1961, 91; Dever 1985, 69–74; 1987, 174–175; 1992b, 13–17; Bechar 2022, 224).

However, this view has been widely challenged (Shea 1979; Redford 1979, 1982; Bimson 1981, 124–132; Bienkowski 1986, 127–128; Hoffmeier 1989, 2004; Na'aman 1994b). Hoffmeier provides the most thorough and fundamental refutation of the theory, reviewing all the relevant Egyptian texts from Ahmose to Thutmose III and undertaking a lexical study of the terms used to record military action. In light of this he reassesses the traditional view of the end of the MBA and concludes (1989, 190): 'The Egyptian textual evidence simply does not account for the widespread destruction of MB IIC Palestine.'[23]

Alternative explanations are not lacking and what follows is only a brief summary. Kenyon (1973b, 528) suggested that 'the groups of Asiatics displaced from Egypt at this stage' (i.e. at the expulsion of the Hyksos) destroyed the MB IIC cities. There is no evidence or motive for such action and her suggestion gained no following. However, Piotr Bienkowski (1986, 128) has offered a more nuanced version: 'A period of fighting and mutual destruction between the MB Palestinian towns, perhaps influenced by an influx of Asiatics from Egypt causing tension, population pressure and tribal rivalries, is as likely an explanation as any.' But his suggestion is somewhat weakened by his own argument that 'in contrast to the vast hordes sometimes inferred ..., the number of Hyksos expelled from Egypt was not all that high' (1986, 134). Several scholars have suggested that an invasion by Hurrians and other northern elements played a part in the fall of MB IIC cities (Mazar 1968, 90–91; Finkelstein 1988, 342; Na'aman 1994b; but see Hess 1997b, 153–156). Bietak has suggested that 'Shosu bedouins [i.e. Shasu] also should be considered in constructing a scenario for the late MB IIC period and the transition to the Late Bronze Age' (Bietak 1991, 62).[24]

Other scholars have shifted the focus from external forces to internal processes. David Ilan argues that in the case of the larger cities their fortifications served not only for defence but were prestige projects, proclaiming 'the power of the polity vis-à-vis other polities, and of the ruling elites vis-à-vis the ruled ... – a sort of conspicuous consumption of society's most precious resource: human energy' (Ilan 1995, 317; cf. Dever 1987, 154). In spite of their outward display of strength, by the end of MBIIC these cities were ripe for socio-political disintegration and revolt. Ilan speculates that even minor droughts and food shortages 'could have brought the socio-economic periphery into the strongholds of the chiefs and kings to pillage and burn' (1995, 314). Note that this is not far removed from Redford's comment (1992, 264), intended as a dismissal of the biblical conquest narratives: 'Cities with massive fortifications fall easily to rustic nomads from off the desert.'

Bunimovitz (1995, 323–324) writes of the situation in the central highlands: 'Thus, negative interaction (i.e. competition over natural and human resources; warfare) between the peer polities that shared the Central Hill country may explain the chronic insecurity and instability in this region as well as the "domino effect" which marked the collapse there.'

Along with a plethora of explanations comes the likelihood that the destructions were spread over a lengthy period, beginning *before* Ahmose's expulsion of the Hyksos and continuing long after (Finkelstein 1988, 342; Bietak 1991, 57–62; Dever 1992b, 13–16; Na'aman 1994b, 181, 184; Bunimovitz 1995, 322). Some cities may have suffered multiple destructions before their final demise (Dever 1992b, 13–14).

[23] See also replies by Dever (1990) and Weinstein (1991) and Hoffmeier's responses (1990, 1991).

[24] Commenting on my argument (Bimson 1981) for early Israelites as the destroyers of some MBIIC cities, Bietak (1991, 64, n. 45) remarked: 'I prefer to use the more general term "Shosu" in connection with this suggestion.'

Figure 8. Middle Bronze Age cylinder seal of unknown provenience with battle scenes and a strongly fortified city (courtesy by S. Moussaieff, photo: R. Wiskin).

That the Israelites arrived in Canaan at a time of internal tensions is implied by certain details in the biblical tradition. Thus we find the inhabitants of Canaanite cities sometimes prepared to aid the Israelites against their own rulers, as in the cases of Jericho (Josh. 2:1–21) and Bethel (Judg. 1:22–26). In other cases the arrival of the Israelites may have been a catalyst for instability; thus Gibeon became a city at war with its neighbours following its alliance with the Israelites (Josh. 9:3–27; 10:1–5). In such ways Israel's arrival could have contributed to a 'domino effect' such as Bunimovitz thinks occurred in the central highlands. In fact I would suggest that Bienkowski's theory (see above) concerning 'an influx of Asiatics from Egypt causing tension, population pressure and tribal rivalries', hence fuelling mutual destruction among MB cities, is a highly apt description of the incoming Israelites and their impact.

According to the biblical traditions the Israelites themselves – with a combination of Canaanite compliance, trickery, guerilla warfare and divine intervention – captured several Canaanite cities and in three cases destroyed them by fire. I will consider in turn the three cities the Israelites are specifically said to have burnt.

8. Jericho, Ai and Hazor Reconsidered

8.1 The fall of MBA Jericho

The first of the cities to be burnt was Jericho (Josh. 2:1–24; 6:1–25). Jericho of the conquest narratives (Tell es-Sultan) has a long history of excavations beginning in 1868 (see conveniently Bartlett 1982, 29–34). Garstang's work there (1930–36) left many unanswered questions and, as mentioned earlier, a particularly confused picture of LB occupation. Kathleen Kenyon's excavations (1952–1959) were the first to bring clarity to the stratigraphy and history of the site. Contrary to Garstang's conclusion, it was clear that there had been scant occupation during the LBA, a finding which cast serious doubt on the biblical account (see below). The MBA city, on the other hand, had been populous, typically well-fortified and had come to a catastrophic end. In Kenyon's words:

> All the Middle Bronze Age buildings were violently destroyed by fire. The stumps of the walls are buried in the debris collapsed from the upper storeys, and the faces of these stumps and the floors of the rooms are strongly scorched by fire. The destruction covers the whole area, about 52 metres by 22 metres, in which the buildings of this period surviving subsequent denudation have been excavated. That the

Figure 9. Remains of the Middle Bronze Age revetment wall at Jericho (photo by the Arbeitsgruppe für Biblische Archäologie).

> destruction extended right up the slopes of the mound is shown by the fact that the tops of the wall-stumps are covered by a layer about a metre thick of washed debris, coloured brown, black and red by the burnt material it contains; this material is clearly derived from burnt buildings farther up the mound. (Kenyon 1979, 181–182; cf. Kenyon in Holland 1981, 370.)

Kenyon's excavations also revealed evidence that an earthquake occurred at Jericho shortly before the MBA city was razed (Kenyon in Holland 1981, 370; cf. Bimson 1981, 122–124; Bartlett 1982, 94; Wood 2003, 264). As noted above, seismic activity is a feature of the Jericho area and an earthquake may well underlie the account of the collapse of the city walls in Josh. 6:20 (Nur and Burgess 2008, 67–68). Indeed a strong case can be made for one overarching natural explanation for all the phenomena referred to in Josh. 3–10, from the blocking of the Jordan and the fall of Jericho's walls to Joshua's extended day and the rain of 'large stones from heaven' at the pass of Beth-horon (James *et al.* 2022).

There is also circumstantial evidence that shortly before the earthquake and fire the inhabitants of MB Jericho 'suffered a plague' (Kenyon 1957, 255). Kenyon deduced this from a number of late MB tombs containing simultaneous multiple burials. Since the tombs had not been used again (as would have been normal practice) she inferred 'that some catastrophe caused high mortality on an occasion very late in the history of Middle Bronze Age Jericho' (1957, 254). This catastrophe was unlikely to have been the fall of the city itself because the dead were placed in the tombs with 'elaborate burial ceremonies' and 'rich equipment', an act difficult to envisage in the wake of such complete destruction. Lack of signs of injury on the skeletons ruled out enemy action and 'lavish provision of food in the tombs' excluded famine. Kenyon's presumption was therefore 'that it was some virulent disease' which led to the multiple burials (1957, 255; also Kenyon 1960, 267; Bartlett 1982, 94; Bienkowski 1986, 127). It is worth noting that the Israelite camp at Shittim, from which spies were sent to Jericho (Josh. 2:1), also suffered a severe plague according to Num. 25:1–9. The sequence of plague, earthquake and fire therefore seems to be present in both the biblical narrative and the archaeological evidence.[25]

Bryant Wood has argued that MBA Jericho was not destroyed until the end of LBI (Wood 1990, 51–53; 2003, 262–264) but on this issue he has almost certainly misinterpreted the ceramic evidence (Bimson 2005, 84–91). The most recent excavations at Jericho by the joint Italian-Palestinian expedition (1997–2000 and 2009 onwards) have found no evidence that suggests MBA Jericho survived to the close of the LBI period (Mullins 2015, 519, n. 7; cf. Nigro 2020, 201–202).[26]

Figure 10. Middle Bronze Age tomb at Jericho (courtesy of the British Museum Trustees, London; photo: P. van der Veen).

[25] Wood (1990, 53–57) presents evidence for further correspondences between the biblical narrative and the archaeology of MB Jericho's demise.

[26] Wood's attempt to extend MB Jericho's life to the end of LBI must not be confused with suggestions that the destruction of MB cities was a process that extended into the 15th century BC, with some cities falling to the first Asiatic campaign of Thutmose III (e.g. Seger 1975, mentioned in connection with Jericho by Nigro 2020, 201). Suggestions such as Seger's (cf. Bietak 1991, 58–61; Dever 1992b, 13–14; Hoffmeier 1990, 88) belong to a separate debate over the nature and timing of the end of MBIIC and will not be discussed here – except to note: (1) that in the light of Hoffmeier's arguments (1989, 186–188) it is highly improbable that Thutmose III played any role in the destruction of MBIIC cities; (2) Daphna Ben-Tor (2007, 157) discusses a cowroid (cowrie-shaped) seal/amulet from late MB Jericho Tomb G1, finding the best parallels to its design on early 18th Dynasty examples. Hence, she suggests it 'supports the continuation of the Middle Bronze Age in Palestine into the early 18th Dynasty …' Yet her case is not as strong as it may seem at first glance. While she argues 'that no such examples are *securely* dated to the Second Intermediate Period in Egypt or in Palestine' (Ben-Tor 2007, 157, emphasis added), van der Veen (pers. comm.; also van der Veen and Zerbst 2022, 102, n. 102) notes that *identical* cowroids – albeit with a *similar* decoration – are attested during the Hyksos period or MBIIC. Van der Veen (pers. comm.) further notes that a scarab with the *same* decoration was discovered by W. Flinders Petrie at Tell el-'Ajjul, who dated it to Stratum II (LAZ 1006) of the Hyksos period (Petrie 1934, 2–4, Pl. 7:

Following the destruction of the MB city the site was abandoned until limited LB occupation began on the hill above the spring. According to Bienkowski's thorough analysis of the pottery, this LB settlement lasted *c.* 1425–1275 BC (Bienkowski 1986, 136–137, 156).[27]

Yadin recognised that the imposing MBA fortifications at Jericho provide a good correspondence with the biblical narrative and suggested that the latest MB wall had continued in use throughout the LBA to be attacked by the Israelites in the 13th century BC (Yadin 1982, 22). Burke rejects the idea that the MB walls could have remained in use for three centuries without needing to be rebuilt (2008, 282; but cf. Nigro 2020, 202 on LB additions), and there is no evidence that Jericho's LB settlement was destroyed rather than abandoned, or that it survived to the late 13th century BC (Bienkowski 1986, 136–137, 156).

By contrast the burning of MB Jericho, preceded by an earthquake and probably a plague, provides close correspondence to the biblical account of the city's fall.

8.2 The problem of Ai

According to Josh. 7–8 the city taken and burnt by the Israelites after Jericho was Ai, the first city they attacked on their advance into the hill-country. It was confidently identified with et-Tell by Albright (1924), whose opinion settled the matter for the majority of scholars. Joseph Callaway, who directed excavations at et-Tell from 1964 to 1970, reaffirmed Albright's identification but, like J. Marquet-Krause before him, found no evidence of occupation between the destruction of the EBA city (*c.* 2400 BC) and the beginnings of an Iron Age village (*c.* 1200 BC).

Callaway wrote (1968, 312): 'Ai is simply an embarrassment to every view of the conquest that takes the biblical and archaeological evidence seriously.'[28] H. J. Franken notes the irony of the fact that it is the historicity of the Israelite attack on Ai which is usually considered to have been refuted, 'instead of our identification of Ai with et-Tell' (Franken 1976, 6).

The Hebrew Bible provides an unusual amount of topographical information for locating Ai. As Jehoshua Grintz noted (1961, 201), of all the conquest accounts in the book of Joshua, the taking of Ai 'is the most detailed from a tactical and topographical point of view. It is as though the author spared himself no trouble to be certain to inform us of the exact site of the place ...'. According to this information Ai lay near to Bethel (Josh. 8:17, 12:9) and to the east of it (Gen. 12:8, 13:3; Josh. 7:2, 8:9). It was also not far from Beth-aven (Josh. 7:2). A mountain or significant hill (Heb. *har*) stood north of Ai (Josh. 8:11) and a suitable site for an ambush lay west of it, between Ai and Bethel (Josh. 8:9, 12). It is therefore necessary to locate Ai in relation to Bethel, Beth-aven and the topographical features mentioned in Josh. 7–8.

The identification of Bethel with Beitin has been central to the search for Ai and claims that alternative locations for Ai have been ruled out by excavation usually depend on the Bethel = Beitin equation (e.g. Callaway 1968, 315; 1969, 5; Zevit 1985, 68, n. 4). Bethel's identification with Beitin was first proposed by Edward Robinson (1856, 449–450), who supported it with patristic evidence and on etymological grounds:

> According to Eusebius and Jerome, it [Bethel] lay twelve Roman miles from Jerusalem, on the right or east of the road leading to Sichem [Nablus] .... From Beitin to el-Bireh we found the distance to be forty-five minutes, and from Bireh to Jerusalem three hours, with horses. The correspondence therefore in the situation is very exact; and the name affords decisive confirmation. The Arabic termination *în* for the Hebrew *el*, is not an unusual change; we found indeed several instances of it ....

Albright described the identification as 'obvious' and 'one of the cornerstones of ancient Palestinian topography' (Albright and Kelso 1968, 1, 3). But it has been seriously challenged by David Livingston, who meticulously dismantles Robinson's reasoning

251; 2–4; Keel 1997, I: 380–381, 810). Keel's suggestion that it could possibly have derived from Stratum I, is but a mere guess. Petrie also found scarabs of Sheshi (14th dynasty) and Apopi II (15th dynasty) in Stratum II, and O. Tufnell therefore assigned the stratum to the 16th century BC (1993, 51). Another example with the *same* design was found in a tomb (Cave 1) at the Governor's house in Jerusalem, but as its repository belongs to LB II, the scarab may have been an heirloom (Keel 2017, 287: 17). Hence, while decoration and style continued into the early 18th dynasty (*c.* 1550–1450 BC; see also Säve-Söderbergh and Troy 1991; Pl. 11: 185/47:1G, 185/65:1A), these decorations on scarabs and cowroids could well have originated during the later Hyksos period, as the Tell el-'Ajjul specimen and similarly decorated scarabs from that period seem to indicate.

[27] Since the small settlement of LB Jericho did not survive long into the 13th century BC it poses a problem for the 13th-century conquest model. In an attempt to rescue the model, Kitchen (2003, 187) attributes the absence of suitable remains to erosion. Erosion of remains does of course occur, as it did on a large scale while Jericho lay abandoned after the fall of the MB city, producing the thick layer of ash and silt mentioned by Kenyon. If, as Kitchen argues, there was a LBIIB town which also suffered erosion it should have left its own layer of wash, albeit on a smaller scale. Failure to find the expected erosional debris from LB walls or strata during Kenyon's excavations was specifically noted by A. D. Tushingham (1953, 64; 1954, 103).

[28] The solution offered by Callaway (1968) is that the incoming Israelites destroyed the first phase of the Iron Age village at et-Tell at some point in the 12th century BC. For a critique of this theory see Bimson (1981, 57–59).

(Livingston 1970; briefly in Bimson and Livingston 1987, 47–48). Robinson's etymological argument is not conclusive because neighbouring sites can be occupied in succession and names can shift accordingly (Aharoni 1979, 123–124 provides several examples). Because of Robinson's claim that his estimated distances provided 'very exact' correspondence to the patristic sources, Livingston (1970, 33–41) pays particular attention to the details in the *Onomasticon* of Eusebius and Jerome's Latin translation and expansion of that work. He shows that Robinson's estimate of the distance from Jerusalem to Beitin was incorrect and that the true distance points rather to el-Bira (or Bireh) as the real site of Bethel (Livingston 1970, 39–42).

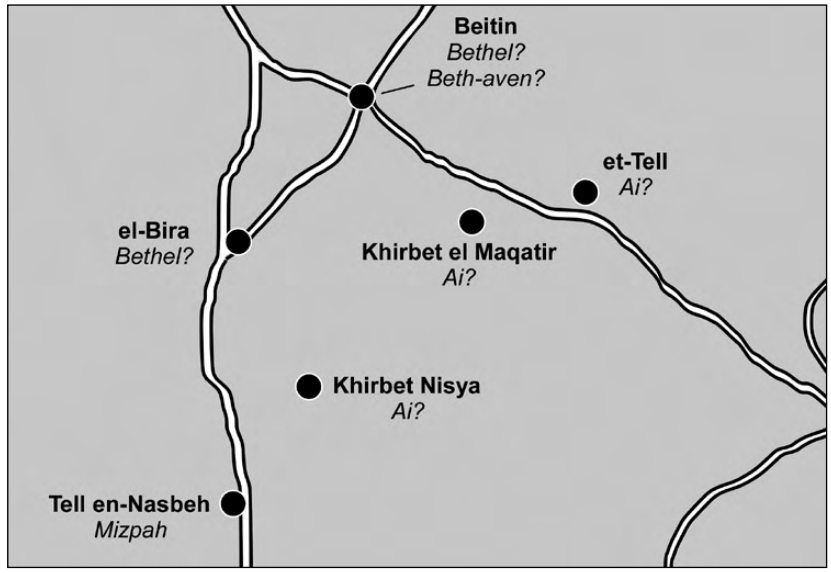

Figure 11. Map of the region north of Jerusalem, including various candidates for biblical Ai and Bethel within the heartland of Benjamin (U. Zerbst).

These and other arguments of Livingston's were challenged by Rainey (1971) in favour of Robinson's identification. Livingston defended his position (1971, 1994; cf. Bimson 1981, 205–211), and validation of his use of the patristic evidence subsequently came in a new annotated translation of the *Onomasticon* (Freeman-Greville, Chapman and Taylor 2003). Eusebius describes Bethel as 'twelve milestones away from Ailia [Jerusalem] on the way to Neapolis [Nablus]' (*Onomasticon* 40), and a careful study of the Roman milestones that have survived shows that 'el-Bira was located 12 milestones along, but Beitin would have been about 15' (Chapman and Taylor 2003, 177). Eusebius also says that Gibeon was 'about 4 milestones west' of Bethel (*Onomasticon* 66), which is 'wholly incompatible with the currently accepted identification of Bethel with Beitin' (Chapman 2003, 131); Beitin was 'at least 5.5 miles away' from Gibeon (Chapman and Taylor 2003, 177).

Because of extensive modern occupation Livingston's proposal to identify Bethel with el-Bira is very unlikely to be tested by excavation. There have, however, been several surface surveys of the town's acropolis (Ras et-Tahuneh), revealing pottery of the EBA, MBA, Iron I and Iron II as well as Roman and Byzantine periods (see Wood 2008, 220 for details). These finds increase the likelihood that el-Bira is the site of Bethel.

At Beitin the results of four seasons of excavation (1934, 1954, 1957, 1960) did not altogether fulfil expectations. According to 1 Kings 12–13, Jeroboam I (first ruler of the northern tribes after the division of the kingdom) turned Bethel into a cultic centre to rival the temple in Jerusalem. In the 8th century BC it was still 'the king's sanctuary and a temple of the kingdom' (Amos 7:13). As noted above, the first season at Beitin revealed a destruction level which Albright felt 'compelled' to identify with the Israelite conquest of Bethel (1934, 10). But he also noted in that season's report: 'In one respect, however, our archaeological results have diverged widely from the expected picture. No trace of the sanctuary built by Jeroboam I and still used in the following two centuries was found …' (1934, 3). Nor was anything of a formal cultic nature discovered during any of the subsequent seasons (Wood 2008, 217–219).

If Bethel is to be located at el-Bira, Beitin becomes a suitable candidate for Beth-aven, a place for which no convincing site has been proposed (Livingston 1994, 158; Wood 2008, 221–228). The identification is certainly 'linguistically possible' (Chapman 2003, 121).

On the basis of his new proposal for the site of Bethel, Livingston suggested that Ai could be identified with ruins known as Khirbet Nisya, about two km south-east of el-Bira.[29] Sixteen excavation seasons at Khirbet Nisya between 1979 and 2002 produced pottery and small finds compatible with Livingston's proposal, but there were no building remains that could be confidently dated earlier than the Hellenistic period (Livingston 2003). It is very doubtful that Khirbet Nisya was ever a fortified site, which makes it a weak candidate for the gated city of Ai (Josh. 7:5; 8:29).[30]

[29] Livingston (1970, 43) originally referred to the ruins as 'unnamed' but later found that on a map published by the Palestine Exploration Fund in 1883 the site is named Khirbet Nisieh, which Livingston modernised as Khirbet Nisya (see Bimson and Livingston 1987, 68, n. 44).

[30] The site lacked stratification and much of the diagnostic pottery was found in fill from late agricultural terraces. The construction of these, and a building complex from the

Figure 12. Excavations of the gate at Khirbet el-Maqatir (courtesy of S. Stripling and photo by the Associates for Biblical Research).

For these and other reasons Bryant Wood rejects Livingston's suggested location of Ai (Wood 2008, 212–214). However, he fully accepts Livingston's identification of Bethel with el-Bira and on that basis has proposed Khirbet el-Maqatir as an alternative site for Ai. Wood has provided a detailed case for this identification on topographical grounds (2008, 228–238), and excavations there (1995–2001 and 2009–2016) uncovered a fortress of about 3 acres.[31] Its walls were four metres thick, and a gate was identified on the north side, in keeping with the implication of Josh. 8:11 (Wood 2008, 230). From the pottery found at the site Wood argues that 'the fortress had a short life span, with a destruction at the end of the Late Bronze Age I' (2008, 236). He reports finding 'Abundant evidence for destruction by fire ... in the form of ash, refired pottery, burned building stones and calcined bedrock' (2008, 231).

A full independent assessment of the pottery finds and stratigraphy of Khirbet el-Maqatir must await publication of the final excavation report.[32] However, on the basis of pottery published so far (e.g. Wood 2008, 232–236) Robert Mullins has questioned Wood's claim that the fortress was destroyed at the end of LBI: 'The pottery that Wood has published from his "LB fortress" is actually MBIIC, making his settlement typical of Middle Bronze Age sites found elsewhere in the hill country at this time.' (Mullins 2015, 519, n. 8. I have tentatively expressed the same conclusion [Bimson 2010].) This is in keeping with Dever's statement (1987, 154) that even MBA towns and villages of only 2 to 4 acres were surrounded by walls.[33]

Byzantine era, may have removed any stratified remains that once existed but this can only be speculation. Livingston seems to have remained convinced that Khirbet Nisya was the site of Ai until his death in 2013.

[31] Two details in the biblical account of the attack on Ai imply it was a relatively small place. It is said to be smaller than Gibeon (Josh. 10:2), which is less than 12 acres in size, and the scouts sent by Joshua report that 'about two or three thousand men' would be adequate to take Ai because its people 'are few' (Josh. 7:3). In fact 'two or three thousand men' could be read as 'two or three units of men', depending on the translation of Heb. *'elep* (Pitkänen 2010, 172; Zerbst 2018, 101–145). On the problematic '30,000 mighty men of valour', who take part in the subsequent ambush and get close to Ai *undetected* (Josh. 8:3–7), see the suggestion of Wenham (1967, 26).

[32] The second volume (Stripling and Hassler 2023), dealing with finds from Late Hellenistic, Early Roman and Byzantine periods, has been published ahead of the first, which will cover Bronze Age and Iron Age finds.

[33] A schematic steatite scarab, depicting a falcon-headed sphinx with straight thin legs accompanied by 'ankh- and T-shaped symbols, was found above bedrock in 2013 at Khirbet el-Maqatir (pers. comm. of S. Stripling to P. van der Veen, 2022). Wood dates the scarab roughly to the time of Amenhotep II and calls it an 'Amenhotep II scarab' (as *similar* scarabs with cartouches of Thutmose III and Amenhotep II are known). His attribution is however tentative at best. Even his consultant Daphna Ben-Tor assigns the scarab more generally to between 1550–1450 BC (Wood 2013, 93), while Wood comments: 'Although the scarab was likely fashioned between 1550 and 1450 BC, it is entirely possible

What can be said at present is that, if el-Bira is the true site of Bethel (as seems altogether likely), Khirbet el-Maqatir is the most promising candidate so far for identification with the Ai of Josh. 7–8. But Wood's dating of the fortress's destruction to the end of LBI is questionable; a date at the end of MBIIC would bring it into the same archaeological context as the fall of other MBA cities.

8.3 MBA Hazor

Israel's northern campaign, culminating in the destruction of Hazor, is narrated in Josh. 11:1–13. Yadin believed his excavations at the site (1954–1958 and 1968) revealed signs of violent destruction at the end of MBIIC in both the lower city and the acropolis (or upper city). In the extensive lower city, his evidence for this consisted of a layer of ash that marked the end of Stratum 3 in Area C: 'Stratum 3 came to its end as a result of a violent conflagration, and a thick layer of ashes separated its end from the one which followed.' (Yadin 1972, 31; the ash layer can be clearly seen in Yadin *et al.* 1960, Pl. XXVI, 3.) Above this layer of ash 'a new city was built in Stratum 2. Except for one minor case in which walls of the previous stratum were reused, all the walls of Stratum 2 were laid afresh with no relation to those of the stratum below them.' (Yadin 1972, 32)

In Area K of the lower city Yadin also attributed to Stratum 3 a 'destroyed casemate-wall' connected to the gate-complex; this wall had been restored in Stratum 2 with additional courses of stones (1972, 61–62; Yadin *et al.* 1961, Pls. CXXXIX, 1; CXL, 4). In Areas A and B of the acropolis Yadin believed there had been a brief hiatus in occupation at the end of Stratum XVI (1972, 125).

However, Shlomit Bechar (co-director of the current excavations at Hazor), finds no evidence for a destruction of the MBA city. Yadin's 'thick layer of ashes' is seen merely as evidence that one building in Area C was destroyed by fire, 'but not necessarily the entire Stratum 3 in Area C, and of course not the entire stratum of the lower city' (Bechar 2022, 45). In the upper city no evidence has been found of the hiatus claimed by Yadin (Ben-Tor 2016, 78–79).

On the other hand, significant changes to the layout and architecture of Hazor took place at the MBA/LBA transition, and a detailed examination of these leads Bechar to write (2022, 70): '... The fact that a destruction level was not uncovered at Hazor within the discussed strata does not imply that Hazor did not witness a crisis.' Evidence for this crisis is not confined to the city itself. In the Galilee 'the archaeological evidence indicates that, at the very least, people left the hinterland of Hazor of their own will at the end of the MBA or the beginning of the LBI' (Bechar 2022, 235).

Within the city Bechar finds that 'the major differences occurred between Strata XV and XIV on the acropolis, and between Strata 2 and 1b in the lower city', i.e. between the LBI and LBII periods (2022, 73). However, these were foreshadowed by drastic changes in the use of space ('crisis architecture') *during the preceding strata*, i.e. during Stratum XV on the acropolis and Stratum 2 in the lower city. Domestic, administrative, and cultic spaces were all reduced in scale. In Area A in the centre of the acropolis the situation is particularly striking. Very few remains could be attributed to Stratum XV (LBI) and all were 'interpreted as a continuation of the MBII buildings' (Bechar 2022, 71).

> However, since no activity dated to the LBI was identified in the area of the Complex of Standing Stones and the Early Palace, it can be suggested that the southern part of the acropolis was not in use during Stratum XV and only the center and the road leading to it were used. This cancellation of the southern part of the acropolis can be seen as a partial abandonment, indicating a reduction of the site's occupied area. This may imply a severe population decrease ... .

Bechar concludes that during LBI at Hazor most activities took place in the lower city. But even there Stratum 2 displays 'crisis architecture':

> [T]he analysis of the architectural fabric of the city has shown that open areas were most prevalent in Stratum 2 in the lower city, suggesting a decline in the need for built structures. This, in addition to the (at least) partial abandonment of the acropolis, strengthens the conclusion ... that the city went through a decline during the LBI.

Bechar suggests this crisis at Hazor was symptomatic of an economic decline 'caused by the expulsion of the Hyksos from Egypt and probably also affected by the Egyptian military campaigns in the northern and southern Levant' (2022, 73). Two points call for comment here. Firstly, the suggested impact of Egyptian military campaigns is questionable, relying as it does on an outmoded, mono-causal explanation for the MBA settlement crisis (see under **Causes of the MBIIC Destructions**).

that it remained in use until 1406 BC.' Van der Veen confirms Ben-Tor's date but adds that very few exact parallels of the schematic type are actually known from Southern Levantine sites. As a matter of fact, similar 'sandwiched style' scarabs with schematic representations of falcon-headed sphinxes are generally at home during the MBII–LBI transition period. A similar schematic falcon-headed sphinx, also with thin legs (but accompanied by uraei), was found at Tell el-'Ajjul and likely belongs to Stratum II (Field H, 775), dated to MBIIC (Keel 1997, 327, no. 656), while another specimen was found at Pella in Tomb 62, dated to the MBII–LBI transition period (Eggler and Keel 2006, 223, no. 51). A more carefully designed scarab was found at Tell el-'Ajjul Cemetery Ib and is assigned by Keel to MBIIC–LBI (1997, 149, no. 127). (I am indebted to Pieter van der Veen for the information in this footnote.)

Secondly, a causal connection with the expulsion of the Hyksos is somewhat tenuous. It is Bechar's view that 'the Hyksos had some type of relationship with the inhabitants of the Canaanite city-states during the MBA', either 'blood ties, trade relationships, or marital connections' (2022, 14). It is certainly plausible that some level of kinship underlay cultural contact between the southern Levant and Egypt during the Second Intermediate Period (Ben-Tor 2021). As for trade relationships, Bechar bolster's her case by pointing to several Egyptian statues found at Hazor. Fragments of 19 statues have been found in all; one dates from Egypt's Old Kingdom and one probably from the New Kingdom, but the rest have been dated to the Middle Kingdom. The authors of a study of Egyptian finds from Hazor note that the MBA city was not established early enough to have had direct contact with Middle Kingdom Egypt, so 'how and when all these objects were brought to Hazor remains a matter of conjecture. They were all apparently still in use at the site during the Late Bronze Age.' (Connor *et al.* 2017, 574.) This last statement is based on the fact that most were found in the LBA destruction level (Bechar *et al.* 2021, 56); all seem to have been deliberately mutilated.

As one of the statues is probably of New Kingdom date it is possible that some of them reached Hazor during that period. Egyptian royal and private statues certainly appeared in Palestine during the 19th and early 20th Dynasties (Weinstein 1981, 20). However, the period of Hyksos rule in Egypt is considered a more likely time for the arrival of the Middle Kingdom items at Hazor. Marcel Marée writes: 'Our prime suspects are the Hyksos ... because they originated from Canaan and maintained close commercial and political relations with the region' (Marée 2017, 583). It is largely on this basis that Bechar concludes (2022, 73): 'Thus, the cessation of these relations due to the expulsion of the Hyksos would probably lead to an economic decline at Hazor.'[34]

The phenomenon of Middle Kingdom statuary being plundered and sent to Levantine city-states during the Hyksos period was widespread but is not well understood (Weinstein 1975; Ahrens 2011). Alexander Ahrens (2016) has assembled evidence that tomb robbing in the Memphis/Fayyum region and elsewhere was well-organised during that time, and that many looted objects were dispatched to the Levant via the Hyksos capital Avaris (Tell el-Dab'a and its surrounding region). Even so, motivations for this looting and dispatching of grave-goods remain opaque. From the Egyptian side it is possible that the waning power of the 13th Dynasty brought with it a collapse of normal trade links, creating a dire economic situation; trade in stolen grave-goods with the Levant and Nubia became a profitable last resort (Ahrens and Kopetzky 2021, 299). As for the Levantine recipients of these goods, it is suggested that the final disintegration of Egypt's Middle Kingdom 'left a void, which the rulers of the more powerful Levantine city-states were eager to fill. Therefore, it can be assumed that by surrounding themselves with Egyptian or Egyptianised objects they emphasised their position as Egypt's legitimate successors in the region.' (Ahrens and Kopetzky 2021, 300; cf. Miniaci 2020, 12.)

This scenario is, of course, conjectural, but no more so than Bechar's suggestion that the Middle Kingdom statues found at Hazor indicate trade relations with the Hyksos, and that the end of those relations plunged Hazor into economic decline. As yet we simply do not know what kind of relations Hazor had with Egypt during the era of Hyksos rule, or how the city might have been affected economically by the defeat and expulsion of the Hyksos.

As an alternative to Bechar's view I suggest there was an Israelite assault on MBA Hazor, resulting in a limited destruction (including the limited, perhaps token, burning evidenced by Yadin's 'thick layer of ashes' in Area C), and that this precipitated the partial abandonment and decline of which Bechar finds evidence in Stratum XV/Stratum 2. Of course this would be a lesser Israelite victory than is reported in Josh. 11:11–13, but we must keep in mind that the claims made in those verses are part of the transmission code observable throughout Josh. 9–12, characterised by stereotyped phrases and hyperbole (see under: **A Contextual Approach to the Conquest Accounts**).

The possibility of an Israelite assault on the MBIIC city is strengthened by three considerations. Firstly, Hazor reappears as a Canaanite city in Judg. 4. This makes sense archaeologically if Israel's campaign in Josh. 11 was against the MBA city, because Hazor survived its MBA/LBA crisis and the LBIA city shows cultural continuity with the MBA (Bechar 2022, 73). By contrast there was no resurgence of Hazor after the destruction of the final LBA city. This has given rise to some unconvincing attempts to make sense of the two accounts of Hazor's defeat in Josh. 11 and Judg. 4 (for critiques see Yadin 1972, 10–11; Bimson 1981, 181–185).[35]

[34] Bietak had earlier proposed a very different view. Put simply, he argued from the distribution of different types of Tell el-Yahudiya Ware that the Hyksos 'dominated only the south of Palestine and some coastal centers. The major part of Palestine ... seems to have consisted of a cluster of powerful city-states .... There is no indication that they had close ties with the Hyksos.' He even suggested they may have constructed their massive fortification systems against the Hyksos and were on good terms with the early 18th Dynasty (Bietak 1991, 60–61).

[35] It has recently become apparent that the end of LB Hazor was more complex than previously supposed. There may have been a simultaneous destruction of Str. XIII on the acropolis and Str. 1B in the lower city, following which Str. 1A of the lower city continued to exist for a few years or

Secondly, Hazor is described in Josh. 11:10 as 'formerly the head of all those kingdoms'. Malamat (1960, 12–19) argued that this was a description of MBA Hazor (see also Aharoni 1979, 149). It is far less applicable in subsequent periods; thus Millard felt obliged to write that LBA Hazor 'seems to have enjoyed *some pretence to be* "head of all those kingdoms"...' (Millard 1973, 42, emphasis added).[36] Bechar's study of the MBA/LBA transition gives new life to Malamat's argument: 'Not only do the results of this research support these claims but also offer several reasons for the changes that took place. In fact the present study suggests that Canaan's heyday was during the MBA when cities were independent city-states.' (Bechar 2022, 239; on her last point see also Wright 1961, 88; Ahlström 1993, 173; Mazar 1993, 226.)

Thirdly, according to Josh. 11:1, Hazor at the time of Joshua was ruled by a king called Jabin/Yabin. Close parallels to this name occur in the form Ibni-Adad (Akkadian form of West Semitic Yabni-Hadad), the name of a king of Hazor known from the Mari archives (Yadin 1972, 5), and in the form *Ib-ni* attested on an 18th/17th-century BC cuneiform tablet found at Hazor itself (Horowitz and Shaffer 1992). Dever, who is otherwise dismissive of the biblical conquest traditions, notes the linguistic equivalence of Akkadian *Ib-ni* and Hebrew Yabin and remarks: 'It would appear that the authors of this passage in the Hebrew Bible, however late, had *some* knowledge of an Ibni (that is, a Yabin) dynasty at Hazor, stretching all the way back to the Middle Bronze Age centuries earlier.' (Dever 2003, 68, emphasis original.)[37] In fact the discovery of the name *Ib-ni* at Hazor 'suggests strongly' to Dever that the authors of the book of Joshua 'had reliable historical sources, oral and/or written' (Dever 2003, 68; cf. Mullins 2015, 518, 519).[38]

Figure 13. Tablet fragment from Hazor with the name Ibni / Yabin (photo and courtesy of Prof. W. Horowitz, Hebrew University of Jerusalem).

The fact that the king of Hazor is called Jabin in both Josh. 11:1 and Judg. 4:2, 23–24 is unproblematic if, as the extrabiblical evidence suggests (see Dever above and Yadin 1972, 5), this was a dynastic name at Hazor.

Uncertainties remain concerning the MB/LB transition at Hazor, but I believe a case can be made for identifying an attack by early Israel as the catalyst for the crisis which Bechar has discerned in the archaeological evidence.[39]

9. Israel in Canaan during the Late Bronze Age

The theory proposed here is not unique in placing early Israel in Canaan substantially before the LBA-Iron I transition. In Aharoni's version of the Peaceful

decades before being abandoned. It is just possible that, within the conventional chronology, the extended life of the lower city might accommodate the reappearance of Hazor in Judg. 4. However, a rival interpretation of the archaeological evidence makes the destruction of Str. XIII on the acropolis contemporary with that of Str. 1A in the lower city, so that LBA Hazor in its entirety was destroyed and burnt on a single occasion. Current evidence does not resolve the difficulty (Bechar *et al.* 2021, 67–69; Runjajić *et al.* 2023, 13).

[36] Yadin (1972, 10) took the view that by the time of the biblical author 'Hazor was no longer an important city and the "beforetime" refers to its status on the eve of Joshua's conquest.'

[37] For further discussion of the biblical name Jabin and its extra-biblical parallels, see Hess (1996, 206–207) and references there.

[38] Dever's comment prompts the question: If this may be true in the case of Hazor, why not also in other cases? Of course, if the focus is on the end of the Late Bronze Age (as it is for Dever) there are simply too many sites lacking evidence for destruction, or even occupation, at the right time (Dever 2003, 54–71 with Table 4.1). In such cases the evidence obviously precludes 'reliable historical sources' for the conquest narratives. However, as I have tried to show, when the focus is shifted to the end of the Middle Bronze Age more opportunities for correlation are opened up.

[39] In Judg. 18:27 a fourth city is added to those destroyed by fire in the book of Joshua. Here it is claimed that the city of Laish was captured and burnt by the Danites, who then rebuilt it and renamed it Dan after their ancestor. (Cf. Josh. 19:47 where the city's original name is given as Leshem and burning is not mentioned.) Details in the narrative (Judg. 18:1 and 30; cf. Ex. 2:22) suggest that these events were understood to have happened early in the time of the judges (Bimson 1981, 193–196; Pitkänen 2010, 399), i.e. not long after Israel's entry into Canaan. It may therefore be significant that Laish/Dan was destroyed by fire at the end of MBIIC and soon rebuilt in LBI (Biran 1993, 326; 2008, 1687).

Figure 14. Western Asiatic travellers to Egypt depicted in the Middle Kingdom tomb of Khnumhotep at Beni Hassan in Middle Egypt (photo and courtesy E. Lessing, Vienna).

Infiltration theory an early wave of settlement took place in the fourteenth century BC with some tribes probably arriving earlier (Aharoni 1982, 174). Volkmar Fritz has tentatively placed the arrival of Israel's ancestors as early as the 15th century (Fritz 1981, 71). In Mullins' view, 'The identity of Israel as a socioethnic group was not only shaped by what took place ... as Canaan transitioned into the Iron Age, but is rooted in events extending back three centuries earlier (ca. 1500–1200 BC).' (Mullins 2015, 522; cf. Lemche 1985, 416.)

As we have seen (under: **Israelite Origins without a Conquest**) it is widely believed that early Israel had seminomadic origins. Although this view is normally embraced by scholars who reject the historicity of the conquest traditions, there is no obstacle to combining it with an acceptance of those traditions in their broad outline.

That early Israel's ancestors were seminomadic pastoralists is an almost unanimous tradition in the Hebrew Bible (e.g. Gen. 30:31–43; 32:13–21; 37:12–16; 46:31–34; Ex. 12:38). We have seen above that according to Num. 32:1–5 the tribes of Reuben and Gad were pastoralists who requested appropriate territory on arriving east of the Jordan. We find the Reubenites still following that lifestyle in Judg. 5:15–16. The Kenites, who arrived in Canaan with the Israelites according to Judg. 1:16, are still (at least in part) tent-dwellers in Judg. 4:11.

The rallying cry attributed to the rebel Sheba ben Bichri in the time of David – 'Every man to his tents, O Israel!' (2 Sam. 20:1) – hints at a long seminomadic past, and perhaps at a lifestyle many were still following. The cry is echoed at the division of the kingdom after Solomon's death (1 Kings 12:16). According to 1 Chron. 4:39–41 some of the descendants of Simeon were wandering pastoralists in the time of Hezekiah, and in Jer. 35:1–11 we find the ultra-conservative Rechabites eschewing all trappings of settled life around 600 BC.

If some segments of early Israel were seminomadic pastoralists, the Egyptian term Shasu (as noted above, a generic, non-ethnic appellation) would have been entirely appropriate for them. In addition to Finkelstein and Faust several other scholars have suggested a connection between early Israel and one or more Shasu groups (for references see above under: **Israelite Origins without a Conquest**).

We noted above (following van der Veen 2022) that the Egyptian *t3 š3św r'-b3-n3* can be understood as 'Shasu-land of Reuben'. Another significant Shasu toponym occurs in the list from the temple of Ramesses II at Amara West, copied from an earlier list in Amenhotep III's temple at Soleb, namely *t3 š3śu yhw3*, 'Shasu-land of Yahwa'.[40] The last element corresponds linguistically

[40] Amara West no. 97 = Soleb List 4, A2.

to Yahweh, the name of Israel's God (Hoffmeier 2005, 242; 2012, 118), and has therefore sparked speculation that it may refer to territory where a cult of Yahweh flourished. Its proximity in the lists to *t3 š3śu se-ʿ-r-er*, which has been read as 'Shasu-land of Seir', suggests a location in southern Transjordan, north-eastern Sinai or the Arabah, since biblical Seir seems to have been close to, or even part of, Edom (Gen. 32,3; 36,8–9). Some poetical texts in the Hebrew Bible (e.g. Deut. 33:2; Judg. 5:4–5) imply the worship of Yahweh originated in that area, and that it continued to be thought of as in some sense Yahweh's base of operations. It has therefore been suggested that 'Shasu-land of Yahwa' might link some Shasu groups with early Israel (e.g. Rainey 2003, 180; Faust 2006, 477, n. 12).

An objection to this theory is that biblical Seir is normally spelt in Egyptian texts with one '*r*', not two as in *se-ʿ-r-er.* This led Astour (1979) to identify *se-ʿ-r-er* with an alternative toponym Šeḫlal, which would locate *t3 š3śu se-ʿ-r-er* and associated Shasu-lands (including 'Shasu-land of Yahwa') in Syria or Lebanon (see Hoffmeier 2005, 242–243; 2012, 118–119). But several scholars have argued for a location in southern Canaan, regarding the spelling *se-ʿ-r-er* as simply defective (see van der Veen 2022, 29–30 with nn. 35, 36 and 39). On balance, a southerly location for these Shasu-lands is very probable and a link with some part of early Israel therefore seems likely.

Can we expect to find archaeological evidence of seminomadic Israel prior to settlement? Finkelstein has argued that the Canaanites who adopted a pastoral lifestyle because of the crisis at the end of the Middle Bronze Age would have been almost invisible archaeologically, but he does offer indirect archaeological evidence for their existence (e.g. 1988, 342–345; 1995, 355–356). Steven Rosen (1992) has criticized Finkelstein on the matter of archaeological invisibility, but Finkelstein has a recent ally in Erez Ben-Yosef who writes (2020, 36): 'It is difficult to detect nomads in the archaeological record ... and even if some remains, such as pens and tent clearings, are detected, it is extremely challenging to precisely date them.' Going further he states: 'There is compelling evidence indicating that nomads can be *entirely* archaeologically invisible' (Ben-Yosef 2021, 158, emphasis original; cf. Finkelstein 1992).[41]

Ben-Yosef issues a broad challenge to the way archaeologists have traditionally reached a view of mobile societies, 'mostly based on Bedouin ethnography ... and inferences from ancient Near Eastern archives' (2021, 156). He opines that the prevailing 'rather flat perception of nomadic social organization includes an underlying assumption that increased social complexity and the creation of strong political bodies in the southern Levant were exclusively phenomena of the settled' (Ben-Yosef 2021, 156). He is also critical of the 'ultra-rapid sedentarization process' normally deduced from the Iron I hill-country sites, and complains (2021, 170): 'The other possibility, of a slow, multi-generation sedentarization process of a polymorphous society that had substantial nomadic components even during the early monarchic period has never been seriously considered.' In his view (2021, 170, n. 14) nomadic elements continued to exist within Israel 'most probably all through the Iron Age' and he refers in that connection to 'the interesting case of the House of Rechab' (on which see also above).

The term 'polymorphous society' is adopted by Ben-Yosef (from Lemche 1985) to describe Bronze and Iron Age societies which included settled and nomadic elements, because 'it takes into account the countless possible situations along the sedentary–nomadic continuum' (Ben-Yosef 2021, 159). It therefore seems an entirely appropriate term for describing LBA Israel.

Should we expect the early Israelites to have displayed a distinctive material culture as they began to settle down? Faust (2006, 180, n. 12) addresses this question in the context of the early Iron I villages:

> If the settlers were Shasu, what kind of pottery do we expect them to have produced: Egyptian? Mesopotamian? They were in contact with the Canaanite society throughout the Late Bronze Age, and these [Canaanite pottery forms] are probably the forms they were familiar with. If these simple forms were not seen as meaningful, and they probably were not, there would have been no problem in using them. Moreover, they were probably the only forms the new settlers knew, and they had to use something.

Although Faust is assuming settlement at the beginning of Iron I, the same would be true in earlier periods. The point is that if we allow for a period of contact with Canaanite society, as called for by Fritz (1981) in his 'symbiosis hypothesis', we would not expect Israelites who sedentarized during the LBA to produce distinctive pottery. According to Judg. 1:27–33 Canaanites remained dominant in many places and the early Israelites settled alongside them. On our current understanding of the symbiotic relationships between seminomads and settled societies (the former being economically dependent on the latter), even tribes or clans which remained seminomadic would necessarily have had contact with urban Canaanites (cf. Judg. 4:17 on the clan of Heber the Kenite and Hazor).

In spite of its familiar rhetoric against Canaanites, the Hebrew Bible reports cases of Canaanites and other non-Israelites being incorporated into Israel (e.g. Rahab and her extended family in Josh. 6:25). A careful

[41] Note, however, that on other matters Ben-Yosef (2020) has points of serious disagreement with Finkelstein.

study of biblical data by Pitkänen (2004, 174–182), using comparative studies of ethnicity and assimilation, finds several examples. Pitkänen summarises (2004, 174–5):

> While the Exodus group settles, individual Canaanites join the group and adopt Israelite customs, become grafted into Israelite genealogies and adopt Israelite beliefs of common ancestry and history. Also, through conquest or voluntarily, some highland cities are similarly assimilated to Israel, though other cities stay independent. This process continues through centuries, expanding beyond the highlands during the early monarchy. Thus, Israel is born in Egypt and grows in and from Canaan.

Early Israel's shift to a settled agricultural existence is unlikely to have been uniform. Modern examples suggest that the wealthier, leadership strata may have been the first to adopt settled lifestyles, with others then finding sedentarization easier as a consequence (Lemche 1985, 143–144 and more generally 136–147). This may have been the case among Israel's 'judges' (at least some of whom were wealthy if traditions concerning family-size are anything to go by: Judg. 10:3–4; 12:8–9, 13–14). On the other hand, as Stager (1985a, 85) points out, 'So long as the Late Bronze markets and exchange networks were still operating, the sheep-goat pastoralists would have found specialization in animal husbandry a worthwhile occupation.' Such may have been 'the clans of Reuben' (Judg. 5:15–16) at the time of Deborah (cf. Stager 1988).

In a provocative essay Brendon Benz (2015) looks for 'historical data that point to the insider status of some of the early Israelites …' and finds suggestive parallels between examples of 'corporate political activity' in the 14th-century Amarna archives and incidents in the book of Judges.

The portrait of Israel in the Song of Deborah (Judg. 5) stands out against the more usual picture of united tribes. The poetic roster includes most of Israel's familiar constituents but does not mention Judah or Manasseh and adds Machir (Judg. 5:14; cf. Num. 32:39–40). Most striking is the number of tribes and other entities which are chided for their non-involvement in the coalition against Hazor (Judg. 5:15–17), including the city of Meroz (Judg. 5:23). Benz comments (2015: 463):

> The depiction of Israel in Judges 5 closely corresponds to the nature of the multi-polity decentralized lands attested in the Amarna letters. As was the case with the members of the land of Gina [in EA 365], the constituents of Israel had the autonomy to choose whether or not they would participate in the collective.

Abimelech's bid for kingship over Shechem (Judg. 9:1–6) is an incident which seems out of place in Israel's pre-monarchic era. Benz notes that, because of its jarring nature, some scholars suggest Judg. 9 'is a Canaanite rather than an Israelite narrative', but comments: 'However, if the political structure of early Israel reflected that of the Late Bronze Age multi-polity decentralized lands, the idea of a monarchy within Israel before the establishment of the Israelite monarchy would not be out of place.' (Benz 2015, 464.) Benz also compares the eventual rejection of Abimelech by the leaders of Shechem (Judg. 9:22–57) with the Gublites' rejection of Rib-Addu in the Amarna correspondence (EA 138).

As a result of the comparisons he draws, Benz finds 'several striking points of continuity between some of the biblical accounts of Israel's formative stages and the socio-political landscape of the Late Bronze Age …' (2015, 463). These accounts are unlikely to have been created by Judean scribes of the exilic or post-exilic periods, as is often supposed. Rather they suggest 'that at least a proportion of the early Israelites were heirs to the social and political structures of the Late Bronze Age Levant' (2015, 464). Benz refers to this component of early Israel as 'geographical, economic, and political insiders', distinct from the 'geographical outsiders' responsible for the Exodus tradition, and from 'economic and/or political outsiders'. This distinction is of course unnecessary if the group contributing the Exodus tradition was in Canaan from the start of the Late Bronze Age. Many of them would quickly have become 'insiders' in Benz's use of the term, while others could have remained 'outsiders' in economic and political terms.

10. The *'Apiru* Question

Across a wide spectrum of theories explaining Israel's origins there has been much speculation concerning the possible roles of the *'apiru* known from Egyptian texts and the *ḫabiru* of the cuneiform Amarna Letters. The latter entered scholarly debate with the discovery of the Amarna archive in 1888 but the *'apiru* had been known since 1862. It seems to have been W. M. Müller who first connected the two (Rainey 1987, 540). For convenience I will use the term *'apiru* throughout this discussion (cf. Weippert 1971, 3, n. 4), except when quoting scholars who prefer one of the many other possible renderings (see Doak 2011, 2, n. 1 for various permutations).

In the late nineteenth and early twentieth centuries there was much enthusiasm for identifying the *'apiru* of the Amarna Letters with the biblical Hebrews (*'ibrim*) entering the land under Joshua. From the outset this skewed the interpretation of the letters, leading some scholars to view the *'apiru* as recent invaders from outside Canaan (e.g. Jack 1925, 19, 44, 129). In

Figure 15. EA 245 of Biridiya of Megiddo concerning the revolt instigated by Labayu of Shechem, one of the ʿapiru *troublemakers in the region (courtesy of the British Museum Trustees, London; photo P. van der Veen).*

fact, the letters provide no evidence for this view (notwithstanding the attempt of Waterhouse 2001, 40–41 to revive it).

The identification of *ʿapiru* with *ʿibrim* was fuelled by the publication of letters written by Abdi-Heba, ruler of Jerusalem (EA 285–290). His complaints that *ʿapiru* had plundered lands and taken cities provided a superficial resemblance to the activities of Joshua's troops. The identification was initially attractive because it seemed to confirm at a stroke the historicity of the conquest narratives and the early date for the Exodus (suggested by 1 Kings 6:1). But it was soon noticed that *ʿapiru* were only mentioned in Amarna correspondence from southerly cities, while the Hebrew Bible has Joshua's army campaigning further north (notably against Hazor). Winckler (1895, 18) appeared to solve this problem by identifying the *ʿapiru* with people designated logographically in the letters as SA.GAZ, whose activities were more widespread. Albright and others (e.g. Moran and Rainey) accordingly proceeded to translate all the SA.GAZ references in the letters as *ʿapiru*. But Winckler's theory raised its own problems. The SA.GAZ are conspicuous as troublemakers in the far north, harassing the ruler of Byblos and Simyra for example – in a region not affected by the Israelite conquest. Once again, the identification of *ʿapiru* and *ʿibri(m)* had skewed the interpretation of the letters. While some texts from Boghazköy (the Hittite capital Ḫattuša) do make *ʿapiru* and SA.GAZ equivalents, there are no grounds for assuming that this should be applied to every instance of SA.GAZ. In short, not all SA.GAZ need have been *ʿapiru*, to re-purpose the adage (e.g. Chaney 1983, 56) that 'not all *ʿapiru* were "Hebrews"'.[42]

A direct equation between the *ʿapiru* of the Amarna correspondence and Israelites who had recently entered Canaan under Joshua is still defended by some conservative scholars (e.g. Waterhouse 2001; Wood 2003, 269–271). Notwithstanding difficulties (cf. Rainey 2008, 52–53), the argument can certainly be made within the conventional chronology. On the other hand, the *CoD* chronology lowers the Amarna period to the decades around 1100 BC, not long before the beginning of the Israelite monarchy. This makes a direct equation of Amarna *ʿapiru* with Joshua's Israelites all but impossible, as it would require a very late date for Israel's entry into the land (see further below).

The debate over a possible connection between *ʿapiru* and *ʿibri(m)* is ongoing and the literature generated is vast.[43] Whether there could be a linguistic connection between the two terms is particularly contentious, with some scholars finding a connection plausible (e.g. Weippert 1971, 74–82; Na'aman 1986, 278; Lemche 1988, 89; Davies 2004, 33), and others strongly disagreeing (Kline 1957, 54–61; Borger 1958; Rainey 1987). Rainey has repeatedly denounced such a connection as impossible. In one of his strongest condemnations of the idea he describes attempts to relate *ʿapiru* and *ʿibri* as nothing short of 'silly'; 'absurd mental gymnastics' on the part of 'wishful thinkers who tend to ignore the reality of linguistics' (Rainey and Notley 2006, 89; cf. Rainey 2008, 53; also 1987, 541 and 2003, 174 for his technical arguments).

In addition to the linguistic difficulties the main objection to a close association of the terms is that while *ʿibri(m)* is an ethnicon, *ʿapiru* is a sociological term (Rainey 1987, 539; Dever 2003, 74; cf. Weippert 1971, 83). *ʿApiru* are referred to in over 250 sources from Egypt, Mesopotamia and Anatolia, spanning the 19th–12th centuries BC (Doak 2011, 15). Broadly speaking the term seems to have three uses (Lemche 1985, 421; cf. Doak 2011, 16–17): a) it can refer to outlaw bands and social outcasts, people uprooted from tribal or urban society who live beyond the control of the city-states; b) it can refer to fugitives or refugees and c) in the Amarna Letters it takes on a pejorative sense for groups, cities

[42] I am indebted to Peter James† (pers. comm.) for the insights in the foregoing paragraph.

[43] See for example the extensive references in Weippert (1971, 74–82) and Doak (2011, 4–5).

and rulers opposed to the Egyptian administration (or those whom loyalists wished to portray as such).

However, defining *'ibri(m)* as an ethnicon and *'apiru* as a sociological appellation is challenged by some as an oversimplification (e.g. Chaney 1983, 56–57). Regarding the occurrence of *'ibri(m)* in the book of Exodus, Graham Davies asks (2004, 33, emphasis original):

> But is it so clear that 'Hebrew' in Exodus is an ethnic term? It seems to me quite possible that there too it actually has a *social* meaning and that it can be dissociated from any particular ethnic identity. If so, there is probably more significance than is usually recognized in the coincidence of nomenclature between what seems to be an early stratum of the Exodus tradition and the references to *'apiru* in Egypt in the New Kingdom period.

Nadav Na'aman (1986) and Brian Doak (2011) have both compared the behaviour of Amarna *'apiru* with that of parasocial bands and their leaders in the books of Judges and 1-2 Samuel. Both refer to the activities of Abimelech (Judg. 9), Jephthah (Judg. 11) and the migrating Danites (Judg. 18). In addition, Na'aman gives close attention to David's time as an outlaw leader (1 Sam. 22–28; cf. also Na'aman 2010) and to the rebellion of Sheba ben Bichri (2 Sam. 20). These studies leave no doubt that on the sociological level the stories in the books of Judges and 1–2 Samuel provide examples of *'apiru*-like behaviour. Doak (2011, 6) comments: '[T]he narrative of the book of Judges may be read as the most sustained literary product in the ancient Near East depicting a world of habiru-like actors generating political transformation.' Even Rainey, in the same work in which he most strongly condemns any linguistic connection, writes that 'sociologically, it can be said that Jephthah and his militia had become like the Late Bronze Age 'apîru men' (Rainey and Notley 2006, 140).

Like Davies, Na'aman thinks a social meaning underlay the term *'ibri(m)* (1986, 2010). 'With their status as uprooted people living on the margins of society, the bands described in the books of Judges and Samuel are identical to the Ḫabiru of the ancient Near Eastern texts' (1986, 285). From this observation he unfolds a lengthy argument for the evolution of the ethnicon *'ibri* from the appellation *'apiru* (Na'aman 1986, 288).

Many scholars accept that *'apiru* may have been an element of early Israel to a greater or lesser extent (e.g. Lemche 1985, 427–432; 1988, 85–90; Ahlström 1986, 15; Dever 2003, 181; Faust 2006, 184). Thus Faust (2015, 475) allows some *'apiru* a role in his theory of Israel's Shasu origins: 'These seminomads came, most probably, from among the Shasu groups (perhaps including small groups of "local" 'Apiru, or outcast Canaanites). This is, most likely, the core of Merneptah's Israel.'

I will return to the '*apiru* question in the next part of this paper.

11. In the 'Centuries of Darkness' Chronology

Here I will restrict discussion to four topics which have featured in this paper. Each one will be dealt with briefly and without any claim to have offered a definitive conclusion.

11.1 The date of early Israel's arrival in Canaan

On the outmoded view that many MBIIC cities fell to Egyptian armies in a short period at the start of the 18th Dynasty (*c.* 1550 BC) their fall could be dated fairly precisely to the middle of the 16th century BC. As we have seen, many scholars now envisage a more prolonged affair. Bunimovitz (1995, 322) remarks: '... the settlement crisis at the end of the Middle Bronze Age seems to be a continuous process, which had already begun at the end of the seventeenth century BCE and lasted to the end of the sixteenth century BCE. The early destructions, and at least some of the later ones, may therefore be related to internal instability and conflicts rather than to Egyptian military campaigns.' (See also Na'aman 1994b, 181.)

These dates are ultimately linked to Egypt and so need revision if the *CoD* chronology is correct. This would lower the beginning of the 18th Dynasty by around 250 years. Translating the dates estimated by Bunimovitz into *CoD* terms (i.e. subtracting *c.* 250 years from, say, 1610 BC and 1510 BC) we could suggest that the settlement crisis had already begun *c.* 1360 BC and lasted to *c.* 1260 BC. In this revised context what may we say about the time of early Israel's arrival in Canaan?

A conservative ('maximalist') treatment of the Hebrew Bible's chronological data (1 Kings 6:1; Judg. 11:26; Num. 14:32–34) places the Exodus at 1446 BC and the beginning of the conquest of Canaan at 1406 BC (e.g. Wood 2007). Hoffmeier (2007, 235–239) and others have suggested that the 480 years mentioned in 1 Kings 6:1 (the period between the Exodus and Solomon's fourth year) should be regarded as symbolic rather than literal. The figure plays no part in Hoffmeier's dating of the Exodus and Conquest since he prefers a 13th-century date for those events (Hoffmeier 2007, 226). Wood (2007, 253) protests that Hoffmeier ignores an argument by Cassuto 'which demonstrates that the 480 years in 1 Kings 6:1 should be understood as a precise, not a symbolic, number.' It is unclear to this writer why a precise number cannot also be symbolic; in fact, precision might lend power to the symbolism.

As yet we have no clear date for the beginning of the MB settlement crisis, nor do we know how lengthy a process it was, or when within that process the cities of Jericho, Ai and Hazor were destroyed. A date for their destruction within the first half of the 14th century BC

Figure 16. Ivory carving from Late Bronze IIB Megiddo, depicting among others a ruler standing in his chariot with two Shasu prisoners and the same king seated on his throne, flanked by sphinxes (photo courtesy Eye2EyeMedia).

would not be far from the biblical date, but a later date may well turn out to be necessary.[44]

11.2 'Apiru again

In the conventional chronology the Amarna Letters were written roughly 1360–1330 BC. In the *CoD* chronology that thirty-year period is redated to *c.* 1110–1080 BC, locating the Amarna correspondence late in the period of the biblical judges. The *CoD* revision therefore places a long period between the time of Joshua and the Amarna Letters (the exact length of that period being dependent on the date given to Israel's arrival in Canaan).

As noted above, Na'aman, Doak and Benz have all found close similarities between the actions of the *'apiru* of the Amarna period and those of parasocial groups in the books of Judges and 1–2 Samuel. Although these authors are not attempting to redate the Amarna Letters to the time of the biblical judges, their conclusions are entirely consonant with the dating of the Amarna Letters in the *CoD* chronology. Arguably the *CoD* revision allows us to identify the Amarna *'apiru* with biblical *'ibri(m)* in a more satisfying way than is allowed by the conventional chronology.

11.3 Israel in Merenptah's reliefs

If we accept the interpretation of Merenptah's reliefs advocated by Yurco and Hasel, Scene 4 at Karnak provides evidence that Israel was in possession of chariots by the time of Merenptah. This has been a surprising discovery within the conventional chronology because of the prevalent view that Israel had only recently arrived in the land or (even more troubling) was still in the process of emerging from either Shasu or displaced Canaanites.

An Israel equipped with chariots is perfectly at home in the *CoD* chronology, in which Merenptah's clash with Israel belongs to the 10th century BC (James *et al.* 1991, 386; James 2015, 236–257; van der Veen and Ellis 2015, 264–273). There are hints that David may have had a small chariot force (2 Sam. 8:4; 15:1) and by the time of Solomon chariots were a significant part of Israel's army (1 Kings 9:19; 10:26).

The Shasu in Scenes 5 and 7–8 at Karnak are not a problem in a 10th century context if we heed Ben-Yosef's comment that 'a slow, multi-generation sedentarization process of a polymorphous society that had *substantial nomadic components even during the early monarchic period*' should be considered (Ben-Yosef 2021, 170, my emphasis). However, I am not suggesting that the Shasu in Merenptah's reliefs were part of Israel (the absence of the name 'Israel' from Scenes 5 and 7–8 would be as problematic for that view as it is for Rainey's); my point is simply that Shasu do not pose a problem as contemporaries of early monarchic Israel.

11.4 The LB-Iron I transition

In the *CoD* chronology the Solomonic era is to be aligned with the last phase of the LBA (James *et al.* 1991, 197–203; 2015). This requires a radically different view of the changes which took place at the beginning of Iron I. I have argued (Bimson 2015, 99–104) that the LB–Iron I transition marks the division of the kingdom after Solomon's death (1 Kings 12). This

[44] It may be objected that if early Israel was in Canaan before the beginning of Egypt's 18th Dynasty, we would expect clashes to have occurred during that dynasty between Israelites and Egyptian troops and to find this reflected in the biblical settlement traditions. But as Hoffmeier points out when arguing that Egypt's armies did not destroy MBIIC cities, we do not find 'the slightest hint in the Egyptian literature of the first century of the New Kingdom that they were in the hill country of Palestine' (Hoffmeier 1989, 190; also Na'aman 1994b, 183). Since the hill country seems to have been the main area of initial Israelite settlement (e.g. Josh. 8:30–9:27; 18:1; 24:1; Judg. 1:2–25; 9:1–49), the early Israelites were well away from Egyptian incursions during that time.

momentous event caused the collapse of Solomon's trading network and hastened the decline of cities dependent on it. Many people were forced to find alternative lifestyles and exploit new areas to survive. The rebellion of the northern tribes was not simply the rejection of Solomon's son Rehoboam, but a turning away by many from the values of an exploitative and oppressive regime to a more egalitarian way of life. This transition, chosen by some but an economic necessity for others, is what we find reflected in the Iron I highland settlements.

In this context the level of cultural continuity for which Dever argues is entirely to be expected, but so are the observed changes in the pottery repertoire (Bimson 2015, 101–102). The repertoire of the highland villages is limited to a few forms and imported and decorated wares are absent. We can agree with Faust (2006, 69) that 'this limited repertoire sent internal messages concerning the society's values, namely, simplicity and egalitarianism.'

One of Faust's main objections to Dever's theory of indigenous origins is that it does not explain the process by which Israel developed its ethnic identity: 'But what is missing from the various descriptions is an explanation of *how* they became a different ethnic group .... What was the process of ethnogenesis?' (Faust 2006, 181, emphasis original.) As I have pointed out elsewhere (Bimson 2015, 102, n. 6), in the context of the *CoD* chronology 'we are not under any obligation to explain Israel's ethnogenesis against the background of the Iron I period, so this particular objection to interpreting the Iron I settlements in terms of internal processes has no force.'

12. Concluding Remarks

Finkelstein (1995, 351) comments on why the biblical account of early Israel has fallen into disuse in reconstructions of Israel's origins: 'Its relatively late date and its literary-ideological character make it irrelevant as a direct historical source.' He does not deny that 'some historical data may be embedded in it', but feels that 'the extraction of these possible historical nuclei from the biblical text is a difficult and Cisyphean task, if possible at all.'[45]

In my opinion this is an overly negative view. The book of Joshua probably contains earlier material than Finkelstein and others assume, and its literary-ideological character does not rob it of historical worth.

In this paper I have tried to adopt the holistic approach advocated by Frendo, giving biblical data 'due weight' alongside other types of evidence. Of course, it is not a simple task to identify what Finkelstein calls 'historical nuclei' within a text that is not (in any modern sense) historiographical; but I suggest that when archaeological evidence provides a close correspondence to the details of a narrative (as in the case of the fall of MB Jericho) we are entitled to think we have at least come close to succeeding. The conquest traditions are not irrelevant as a historical source; they should be rehabilitated (with the caveats offered under **A Contextual Approach to the Conquest Accounts**) and given an archaeological context at the end of the MBA.

In a sensitively written endnote, Faust (2006, 233–234) defends Albright and others against some of the fierce attacks their work has come under. He says:

> They were, like us, influenced by their background and beliefs. However, a look at their work shows that they did not 'cheat'; there was no knowing and systematic distorting of the evidence. In the debate over the date of the Israelite settlement, for example, it seems that most archaeologists, like Albright, would have preferred a date that conforms with the biblical date for the Exodus and Conquest. However, they accepted a later date due to the accumulating evidence.

My own reading of Albright's early articles persuades me that this is true. It is ironic that Albright was not living at the right time to see evidence accumulate in support of his early dating of a conquest by the Joseph tribes. We can only speculate whether that earlier conquest would have become the dominant one in his thinking, eclipsing his 13th-century conquest theory, instead of the other way round. It is also ironic that, in spite of the collapse of the Albrightian synthesis, the debate over Israel's origins has remained stubbornly focused on the decades around 1200 BC.

[45] We might expect that among the 'historical nuclei' we could confidently include the tradition of the Exodus from Egypt. Indeed, Finkelstein himself has written (1988, 348): 'Along with most other scholars, we accept that there must be a kernel of historical veracity in the deeply-rooted biblical tradition concerning the origin of Israel in Egypt.' However, by the time he co-authored *The Bible Unearthed* (2001), he was suggesting the story of the Exodus might consist of vague memories of the expulsion of the Hyksos, combined with Judah's circumstances in the 7th century BC (Finkelstein and Silberman 2001, 68–71). Faust (2015, 476) lists some 20 scholars who believe the Exodus narrative 'has a historical core' – i.e. that 'there was a group which fled Egypt and brought this story of Exodus with it'. Faust himself agrees there was an Exodus group, but he believes it was distinct from Merenptah's Israel and is unable to say when it might have arrived in Canaan (2006, 187, n. 22; 2015, 477). To Faust's list can be added Rainey (in Rainey and Notley 2006, 118). As far as I am aware Bietak, Hoffmeier and Yurco are the only Egyptologists in his list. On the attitudes of Egyptologists to the Exodus tradition, see Hoffmeier 2015.

Appendix: Are the Philistines a Problem?

It has been argued by Rendsburg (1992, 520–521) and Bruce (2019, 484–485) that biblical references to Philistines at the time of the Exodus and Conquest (Ex. 13:17; Josh. 13:2) are evidence that the Exodus did not occur until the 12th century BC. Alternatively, it is commonly assumed that such references are anachronistic (e.g. de Vaux 1978, 504). Both positions depend on the assumption that Philistines were not present in southern Canaan before the eighth year of Ramesses III. This writer (Bimson 1990) and Peter James (1995, 2000; and 'Towards a Resolution ...' in this volume, pp. 219–248) have long argued that the Philistines were not new arrivals in Canaan during the reign of Ramesses III, but rather received their distinctive identity from being Canaanites who were 'cypriotised' by a series of immigrations and trade connections. As far as numbers were concerned the immigrations were probably small but their impacts were amplified by trade and transfers of technology (Middleton 2015, quoted by James, 'Towards a Resolution ...' in this volume, pp. 219–248). The case can only be summarised briefly here.

Immigrations are discernable from at least the time of the MBA/LBA transition. During MBIIC bilobate chamber tombs and loculi tombs appear in the south of Canaan (at Tell el-Far'ah South, Tell el-'Ajjul, Lachish and Jerusalem). Both types of tombs developed in eastern Cyprus. W. H. Stiebing (1970, 143) concluded that they were probably 'cut by Cypriots living in Palestine' and hence provide 'compelling evidence for the settlement of Eastern Cypriot groups in southern Palestine' during MBIIC (1970, 141).

Around the same time Cypriot Wheel-made Bichrome Ware begins to arrive on the coast of Canaan, becoming so plentiful in LBI that it 'serves as a trademark for that era' (Aharoni 1982, 112). Neutron activation analysis has shown that, like the tombs discussed by Stiebing, much of this imported pottery originated in the eastern part of Cyprus (Artzy, Asaro and Perlman 1973; Artzy 2001, 161, 170).[46] It occurs throughout the eastern Levant coast and in Egypt, but its highest concentration is in the region between Tell el-Far'ah South and Jaffa (Hein and Stidsing 2013, 45–46 with Figs. 1 and 2). Soon after its arrival it was being copied locally, e.g. at Megiddo (Artzy, Perlman and Asaro 1978).

[46] According to a biblical tradition found in Amos 9:7 and Jer. 47:4, the Philistines had their origins in Caphtor. It is therefore of interest that J. Strange (1980, 147–184) has argued, *contra* the majority view (e.g. Kitchen 1965, 5–6), that biblical Caphtor, and Keftiu of Egyptian texts, should be identified not with Crete but with part of Cyprus, specifically with the southern and eastern part of the island. The proposal has been strongly criticised (e.g. Knapp 1983 [my thanks to Peter James for drawing this review to my attention]) and has not gained a following but should perhaps be revisited in light of the above evidence.

Figure 17. Middle Bronze Age/Late Bronze Age I bichrome crater (courtesy of the Dept. of Archaeology of the University of Haifa; photo by Hanay, Wikipedia creative commons).

Bichrome Ware was not the only type of Cypriot pottery to appear in the Levant at the transition from MBIIC to LBI, the two most common being White Slip Ware and Base Ring Ware (for a full listing see Artzy 2019). In comparison with the MBA, trade between the Levant and Cyprus intensified from the beginning of the LBA, when there was also a shift in focus 'from the northern Levant towards the southern Levant, as indicated by the large variety of Late Cypriot pottery wares at several sites in Southern Canaan' (Hein and Stidsing 2013, 48).

As Kenyon commented long ago (1979, 188), during the LBA 'almost as much pottery of Cypriot connections is found as that in the native tradition.' Kenyon emphasized, however, that this was not a break in culture, 'only the addition of new elements', and added (1979, 188): 'This is probably a true indication of the racial position .... The basic Semitic Canaanite population, like its pottery, remains the same. New groups are absorbed, new rulers with alien names may establish themselves in the various towns, but the culture remains Canaanite ....' I suggest this process, underway by the MBA/LBA transition, came over time to account for the 'otherness' of the Philistines as perceived by the Israelites.

I have no wish to deny that references to Philistines in Gen. 26:1, 8 and 14–18 are anachronistic. As argued long ago by Macalister (1913, 39), they could easily derive from a retrospective use of the term 'land of the Philistines' in Gen 21:32 and 34. And I would certainly not claim on the basis of Josh. 13:3 that the Philistine pentapolis (Gaza, Ashdod, Ashkelon, Gath and Ekron) existed within a short time of the Israelites' arrival; this reference to five Philistine rulers and their towns falls within a parenthesis which is best read as an editorial gloss (Bimson 1990, 59). The terms 'land of the Philistines' (Ex. 13:17) and 'sea of the Philistines'

(Ex. 23:31) are likely to be retrospective, i.e. reflecting common usage long after the periods referred to.

Otherwise references to Philistines can readily be understood as referring to Canaanites who became 'cypriotised' by periodic immigrations from, and ongoing trade relations with, the island of Cyprus (in a similar vein, see van der Veen and Zerbst 2022, 195–202).

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Postscript: The above was completed before I became aware of the paper by L. Nigro, 2023. 'Tell es-Sultan/ Jericho in the Late Bronze Age'. In K. Soennecken *et al.* (eds), *Durch die Zeiten, Festschrift für Dieter Vieweger*, 599–614. Gütersloh: Gütersloher Verlagshaus. Nigro assigns sections of mudbrick wall atop the revetment wall to an LB city, which he suggests was the seat of an Egyptian vassal. He also implies that LB Jericho lasted until the end of LBIIB, *c.* 1200 BC. Nigro fails to answer the case made by Bienkowski (1986, 122–125) that the brick sections were part of the MB defences. No evidence is presented to suggest that the LB settlement lasted until the end of LBIIB, or that it was destroyed. Therefore I stand by what I said above.

Phoenicia at the Late Bronze–Iron Age Transition

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One theme on the end of the Late Bronze Age in the Eastern Mediterranean around 1200 BC reappears in scholarly discussions, namely that a wave of destruction hit almost every single site in the region. While it is assumed that the main culprits of this encompassing turmoil were the infamous Sea Peoples known from the Egyptian records of the 19th and 20th dynasties, the material evidence substantiating these claims is widely lacking. Absence of destructions is mostly evident in the region known as 'Phoenicia'. Many scholars however claim that massive destruction from the end of the Late Bronze Age does exist in the region to the south of 'Phoenicia', i.e. the northern part of the southern Levant. But even here material evidence is mostly wanting. A different situation however exists on the northern part of the Levantine coast, at least at some sites, but it still remains a moot point if these sites were destroyed by one particular enemy or by different culprits. In this article we shall examine the evidence and draw some conclusions.

Introduction

Concerning the end of the Late Bronze Age in the Eastern Mediterranean in *c.* 1200 BC, one continuous theme reappears.[1] This theme is that a wave of destruction hit every major site throughout this vast geographic area – from Mycenaean Greece to the Hittite heartland, the trading island of Cyprus and the entire Levant, along with numerous other smaller sites, all of which were caught up in a fiery destruction (Drews 1993; Cline 2014, 2021; Kaniewski, Guiot and Van Campo 2015; Knapp and Manning 2016). The agents of this destruction range from the natural in the form of devastating earthquakes, one after another in a so-called earthquake storm, to the infamous Sea Peoples of Egyptian notoriety. In addition, there were supposedly peasant uprisings, warring polities, land invasions, raiders, pirates, enemies on ships and nameless hordes. The destruction that these various agents of devastation wrought, have been endlessly discussed throughout the scholarly literature. As it seems, the study of the end of the Late Bronze Age (henceforth LBA) has almost exclusively related to massive destructions.

However, in the general scholarly discourse, there is one region which typically has been left out of this scenario, as it supposedly went unscathed by destruction in the years surrounding 1200 BC. This concerns the central Levant, the birthplace of the Phoenicians – as typically the region is described as experiencing no destruction at the end of LBA (Bikai 1992; Bell 2006, 2009; Charaf 2008; Heinz 2016; Núñez 2018).[2] However, if we move slightly further to the south of Tyre, the academic picture of destruction is bleaker. Sites such as Acco, Tell Abu Hawam, Tel Dor, Shiqmona, Tell Keisan, Tel Nami, Achzib and others located further inland, are all assumed to have been destroyed at the end of LBA (Dever 1992; Stern 2012; 2013; Manolova 2020). Stern (2013, 5) succinctly and unequivocally summarised the situation when he stated that,

> All sites on the Canaanite coast of the Hefer Valley, in the Sharon, and on the Carmel coast – without exception – were laid waste at the

[1] I would like to thank the organizers of this conference for inviting me to present this paper, and especially to Peter James for all of our compelling, thought provoking, and at times, dry humor-laden conversations on the Sea Peoples and end of the LBA. I would also like to thank the anonymous reviewers for their helpful comments and suggestions. The research presented in this article is part of a larger project 'Destruction and the End of the Late Bronze Age in the Eastern Mediterranean' funded by the German Research Foundation.

[2] This was not always the case, as in 1973, when Katzenstein stated that 'The trail of destruction wrought by the Sea-Peoples can be traced along the entire eastern Mediterranean coast, from Ugarit (destroyed, never to rise again) in the north, through Arvad, Sidon, Tyre (?), Tell Abu Huwam and Dor, to Ashkelon in the south' (Katzenstein 1973, 59). Barnett (1975, 360), too, claimed that at least Sidon was destroyed at the end of the LBA based on tradition, not on any excavation. However, more recent examinations of the Phoenician coast typically leave out any mention of destruction.

> end of the 13th century BCE in a total destruction that put an end to the Canaanite culture and Egyptian domination. This destruction has been attributed by the excavators of all the settlements in these areas to the Sea Peoples.

What we are being presented with in the scholarly literature therefore is a dichotomy between these two regions in what we later call 'Phoenicia'. Hence in one region there is an utter lack of destruction while in the other there is complete annihilation. One could ask why this is and why the dichotomy exists, as many scholars have done in the past, seeking to explain the diverse trajectories of the two regions. However, this is not the optimal approach to answer this question. Rather, we should ask: does the dichotomy exist at all? Is the central Levant or 'Phoenicia' (henceforth Phoenicia) an outlier without destruction in *c.* 1200 BC while we do find physical evidence of widespread destruction on the northern coast of the southern Levant? To answer these questions, we must first examine whether sites in Phoenicia are really lacking destruction by reviewing the archaeological evidence. Next, we shall examine those sites that were supposedly destroyed, and which are located just south on the northern coast of the southern Levant, to see if they were indeed destroyed. It is only by critically examining the evidence from both regions that we may find out, if a dichotomy existed and if destruction did affect the trajectories of these cities and towns near the beginning of Iron Age I. The nomenclature and classification of the scale and cause of the destruction follows the method presented by Millek (2018b, 2023).[3]

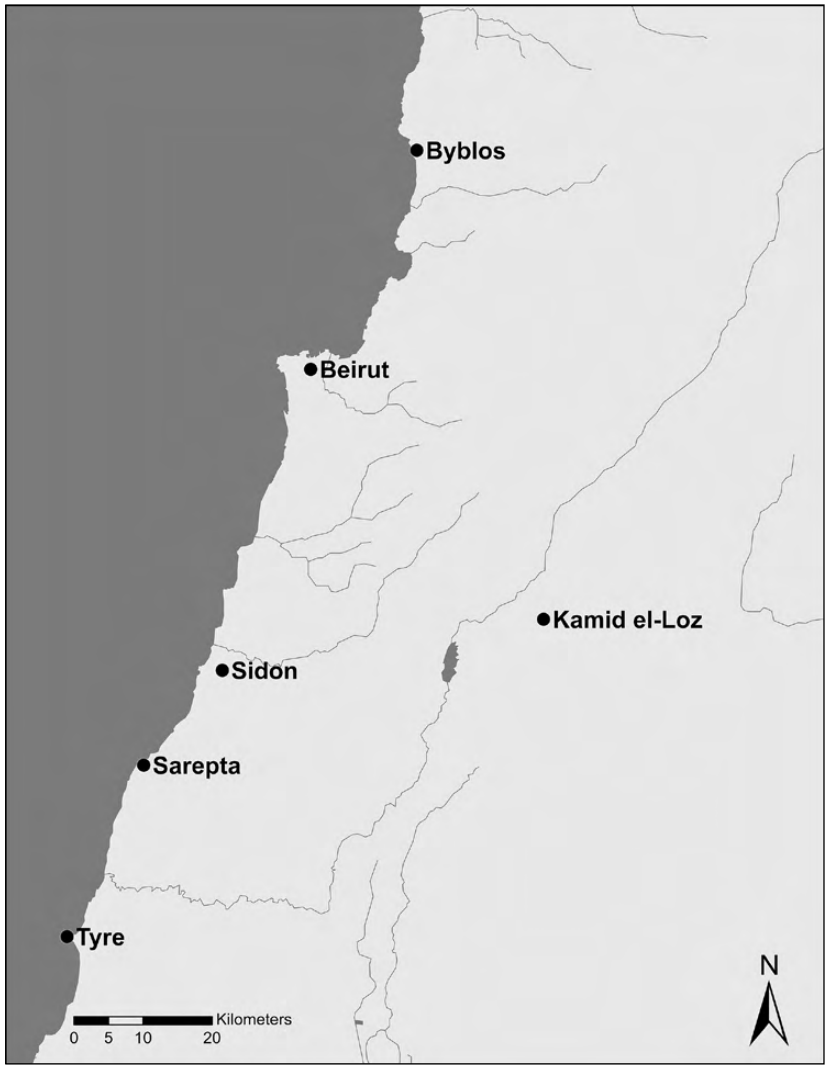

Figure 1. Phoenician sites discussed in this article.

The Central Levant/Phoenicia

It may well be true that the consensus is that there was no destruction in Phoenicia in *c.* 1200 BC. However, if one searches for references to destruction in that region, one finds opposite statements scattered throughout the literature. Byblos (Kaniewski *et al.* 2011; Kaniewski, Guiot and Van Campo 2015), Kamid el-Loz (Metzger 1991, 217; 1993, 121; Adler and Penner 2001, 350; Wagner-Durand 2020, 75), Tyre (Katzenstein 1973, 59; Elayi 2018, 95) and Sidon (Katzenstein 1973, 59; Barnett 1975, 360; Sader 2014, 610) have been cited as evidence for destruction in *c.* 1200 BC. Yet, the physical evidence at each site is lacking, as is the case with all Phoenician sites that have been excavated to date. This is demonstrated by archaeological evidence, beginning with Byblos and the sites located to its south.

On two separate occasions Kaniewski and his team inexplicably – albeit without references – placed Byblos on a map with sites that were apparently

[3] The scale of destruction is: Partial – Single Building – Multi-Building – Site-Wide. Partial destructions have evidence of a destruction though this evidence is often minimal and ambiguous. Single Building destructions are represented only by a single structure from a given phase or level that shows evidence of destruction, while all other buildings lack evidence of destruction. A Multi-Building destruction naturally has several structures with evidence of destruction though not all were destroyed, while a Site-Wide destruction has evidence of destruction in every excavated area based on a large-scale excavation. The causes of destruction are: Unknown; Natural; Accidental; Human Activity.

destroyed by the Sea Peoples in *c.* 1200 BC (Kaniewski *et al.* 2011, 2, Fig. 1; Kaniewski, Guiot and Van Campo 2015, 2, Fig. 1). However, such evidence is clearly non-existent. The LBA material at Byblos is also hampered by artificial levels that Dunand assigned to the site when he excavated there in the early 20th century.[4] That said, after a painstaking recent reinvestigation of the LBA material from Byblos, Kilani demonstrated that there exists no evidence whatsoever that Byblos suffered destruction at the end of LBA (Kilani 2016, 291; 2020, 241). He in fact states that, 'No destruction level that could be associated with the end of the Late Bronze Age was identified in Byblos' (Kilani 2020, 241). Consequently, Kaniewski's claim that the site was destroyed by the Sea Peoples, is truly baseless.

Moving on to Beirut, no habitation levels from the end of LBA have been uncovered there. Even so, Glacis II, which was excavated in Bey 03, 13, 20 and 32, was refurbished sometime between the second half of the 13th and the first half of the 12th century BC (Charaf 2008, 87; Núñez 2018, 120). Suffice it to say, no evidence of destruction of Glacis II has been found yet, and there is no empirical evidence of destruction for the end of LBA, a fact which is even more telling as evidence of habitation at the end of LBA is lacking. At Sidon excavations at the College Site have demonstrated that there was a smooth transition from LBA to the Iron Age without any intervening destruction (Claude Doumet-Serhal, pers. comm. Apr. 10, 2018; Núñez 2018, 120). Likewise, at Sarepta the only traces of burning were uncovered in Area II, Y Stratum G1 in association with industrial activities. Despite the industrial ash, there is only evidence of a smooth transition between LBA and early Iron Age (Anderson 1988, 82, 381, 386). The same is true for Area II, X Stratum V, where again no evidence of destruction was uncovered for the end of LBA (Khalifeh 1988, 27–35, 102–103, 112–113).

As for Tyre, Katzenstein already claimed that the site had been destroyed before any excavations had uncovered evidence of the LBA–Iron Age transition there. As a matter of fact, he simply *assumed* that Tyre, along with other sites on the Levantine coast, had been destroyed (Katzenstein 1973, 59). While the small-scale excavation at Tyre conducted later in the 1970s did yield some remains from the end of LBA (dubbed Stratum XV), again no clear evidence of destruction was found. The small excavated area was covered by a black ashy layer (Bikai 1978, 7–8, 72–76). Yet, no other evidence of destruction, such as fallen or burnt mudbricks and burnt pottery, was unearthed. To the contrary, Bikai offers an alternative explanation for the blackish layer: 'Over much of the excavation area between Strata XV and XIV, a black floor was found. The round fire pit in the centre of the room in area 2 and a *tannur*-oven on the floor in area 1 suggest that the blackness is the result of domestic cooking activity and not of destruction'.[5] If Bikai's interpretation is accepted, Katzenstein's claim, that Tyre was destroyed in *c.* 1200 BC, is built on sand.

The situation at Kamid el-Loz is slightly less explicit, as the evidence of destruction is insufficient. Even so, some indications do suggest that the abandonment of the site at the end of LBA has not been peaceful. Palace P1/2 has no evidence of burning or destruction prior to its abandonment (Adler and Penner 2001, 311–340, 349–350). For Temple T1, evidence of burning is only recorded in three rooms, Rooms F, H and L, all of which are on the western side of the structure. Room F had a black fire layer with burnt debris, Room H's floor was covered with white ash, while Room L was covered with grey ash (Metzger 1993, 96, 105, 112). However, while it does appear that Room F suffered some minor burning event, ashes seem to have been scattered in Rooms H and L (though without clear evidence of a fire). Rooms G, H and K (which are likewise situated in the western half of the structure) show no signs of damage by fire or ash (Metzger 1993, 103–111). Thus, there is no reason to assume that the building was burned in a fire, as evidence of conflagration is minimal at best. On the eastern slope of the tell in Area II-e-5, two structures, dubbed Houses 4 and 5, were uncovered and it was initially assumed that they had been burned at the end of LBA (Heinz *et al.* 2010a, 42; 2010b, 30, 35; Wagner-Durand 2020, 70, 82, Table 3). But Heinz stated later that these houses were not destroyed (Heinz 2016, 181), while Wagner-Durand maintains that they suffered some kind of destruction (Wagner-Durand 2020, 70, 82, Table 3). While the wall foundations were covered over by a layer of greyish-white ash with no further signs of destruction (Heinz *et al.* 2010a, 42; 2010b, 30), the overall evidence is clearly insufficient that Kamid el-Loz suffered major destruction at the end of LBA. Traces of burning were clearly minimal. That being said, the site could have suffered from an attack prior to its abandonment, even if this event did not result in the physical destruction of the site's architecture. In Palace P1/2, an arrowhead was uncovered in Courtyard H (Adler and Penner 2001, 339), while in Temple T1 one arrowhead was recovered from Room M/T1. Two more were found in Room J, while other arrowheads were discovered in Room B and Shrine D. Four arrowheads were recovered from Room K, one of which was found resting on the threshold between the rooms while in Courtyard G three additional arrowheads were found (Metzger 1993, 35, 40, 57, 65, 104, 111, 122). These scattered projectiles may well indicate some military conflict at Kamid el-Loz. Even so, this event does not seem to have resulted in the final destruction of its buildings.

[4] See Kilani (2020, 6–23) for a discussion on the history of excavations at Byblos.

[5] Bikai (1978, 8).

Combining the evidence, the assertion that a wave of destruction in Phoenicia is lacking in *c.* 1200 BC, is indeed borne out by archaeological excavations, even if at Kamid el-Loz the transition to Iron Age I may not have been peaceful. Yet, what was the situation like on the northern coast of the southern Levant? Is there a marked difference between these two regions in terms of destruction?

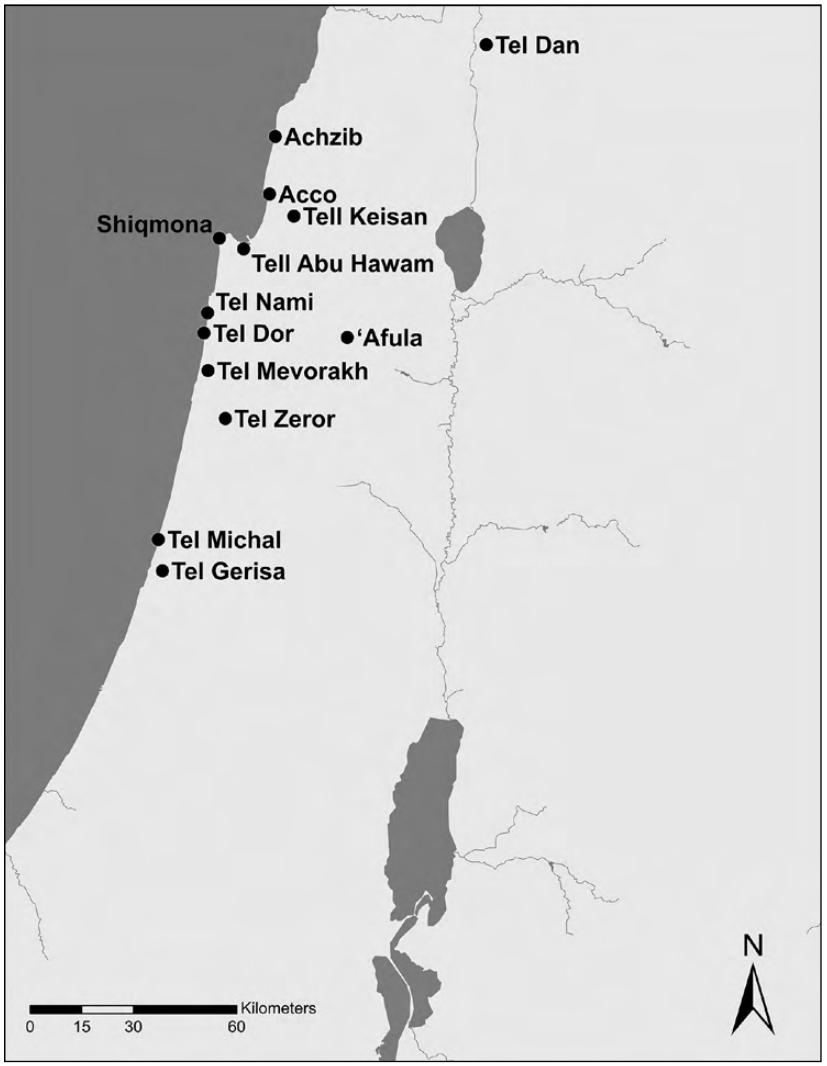

Figure 2. Southern Levantine sites discussed in this article.

The Southern Levant

The supposed destruction wave described by Stern as cited above, spans a number of sites just south of the boundary which scholars have designed between Phoenicia and the southern Levant. This includes sites such as Achzib, Acco, Tell Keisan, Tell Abu Hawam, Shiqmona, Tel Nami, Tel Dor and Tel Mevorakh. Nevertheless, despite the claim that these coastal sites were destroyed in *c.* 1200 BC, only Tel Nami possesses substantial evidence of destruction, and even this evidence may well be due to a natural cause. Beginning again in the north, Stern claimed that Achzib was destroyed by the Sea Peoples, an assertion that was based on a single statement by Prausnitz (1993), who simply wrote that, 'By the end of the Late Bronze Age, the [MBA] defenses were again destroyed' (Prausnitz 1993, 32). However, despite this singular and unsubstantiated statement, Prausnitz nowhere in any of his preliminary reports mentions any such destruction of the defenses. Even worse still, in his excavation he never uncovered any remains of a settlement dated to the end of LBA which could have been destroyed by the Sea Peoples (Prausnitz 1963, 1965, 1993). Moreover, also in the recent excavations at Achzib conducted by Abrahami, no such evidence was found; only evidence of destruction at the end of MB IIB was uncovered, while the final occupation phase at Achzib terminated during LB I, without evidence of any destruction (Abrahami, pers. comm. Apr. 14, 2020; Tharéani, Jasmin and Abrahami 2016, 2017). Based on the lack of habitation in *c.* 1200 BC, we may safely conclude (at least for the time being) that Achzib was not destroyed around 1200.

At Acco, despite claims to the contrary (Dothan, T. 1983, 103–104; Drews 1993, 9, 16, Fig. 1; Nur and Cline 2000, 44, Fig. 1; Nur and Burgess 2008, 225, Fig. 8.1; Cline 2014, 110–111, 114, Fig. 10; 2021, 110; Knapp and Manning 2016, 130) no evidence of destruction has been found. The ash, which some believed to be evidence of the site's demise, in fact turned out to be mere waste derived from a kiln in the industrial area. We clearly do not possess sufficient evidence to prove that the site was destroyed at the end of LBA (Dothan, M. 1981, 111; 1988, 297–299; Dothan, T. and Dothan, M. 1992, 212; Artzy and Beeri 2010, 18; Yasur-Landau 2010, 170). At Tell Keisan, Humbert initially assumed that the burned remains of two rooms from Stratum 13 dated to *c.* 1200 BC and that this burning was caused by the invading Sea Peoples (Humbert 1981, 386–389; 1993, 864). However, Burdajewicz, who is currently preparing the pottery from the final two seasons at Tell Keisan, has personally informed me that the date for the end of Stratum 13 should be lowered to *c.* 1150 BC (Burdajewicz pers. comm. Apr. 16, 2020). Thus, while there exists some evidence of destruction at Tell Keisan, it appears to postdate the end of LBA.

The idea of a LBA destruction of Tell Abu Hawam has persisted in the scholarly literature since the 1950s (Maisler 1951, 22; Anati 1963, 142; 1975, 11; Dever 1992, 100, Fig. 13.1; Stern 2013, 5; Manolova 2020, 1199). However, while various scholars have claimed that the site was destroyed in *c.* 1200 BC (i.e. at the end of Stratum VC), no one so far has been able to demonstrate what the extent of that destruction would have been, nor has anyone provided material evidence to substantiate the claim. That said, the explanation for this is there is no archaeological evidence in any inhabited areas dated to *c.* 1200 BC. This is confirmed by nearly 100 years of excavations at the site (Hamilton 1935; Anati 1963, 1975; Gershuny 1981; Balensi 2004; Artzy 2006, 2013). This is in fact what Balensi, one of the excavators of Tell Abu Hawam, argued. She observed that the only possible evidence is that of the site's fortifications, but that it was not actually caused by devastation. Rather, their 'destruction' was the result of natural degradation, as the defences had not been sufficiently maintained (Balensi 2004, 162). Thus, there is no physical evidence of either a natural, accidental, or wilful destruction at Tell Abu Hawam in *c.* 1200 BC, as the dilapidation of the defences was but a logical consequence of human neglect.

For Shiqmona, Elgavish, the original excavator at the site, claimed that a public building was destroyed at the end of the 13th century BC (Elgavish 1993). Nevertheless, Bar, who conducted recent excavations at Shiqmona and who re-evaluated the material from Elgavish's excavation, has informed me personally that this LB II public building must be redated to the 14th century BC. Moreover, recent excavations have also uncovered remains which do date to the 13th and 12th century BC but again no evidence of destruction has been uncovered (Shay Bar, pers. comm. Apr. 13, 2020). Therefore, based on this new information, we possess no evidence that Shiqmona was destroyed in *c.* 1200 BC.

The only coastal site listed by Stern which has some physical evidence of damage or destruction is Tel Nami, and its partial destruction occurred at the beginning of the 12th century BC. In Area D1, the area was sealed by a thick layer of mud and fallen mudbricks which might have come from a collapsed roof, while in Area G archaeologists discovered a large amount of crushed pottery. It was found that a basalt basin had been knocked off its base, crushing pottery underneath it (Artzy 1990a, 23, 34; 1990b, 75–76; 1991, 197; 1992, 24; 1993, 1096–1097; 1995, 22–23; 2013, 10). Unfortunately, other than this, no further information has been published detailing the destruction or the remains from the end of LBA. Based on the preliminary reports it is unclear how much of each area was affected by destruction. That said, based on published evidence, it seems likely that Tel Nami had experienced a severe storm or other natural disaster that resulted in the collapse of some of the super structure, sealing many of the bronze and gold objects under a layer of mud. Consequently, even though there was some destruction (while some architecture got damaged), it is unlikely that this was part and parcel of some wider trend or destruction horizon. There is no indication from Tel Nami or any of the surrounding sites that an earthquake had been responsible, so that we must assume that this was a localized event without broader implications for the situation in *c.* 1200 BC on the northern coast of the southern Levant.

The final two coastal sites were excavated by Stern himself: Tel Dor and Tel Mevorakh. Starting with Tel Dor, only a small portion of the thirteenth-to-twelveth-century-BC transition was uncovered in Area G Phase 11. In this area, no evidence of destruction was found, a fact which was noted by Gilboa and Sharon, who continued excavations at Dor (Gilboa and Sharon 2003; 2017; Sharon and Gilboa 2013; Gilboa *et al.* 2018, 28–35). The fact that no destruction was found at Dor did not even escape Stern, despite his repeated claims to the contrary (Stern 2000; 2012, 506; 2013, 5). Stern simply reckoned that Dor had to have been destroyed due to the general assumption that all sites in the region would have been destroyed by the Sea Peoples. Yet, as he clearly states, 'The Bronze Age stratum of destruction at [Tel Dor] *has not yet been reached*' (Stern 2013, 5; emphasis added). Accordingly, Tel Dor was probably not destroyed at the end of the LBA, as the destruction was only based on an assumption rather than on empirical evidence. Also at Tel Mevorakh, despite Stern's claim to the contrary (Stern 1978, 76; 1984, 9; 2000, 53, 61; 2012; 2013, 5) no evidence of destruction has been forthcoming. In the report describing the excavation of the Stratum IX Temple, there is no mention of destruction debris or burning (Stern 1984, 8). Rather it seems that the temple had simply disintegrated after the site had been abandoned and left to deteriorate over the next 200 years until habitation was resumed in the Iron Age. As Stern himself states, 'We may assume that the upper part of the wall was made of mud-brick, and it was totally demolished during the 200 years of abandonment, i.e. from the end of the thirteenth century to the late eleventh century B.C.' (Stern 1984, 8). Thus, much as at Tel Dor, it appears that it was only Stern's assumption that the Sea Peoples conquered the area through violent destruction when in fact no such archaeological evidence exists at Tel Mevorakh.

What is clear from the excavated remains is that despite Stern's and others' claims to the contrary, and in contrast to the typical narrative, there is in fact little evidence of destruction in the coastal region just south of Phoenicia. The archaeological evidence does not support the assertion that these sites had suffered devastation in *c.* 1200 BC. Moreover, this evidence even extends beyond these sites on or not so far away from

the coast. At Tel Dan, while Level VIIA1 may contain evidence of destruction and while this evidence had originally been assigned to the early 12th century BC (Ben-Dov 2009, 377), new investigations based on pottery and ^{14}C now suggest that its *terminus ad quem* must be lower by 50 years or more (Ilan 2019, 605–611, 635). Both Dever (1992, 110) and Stern (2013, 5) claimed that 'Afula was destroyed by the Sea Peoples, despite the absence of a settlement from the end of LBA, let alone evidence of destruction for this period (Dothan, M. 1993, 37). The same can be said about Tel Zeror, to which Dever and Stern also attributed a destruction (Dever 1992, 110; Stern 2013, 5). Nevertheless, as Ohata and Kochavi argue,

> At the end of the Late Bronze Age (thirteenth century BCE) the city was abandoned. Only the remains of the brick walls, standing sometimes to a height of 1.5 m. or more, were found in this stratum. Grinding stones and other stone vessels were too heavy to carry, a golden earring, and sherds of pottery are the only testimony left of the culture of the last Canaanite city. (Ohata and Kochavi 1964, 284)

As for Tel Michal, another site which Stern assigned to the ravaging Sea Peoples, the site had already been abandoned for a century prior to the end of the LBA, as Herzog clearly states, 'The settlement remained unchanged in plan and continued to exist until its abandonment in the fourteenth or early thirteenth century BCE' (Herzog 1993, 1037). The same can be said about Tel Gerisa, which Stern claimed was also destroyed (Stern 2013, 5). Yet Herzog describes the situation as follows, 'It was found that the latest LB stratum was abandoned, as there are no traces of destruction by fire' (Herzog 1990, 52).

Traces of ash were uncovered at Tel Qashish, Yokneam and Megiddo Stratum VIIB, but at least for Tel Qashish and Megiddo it is unclear if there is actual evidence of destruction. At Tel Qashish a layer of ash, brick rubble and an abundance of pottery was uncovered in a single square only, and the question must be asked if this was due to a destruction or if this is simply domestic debris and ash from cooking or industry. Even if there was burning, it may still have been a very local incident (Ben-Tor 1993, 1203; Millek 2019c, 182). The evidence is so disturbed by later activities at the site that the situation is far from clear. For Megiddo Stratum VIIB, outside of Area AA, no evidence of a destruction event has been uncovered in any of the expeditions to the site (Martin 2017; Millek 2018b, 279–280). Covered and recorded evidence from the Area AA palace suggests that the 'destruction' that Loud excavated, was no destruction at all. There is a clear lack of empirical data from both the photographs and the descriptions of the palace that the area was ever destroyed in a massive burning event (Millek 2018b, 279–280). Thus, the evidence is equivocal for both sites, and it is far from evident that Tel Qashish and Megiddo Stratum VIIB suffered any kind of destruction in *c.* 1200 BC.[6] At Yokneam burning was uncovered in two buildings, but the cause of destruction of these Multi-Buildings remains unclear. The Iron II builders either removed or covered over the LBA remains other than these two structures which were situated outside of the Iron II fortifications (Ben-Ami 2005, 154, 164; Millek 2019c, 182–183).

Consequently, for all sites in the northern half of the southern Levant, most of the claims concerning site destruction in *c.* 1200 BC are fallacious. Either the destruction occurred well before or after 1200 BC, or evidence seemingly supporting partial or complete destruction is lacking. Moreover, only at two sites do we find clear evidence of destruction, namely at Yokneam and Tel Nami. But also here the evidence is equivocal. At Yokneam the cause of destruction remains unclear, while at Tel Nami the collapse of the super structure may have been natural rather than by human intervention.[7] No perceivable connection even between these two destruction events exists, which is emphasized by their dates. While Yokneam was destroyed sometime during the 13th century BC, Tel Nami fell at the beginning of the 12th.

What Dichotomy?

Of the many features which define the years surrounding 1200 BC, one fundamental aspect is the narrative of widespread destruction throughout the Eastern Mediterranean. Due to this narrative of devastation, which permeates the opinion of modern scholarship concerning the end of LBA, the absence of destruction in Phoenicia is clearly striking. Scholars have asked why this region would have been spared. It has been suggested that as Phoenicia had stronger ties to the Aegean, it was spared by the Sea Peoples whose origins lay in the Aegean. In contrast Ugarit and the southern Levantine polities would have been destroyed wholesale because they did not share the same commercial ties with Greece (Bell 2006, 2009). Others argued that the absence of destruction has 'more to do with lack of knowledge than with lack of destructions' as there exist only few excavated sites with an LBA to Iron Age transition in Phoenicia (Gilboa and Sharon

[6] Under the classification system presented in Millek (2018b), Tel Qashish was a Partial destruction of unknown cause while Megiddo Stratum VIIB was a Single-Building destruction of unknown cause. However, this is to err on the side of caution and it should be noted that there is no definitive evidence of destruction at either site.

[7] Under the classification system presented in Millek (2018b), Yokneam was a Multi-Building destruction of unknown cause while Tel Nami was a Partial destruction of natural cause.

2017, 287). In other words, the lack of destruction is simply related to the fact that such evidence is still awaiting discovery. But surely this is the wrong approach. This consideration is namely hampered by the fact that the underpinning framework has been questionable from the beginning. For the assumption has been that there *was* a dichotomy between Phoenicia and the neighbouring sites just to the south on the northern coastline of the southern Levant, which *were* apparently destroyed. This area would later become part of Phoenicia too. Yet, its assumed destructions are not substantiated by empirical archaeological evidence. Hence the assumed dichotomy between the two regions is a mere figment of interpretation in modern scholarship. As we have argued, many destructions in *c.* 1200 BC remain highly questionable.

As laid out above, there is no such evidence of destruction in Phoenicia in *c.* 1200 BC, though Kamid el-Loz may have been attacked prior to the site's abandonment based on weaponry strewn throughout its buildings. Similarly, of those neighbouring sites just to the south on or near the northern coast of the southern Levant, only Tel Nami possesses evidence of destruction in *c.* 1200 BC. But even here the evidence points to a natural event, likely some kind of storm, which caused the structural failure of the walls. Also, at other sites that according to Stern were destroyed by the Sea Peoples, the evidence of destruction is clearly questionable and is mainly based on false citations and equivocal evidence, such as at Tel Qashish and Megiddo Stratum VIIB. Consequently, there is no dichotomy between these two regions for the simple reason that both essentially suffered no or little destruction *c.* 1200 BC. This can be taken even further as on the entire approximately 600km of Levantine coast, only five sites, Tel Mor, Tel Nami, Tell Kazel, Ras Ibn Hani and Ras Shamra show evidence of destruction (Millek 2018b, 2019a, 2020). Apparently, the destruction of coastal sites was not an endemic issue near the end of LBA. Yet destruction has been claimed in the literature for Tell el-'Ajjul, Ashdod, Ashkelon, Jaffa, Tel Gerisa, Tel Michal, Tel Mevorakh, Tel Dor, Shiqmona, Tell Abu Hawam, Tell Keisan, Acco, Achzib, Tyre, Sidon, Byblos, Arwad, Tell Sukas, Tell Tweini and Ras el-Bassit. But no empirical evidence can be found to support a destruction at any of these sites in *c.* 1200 BC (Millek 2018b, 2019a, 2020, 2023).[8]

With all that said, some caveats remain in our discussion above. The first caveat being that we cannot exclude the possibility that future excavations in Phoenicia will eventually yield some evidence of destruction in *c.* 1200 BC. Surely it would be surprising if a region for some 50 years or so did not witness a fire caused by a burning *tabun*-oven, a misplaced oil lamp, or a wildfire that set a site ablaze. Structures could succumb to a faulty foundation or lack of proper maintenance. Destruction could be caused by winter storms, or even by human hands for whatever reason. What area in the world does not witness decay of architecture, burning or destruction by a neighbouring foe during a period of several years? Naturally the situation would not have been different around 1200 BC. At any rate its cause must be carefully studied to see if the evidence is local or more widespread.

If in the future evidence of destruction in *c.* 1200 BC is excavated, for instance at Tyre (including the burning of a single storage building), one may be tempted to connect it to similar finds elsewhere to the south and north, for instance at Tel Nami and Ras Shamra, whose destructions are believed to have been caused by warfare (Millek 2020). For future interpretations will likely continue to be plagued by the classic narrative of traditional 1200isms.[9] Even so, at Tel Nami, destruction was likely caused by a natural event (though not necessarily by an earthquake) and therefore a connection between that site and the hypothetical burnt storage building at Tyre would be based on faulty presumptions. Also, Tyre and Ras Shamra are separated by nearly 320km. The only armies that had the ability to assert dominance over such a vast terrain were those of the Hittites and the Egyptians during the latter part of the New Kingdom period. Unless one assumes the correctness of the traditional Sea Peoples narrative (or a modification thereof) or that a famine-driven populace or for that matter an uprising peasantry acted as a mythical *deus ex machina*, we shall be left in the dark about the cause of destruction. As I have argued elsewhere, many assumed Sea Peoples destructions are the mere figment of modern scholarship. For nowhere in the Egyptian records do we encounter Sea Peoples causing massive destructions throughout the eastern Mediterranean, for instance at Ugarit, in Phoenicia, or in the southern Levant. Moreover, the Medinet Habu inscriptions nowhere claim that the Sea Peoples destroyed anything at all. The Sea Peoples did not *sksk* or *fḫ* ('destroy') the six northern lands. Rather the cities and regions were *fdq* or 'cut off' (Müller 2001; Millek 2017, 2018a, 2019a, 2020, 2021, forthcoming a). Outside of the singular locale of Amurru, the Medinet Habu texts do not mention one gram of ash or destruction debris. The only individual that caused any far-reaching destruction was apparently pharaoh Ramesses III, who is said to have slaughtered the armies of the Sea Peoples.

[8] In Millek (2017, 2018b, 2019c), I suggested that Ashdod suffered a Partial destruction. However, after further review of the evidence, this was an error on my part as there is in fact no substantial evidence of a destruction, a fact which was already noted by Ben-Shlomo, who stated that, 'Sites like Ashdod display no evidence for destruction in the Early Iron Age levels' (Ben-Shlomo 2011, 202). See further discussion in Millek (2023).

[9] For the misleading concept of '1200ism' see the editorial introduction to these Proceedings – eds.

Consequently, our observation speaks to a wider issue at the end of LBA and to the role which 'destruction' played in it. Hence, if evidence of destruction is found around 1200 BC, scholars automatically assume that it is related to some wider regional or superregional situation. But even if no physical evidence of debris is unearthed, or if the evidence is so minimal that it can hardly be related to a destruction event, scholars like Stern still feel tempted to view this as evidence of a major calamity. This is even the case if the ash is merely derived from an industrial installation. However, if we took the destructions that occurred nearer 1200 BC and put them at c. 1300 BC (still maintaining a chronological wiggle of some 50 to 100 years), fewer scholars would be tempted to correlate the destruction of a Single-Building at Ras Ibn Hani in the northern Levant with that of a Single-Building at Tel Mor (Level VII) in the southern Levant. It is only because of the traditional interpretation that these events are associated with each other.

Hence, mere traces of ash or evidence of burning on a floor or in some part of a room is much too quickly taken as evidence of Site-Wide destruction, not because material evidence supports it, but simply because the layer in which it is found is dated to *c.* 1200 BC. But if the layer were to be dated to *c.* 1300 BC, its significance for the narrative structure would be in doubt, as such evidence is not accounted for at the end of the 14th century BC.

The *c.* 1200 BC destruction narrative has been epitomized by Drews, who stated that,

> Within a period of forty to fifty years at the end of the thirteenth and the beginning of the twelfth century almost every significant city in the eastern Mediterranean world was destroyed, many of them never to be occupied again. (Drews 1993, 4)

While many scholars may not readily agree with Drew's overall interpretation for the end of LBA, his 'Map of the Catastrophy' has nevertheless become the basis for many maps and discussions despite its flaws and inaccuracies (Millek 2018b, 2019a, 2023).[10] Consequently, widespread destruction has become a scholarly dogma for *c.* 1200 BC.[11]

The same holds true for the intentional finagling of many of these events. Much of the debate concerning LBA destruction could labeled as the 'Atlantis Premise', as Puglisi calls it, which according to him is 'an unconscious premise according to which destructions related to a radical historical change or, more specifically, to the disappearance of a highly developed "civilization", like mythical Atlantis, have to be put in a very short, archaeologically undetectable, time span' (Puglisi 2013, 177). It is because of this largely accepted premise concerning the end of LBA that one can find on one single map the *c.* 1250 BC Multi-Building destruction at Hazor, the *c.* 1185 BC Site-Wide destruction at Ugarit, as well as the *c.* 1150 BC Site-Wide destruction at Tell Deir Alla (Millek 2018b, 2019a). But these chronologically unrelated events are separated in time by many generations and therefore no relationship ever existed between the culprit of Hazor's destruction in *c.* 1250 and the earthquake that destroyed Tell Deir Alla in *c.* 1150 BC, simply because Hazor's inhabitants (or even their direct descendants) would have long been dead.

All these issues encapsulated into the 1200 BC narrative have led to an assumed dichotomy between the utter lack of destruction in Phoenicia and the destruction at sites just to the south in the southern Levant. While some have claimed that sites in Phoenicia did suffer havoc in *c.* 1200, most Levantine archaeologists have rejected the traditional destruction and Sea Peoples narrative for this region, as evidence is clearly lacking. However, some scholars are still happy to accept the notion for the southern Levant even if evidence to support it is likewise lacking. This situation has led to the assumed contrast between the two regions.

The general lack of destruction in the northern half of the southern Levant and in Phoenicia can however be contrasted with the situation in the northern Levant, where destruction was apparently more widespread in *c.* 1200 BC. It would go beyond the scope of this article to discuss the situation in the northern Levant more fully. But as I have done this elsewhere (Millek 2019a, 2020, 2023) a brief overview will suffice to demonstrate that the northern Levant was the outlier in *c.* 1200 BC rather than Phoenicia. Much like their regional counterparts to the south, destruction has been cited for several sites. Even so, the evidence is wanting. For Ras el-Bassit, Tell Tweini and Tell Sukas have only produced limited traces of ash or burning in small sections of the exposed LBA to Iron Age transition while there is no empirical reason to assume that these sites were actually destroyed (Millek 2019a, 2020, 2023). Even so, the destruction events at Ras Ibn Hani, Ras Shamra and Tell Kazel were perpetrated by arson, warfare or other. At Ras Ibn Hani, the *Palais Nord* was burned in a Single-Building destruction after it had largely been emptied of its contents while all other buildings of the end of LBA lacked evidence of destruction, suggesting that the destruction of the palace had been intentional. Ras Shamra was largely burnt in a Site-Wide destruction, while weapons of war were found scattered throughout

[10] More than 50% of the 'destroyed' sites listed on the map and discussed in his chapter on the end of LBA were not destroyed or suffered a destruction event either well before or well after *c.* 1200 BC. See the discussion in Millek (2023).

[11] Although of course there are other regional 'destruction horizons' which supposedly are also fraught with false destructions, much like the end of the LBA 'destruction horizon'.

the streets and in the open space of domestic *Ville Sud*. A similar situation is accounted for at Tell Kazel's Multi-Building, though the damage must have been less severe. Only the temple and the two adjacent structures were burned while other buildings were left untouched (though weapons were found scattered throughout the site), suggesting that this site too fell victim to a conflict induced destruction (Millek 2020, 2023). Hence what makes these destruction events stand out is that we find clear evidence of anthropogenic destruction for some sites on the northern Levantine coast, while such evidence is lacking for the remainder of the Levantine coast south of Tell Kazel.

The traditional explanation for these destructions in the northern Levant is of course that of the Sea Peoples. But, as I have discussed elsewhere, no documents from Ugarit equate the three Sea Peoples' tribes referred to by Ugaritic scribes with the 'enemies on ships' harassing the region. Likewise textual evidence is lacking that the Sea Peoples in the Egyptian records were militarily active on the northern Levantine coast (Millek 2020, 2021, 2023). Tell Kazel could theoretically have been targeted by Sea Peoples. But if this had been the case, it would need to be argued that Sea Peoples attacked the site after another group of Sea Peoples and their material culture had arrived at Tell Kazel earlier, while subsequently they and the local citizens had been obliterated (Millek 2020, 114–116). It is not evident that destruction was perpetrated by the same group at Ras Shamra as at Tell Kazel, as both sites are some 120km apart as the crow flies. Again, outside of the metanarrative for the end of LBA, there is nothing archaeological or historical here that would suggest that the events were connected. The same could be said of the apparent attack on Kamid el-Loz. A regional scuffle, not uncommon during LBA, is just as likely (if not more likely), as clear evidence is lacking that these events were related.[12]

Thus all in all, the northern coastal Levant is the standout among the Levantine coastal regions rather than Phoenicia. The northern Levantine coast is the only place where we do detect evidence of anthropogenic destruction or for that matter of destruction at all. But once again, these destructions were not necessarily perpetrated by the Sea Peoples, as these destructions could well be unrelated, unless one argues that Ugarit is referred to in the Medinet Habu texts, which it is not. At Ugarit, while the Lukka, Shekelesh and Shardana were known to its scribes, nowhere do we read who the attacking enemies were.

[12] Though much earlier in time, the Amarna letters mention the squabbles, attacks and anthropogenic destruction between the central Levantine polities and other groups such as the Habiru. See EA 53, 62 147, 174–175, 185–186, 189, 363, 371 and the discussion in Kreimerman (2017, 195).

Conclusions

What then can we say concerning the supposed dichotomy between Phoenicia and their neighbouring sites to the south? As we have concluded, this dichotomy only exists in theory, i.e. in the *interpretation* of the material rather than in the material itself. Most of the allegedly destroyed sites to which Stern and others refer show no evidence of destruction, while the evidence which we do find, is sometimes separated in time by some 50 years or so. As we have said, it is only natural that some form of destruction would have occurred at any given time and over a period of several years. On the other hand, it is the destruction events uncovered at Ras Sharma, Ras Ibn Hani and Tell Kazel that standout as indicative of turmoil. If any contrast be drawn, it should be between the northern Levantine coast and the remainder of the Levant rather than between Phoenicia and its neighbouring regions to the south.

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Towards a Resolution of the 'Sea Peoples' Rubik's Cube: or 'Hiram, King of the Sea Peoples?'*

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As a thought experiment this study offers a look at the so-called 'Sea Peoples' problem through the focus of a different chronology. This can be done once we eschew '1200ism', which has long been a stumbling-block to research by its insistence that there was a total collapse of civilization in the Aegean and Eastern Mediterranean and a break in cultural continuity c. 1200 BC. (See James, van der Veen and Wallenfels, 'BICANE 4 – General Introduction', pp. 1–7). Arguments are offered for a completely new context for the 'Sea Peoples' invasion under Ramesses III in the late 10th century BC, in step with the Centuries of Darkness *chronology.*

Introduction

Some twenty years ago Eric Cline and David O'Connor (2003, 134) wrote that:

> ... we find ourselves still asking the most basic of questions about the Sea Peoples. They remain a mystery, because the sources for their existence and impact are confined to a large extent to the ancient Egyptian expression of the world in writing and art. Until more archaeological evidence emerges to correct that imbalance, the Sea Peoples seem destined to epitomize a classic disjunction between archaeological and historical accounts of the past. Few other peoples present such a stimulating challenge.

Little has changed since they wrote. The experiment outlined here is to see whether the 'disjunction between archaeological and historical accounts' is real or apparent, and whether it can be resolved by using a radically different chronological focus – by employing the model set out in *Centuries of Darkness* (James *et al.* 1991, hereafter *CoD*). There (and in many subsequent publications), we argued for a lowering of New Kingdom Egyptian chronology by *c.* 245 years. The archaeological evidence for the end of the Late Bronze Age and the time of the so-called 'Sea Peoples' invasion, *c.* 1200–1175 BC, would thus be placed in an entirely different historical setting – that of the late 10th century BC. (Conventional dates are sometimes used below for convenience of reference).

The Tenth Century BC: An Age of 'Mini-Empires'

The changes that took place across the Levant at the end of the LBA need to be seen against the background of the rise of nation states, conglomerates of the individual city states that were previously the order of the day. It is now fashionable among minimalists (such as Israel Finkelstein) to see the rise of such regional states as only having started in the ninth century BC, with the Omrides in Israel.[1] However, other evidence suggests a different picture. From biblical and extra-biblical sources (including Assyrian records and the 'Tyrian Annals' tradition) we know of the unification of Israel under king David, the development of a powerful Phoenician monarchy under Hiram of Tyre and the origins of the Philistine pentapolis during the time of Saul and David; while the north saw the rise of the Aramaean kingdom of Zobah/Beth Zobah and the Neo-Hittite kingdoms of Carchemish and Malatya and Tarhuntassa). All these developments were underway in the tenth century BC, a century christened by Egyptologist Kenneth Kitchen (2003, 98–101) as 'the age of mini-empires' in the Near East. They would have been interrelated: for example, there was a strong economic interdependence between Phoenicia and Israel. On the *CoD* model the rise of these kingdoms would dovetail

* My thanks to John Bimson, Juan Tebes, Pieter van der Veen, Ronald Wallenfels, Nikos Kokkinos and Jesse Millek for reading and commenting on drafts. The arguments here are, of course, my sole responsibility. Referencing and bibliography will be necessarily minimal as the literature on the 'Sea Peoples' is now uncontrollably vast, while many recent papers simply repeat and amplify previously held views and offer nothing new; and pay little attention to the primary sources. I would like to dedicate this paper to the memory of Alessandra Nibbi (1923–2007) whose challenging work on the 'Sea Peoples' and Egyptian geography often asked the right questions, though rarely, alas, came up with appropriate answers.

[1] For a scathing critique of Finkelstein's 'evolutionary' view of state formation see Kletter (2004).

chronologically with the decline of the Egypto-Hittite stranglehold over the Levant. The fact that Ramesses II and Hattusili III signed their famous treaty shows that the two major powers were losing their grip over the region. The rise of nation states within the Levant would have encouraged Hatti and Egypt to cease an increasingly pointless power struggle, which was draining their resources, and make an alliance in order to maintain their control over the smaller polities of the Levant (see James *et al.* 1992, 128; James 2021, 70–71).

In agreement with Drews (1993, 1998), the 'Sea Peoples' phenomenon should not be seen in terms of large-scale migrations. Rather much can be interpreted in terms of local self-determination. The *Plst* who feature largely in Ramesses III's 'Sea Peoples' records were certainly the Philistines, as usually agreed. Note that a generation earlier, the city of Ashkelon (later a part of the Philistine pentapolis) had already rebelled against Egypt – in the time of Merenptah. The clash with the 'Sea Peoples', as described by Ramesses III, would seem to have involved a further rebellion of the Philistines against Egyptian authority. It is assumed throughout here that the Philistines had already 'arrived' in southern Canaan long before the reign of Ramesses III (as argued in Bimson 1991; James 1995a, 2000). Their 'foreignness' as perceived by the Hebrews would be due to the 'cypriotisation' of the native Canaanites of the southeastern Levantine coast. As argued by Bimson (1991; and now Appendix to 'An Alternative Paradigm' in these Proceedings, pp. 167–204) the process began at the transition of the Middle to Late Bronze Ages; and the development of a Philistine identity would have been reinforced by occasional arrivals of small numbers of fresh settlers, from potters to military adventurers from Cyprus, including at the LBA/IA transition (James and Bimson in prep.)

Destructions at the End of the LBA

At the end of the LBA there were destructions north of Phoenicia, notably at Ugarit, Tarsus and central Anatolia, not to mention those further afield in the Aegean. To attribute all these to the 'Sea Peoples', as is often done, is completely unrealistic (James *et al.* 1991, 120; Millek 2021; see further Millek, 'Phoenicia at the Late Bronze-Iron Age Transition' in these Proceedings, pp. 205–217). For example, the major Hittite centres like Boğazköy (Hattuša), show no trace whatever of the alleged 'Sea Peoples' pottery. And it would seem that Boğazköy was not destroyed as such but was rather gradually abandoned (Bryce 2005, 345; de Martino 2018). One can only agree with the late classicist Alan Hooker (1977, 160) who long ago made this impassioned plea:

> ... no other ruined site of the Near East shows any more trace of the Sea Peoples than Boghazköy does... I wish that we could rid our minds altogether of the seductive notion of a migratory movement, vast in its scope and destructive in its effects, which swept across Anatolia and the Levant.

Different explanations can be sought in terms of climate deterioration (drought), famine and earthquakes around the end of the Late Bronze Age (see Wallenfels 2019, 496). On the 'devastating famine' which struck the Hittite lands and Ugarit at the end of the LBA see e.g. Singer (2000, 24; cf. Kopanias 2017, 124–126). For the role of a series of earthquakes, or an 'earthquake storm' lasting several decades, in the destruction of LBA centres in the Aegean and Eastern Mediterranean see the arguments of Amos Nur and Daniel Cline (2000). I have discussed the idea of a widespread environmental catastrophe at the end of the LBA elsewhere (James 1995b, 149–169).

As for the claimed destructions south of Phoenicia, at Philistine sites such as Ashkelon and Ashdod, it appears that the scale of these has been exaggerated (see Millek, 'Phoenicia at the Late Bronze-Iron Age Transition' in this volume, pp. 205–217). Again, to attribute them to a 'Sea Peoples' invasion – as has often been casually done – is mere assumption. Rather, some destructions in the south may have been due to the Egyptians, who were capable of meeting out harsh punishment on rebel towns, including the seizing of populations, destruction of crops and the burning of the settlements. Merenptah claims to have subdued both Ashkelon and Gezer. There is now dramatic evidence from the recent excavations at Gezer. These have clarified the stratigraphy and discovered in one area (Str. 13) a destruction layer covered by a thick layer of ashes. One building (including crushed human skeletons) was burnt to the ground 'in a fierce conflagration', identified by the excavators as the handiwork of Merenptah's army (Ortiz and Wolff 2019, 67, 79). On the *CoD* model (James *et al.* 1991, 386) Merenptah was the biblical 'Pharaoh', the father-in-law of Solomon who burnt down Gezer before handing it as a dowry to Solomon (1 Kings 9:16).

Ramesses III says he attacked the towns of the Peleset (Philistines) who had defied Egypt, which may account for the evidence of minor destruction at some coastal sites such as Ashkelon. Elsewhere (see James 2017, 128–129, 132) I have argued that it was Ramesses III who destroyed Lachish VI, as once suggested by Olga Tufnell (in Tufnell *et al.* 1940, 23–24; Tufnell 1953, 51) and Amihai Mazar (1997, 158) – see Bimson (2015, 111) and James and Bimson (in prep.) On the *CoD* model Ramesses III (nickname *ssysw, ss<y>sw, ssw* or *ss* – see van der Veen 2015a) was the 'Shishaq' who campaigned against Rehoboam of Judah in 925 BC and seized his 'fenced cities' including Lachish.

The Year 8 Inscription of Ramesses III

The 'Sea Peoples' construct depends largely on a famous passage in Ramesses III's Year 8 inscription (*MH I*, Pl. 46) as translated by Edgerton and Wilson (1936, 53):

> [16]As for the foreign countries, they made a ⌜*conspiracy*⌝ in their isles. ⌜Removed⌝ and scattered in the fray were the lands at one time. No land could stand before their arms, from Hatti, Kode, Carchemish, *Yereth* and [17]*Yeres* on, (but they were) cut off at [⌜at one time⌝]. A camp [⌜was set up⌝] in one place in Amor. They desolated its people, and its land was like that which has never come into being. They were coming, while the flame was prepared before them, toward Egypt. Their confederation was[18] the Philistines, Theker, Shekelesh, Denye(n) and Weshesh, lands united. They laid their hands upon the lands to the (very) circuit of the earth, their hearts confident and trusting: 'Our plans will succeed!'

The text is quite damaged, as can be seen from the restoration marks of Edgerton and Wilson. It also bristles with problems of both translation and interpretation. Egyptologist Donald Redford (2018, 125–126) analysed the word *šdtt* read as 'conspiracy' by Edgerton and Wilson and translated by Gardiner (1961, 284) as 'plot'. Using the determinative and another occurrence in Ramesses III's records (during a Libyan incursion), Redford argued that *šdtt* conveys speech of some kind, a 'communication' or 'proclamation' delivered verbally:

> The implicit assumption is that these barbarians have illicitly arrogated unto themselves the power to issue binding legal decisions. 'To issue a manifesto' suggests itself.

And while the usual understanding is that the 'foreign countries' made a conspiracy *in* their islands, the preposition *m* (Gardiner G17) can just as legitimately be read as 'with' (Gardiner 1957, §162),[2] a reading which would be consistent with a conspiracy or proclamation in that 'their isles' would be a second party to whatever plans were laid – such as offshore territory owned by the aggressors. And, strictly speaking, the word commonly read as 'isles' is an emendation. The word that was carved is actually *rww*, 'mouths', but is assumed to be a sculptor's error for *iww*, 'islands' (Edgerton and Wilson 1936, 53, n. 16a). 'Mouths' taken literally would suit Redford's understanding of the word *šdtt* as a proclamation, and as *m* can read 'in', we could read 'the foreign countries made a proclamation with their mouths (i.e. verbally)', in order to express their intent on warfare. A problem with this reading, however, is that the determinative for *rww* here is a tongue of land (Gardiner N21) which, on balance, would support the emendation to *iww*, 'islands' (pers. comm. David Lorton). However, the reading 'the foreign countries made a conspiracy *with* their islands' remains plausible. There is no reason to look as far afield as the Aegean when there are plenty of islands off the coast of the Levant, not the least being Cyprus (see further below, **The Foreign Countries and their Isles**).

Nor is there any reason to see in the text an 'itinerary' of the Sea Peoples as so often imagined (see e.g. Redford 1978, 75). As already argued (James 1995a, see further James in prep.), the list of lands, 'Hatti, Qode, Carchemish, Arzawa, Alashiya' that 'could not stand against their [the aggressors'] arms', is not an itinerary but a description of the contemporary Hittite Empire. Hatti, of course, is the central polity based at Boğazköy (Hattuša) in central Anatolia. Arzawa was a major kingdom in western Anatolia and Alashiya was Cyprus.[3] They had both been restored to Hittite control by Tudhaliya IV, though they maintained a measure of independence under their own rulers. His son Suppiluliuma II, the last known emperor, either temporarily succeeded in restoring control over Cyprus (Alashiya) or at the very least sent his forces there in an attempt to re-establish the Hittite claims (see **The Cypriot Connection** below). Qode has been plausibly argued though not proven to be Tarhuntassa, a quasi-independent Hittite kingdom in southern Anatolia (Pamphylia).[4] In any case, Qode was reckoned by Ramessses II to be an important constituent of the Hittite Empire, listing Qode amongst his enemies at the battle of Qadesh (Gardiner 1960, §P3–5). Carchemish, in the Euphrates valley, was the seat of a cadet branch of the Hittite royal family, whose rulers were responsible for administering the vassal city-states of Syria (see James and Mahieu, 'The Hittite New Kingdom and its Relationship with the Neo-Hittite Kingdoms...' in these Proceedings, pp. 141–157). Notably, the list does not include Amurru, once a mainstay of Hittite power in the Levant (subject to Carchemish), and a state largely centred on the cities of the northern Phoenician coast. As noted below, the Hittite vassal kingdom of Amurru may have faded a generation or two before the 'end' of the Hittite Empire, which in itself remains undateable.

[2] Many Egyptologists I have consulted over the years (including Michael Jones, David Lorton, Robert Morkot and Mark Collier) agree that this reading is possible.

[3] The location of Alashiya (Egyptian *'Irs*) has been the subject of a longstanding debate, the major choice being between Cyprus and the mainland opposite, on the Cilician or Syrian coast (for an informative, pithy debate see Merrillees 2005a, 2005b; Muhly, Wachsmann and Merrillees 2006). Cuneiform texts often speak of Alashiya as a source of copper, found in abundance on the island. At present it would seem best to rely on historical arguments for its location, as the scientific analysis of Alashiyan tablets has not been decisive (Gilbert 2017), despite claims to the contrary – I hope to discuss the issues involved elsewhere.

[4] See Simon (2011) for some (rather hypercritical) arguments to the contrary.

Or perhaps Ramesses III did not mention it as it was included in the area under the authority of Carchemish. Overall the list presents an accurate picture of the extent of the Hittite Empire near its demise.

Ramesses III did not say that the 'Sea Peoples' overran the Hittite Empire destroying its cities. The most cautious reading is that he claimed that these peoples made a confederacy which was strong enough to best the might of the Hittite Empire: yet he alone could hold them at bay on his very doorstep. All pharaonic inscriptions are hyperbolic to some degree – in this case we can see the endless self-aggrandisement of Ramesses III as the hero who defeated a fearsome enemy. But rather than dismiss it all as hyperbole, we should try to read the events described in the key passage in the Year 8 inscription as plaintext.[5]

Long ago Cecil Torr (1902, 185) made the acute observation that a coalition of forces which could fight both the Hittites and the Egyptians was most likely based in the Levant:

> It was assumed that the people mentioned in line 18 [of Ramesses III's Yr. 8 inscription] must have passed down south through the region mentioned in lines 16, 17, in order to get to *Zahi* [Canaan] and encounter the Egyptians, as mentioned in line 19. But the inscription does not say so. And in view of the fact that three of the tribes mentioned in line 18 can be assigned to the country between Egypt and the region mentioned in lines 16, 17, the true interpretation must be that the aggressors first moved northward to attack this region and then moved southward to attack Egypt.

Nibbi (1975, 70) made the same observation, favourably reviewed by Egyptologist David Lorton (1977, 316):

> Her... conclusion that the texts seem to refer to a confederation of peoples already residing in the area between the power centers of Egypt and Hatti must therefore be taken seriously: not necessarily along the lines that Nibbi proposes, though it must certainly be agreed that the textual mention of lands, frontiers, settlements and towns, which she stresses (p. 70), speaks against any supposition that the 'Sea Peoples' were not resident in western Asia in the time of Ramesses III.

Torr's perceptive observation is followed here. It is supported by the clues from the general nomenclature used for the 'Sea Peoples' by Ramesses III. As has long been stressed by Nibbi (1972, 1975) and others (Bikai 1992; Drews 2000), Ramesses III's texts describe his 'Sea Peoples' enemies in terms traditionally applied to the 'Asiatics' of the Levant (*ʿAmu*, *Sṯtyw*) and Fenkhu (=Phoenicians[6]).

The 'Sea Peoples' Confederacy

Amongst many other scholars, Redford (2018, 126–128) characterised the 'Sea Peoples' invasion as a plot hatched in the Aegean islands – to move a vast horde (men, women and children) by land and sea to attack Egypt. While he did not make the motives for such a migration clear, he had it plodding through Anatolia and northern Syria, destroying Hittite centres, and taking a year at least to reach its goal, the Egyptian frontier. But as Cifola (1988, 303) wrote: 'A conspiracy of mostly unknown peoples who move from the Aegean Sea ("isles in the midst of the sea") and decide to invade Egypt through the destruction of the Hittite Empire is absolutely unimaginable.' Her remark is valid, though strictly speaking Ramesses III's inscriptions do not refer to an invasion of peoples from the 'isles in the midst of the sea', a concept which has always invoked the Aegean in some scholars' imaginations; see further **Afterword**). And like many other scholars, she sees the 'Sea Peoples' as loosely organised barbarians, arguing (Cifola 1988, 303) that Ramesses III gave 'a description of the enemy so vague that it implies that the enemy force was not a coherent body.'[7] Unfortunately Bryce (2005, 339) took a similar line:

> The view that they were participants in a carefully planned military operation is not sustainable. Rather, they were a largely disorganized array of groups, who banded together from time to time in their wanderings and sometimes joined forces for raids and, on occasions, more extensive military operations. They may have had much of the character of the roving, marauding bands that are frequently depicted by science fiction writers in a post nuclear war environment.

These are mere conjectures. Pharaonic war records frequently omit to mention that the forces they were facing had any formal structure or leadership. If they do, references to foreign chiefs are usually anonymous

[5] Many attempts to dismiss or alter its meaning have been made, perhaps the most presumptuous being that by Ben Dor-Evian (2018, 223) who attempts to correct the meaning of Ramesses III's scribes and, after much verbiage, offers completely unjustified and somewhat incomprehensible judgments such as: 'The sheer counter-ideological sense of the eighth-year passage, along with its clear misrepresentation of current affairs, render the common interpretation unreasonable.'

[6] The Fenkhu are depicted in Egyptian reliefs as Asiatics chopping down trees, and Nibbi (1986) made the brilliant observation that their name in Egyptian means 'wood-cutters', a fitting term for the inhabitants of Byblos and environs who were (*pace* Nibbi) supplying the Egyptians with cedar and pine from at least Middle (or Old) Kingdom times.

[7] In dismissing Cifola's analyses as invalid, Kitchen (2012, 16) mischievously described them as 'dense'.

		Peleset	*Tjekker*	*Denyen*	*Shekelesh*	*Weshesh*	*Teresh*	*Sherden*	*Ekwesh*	*Luka*
'feathered' helmet			•	•						
lands, towns near Egypt		•								
later settled in Canaan		•	•							
Ramesses III, Year 5		•	•							
Ramesses III, Year 8		•	•	•	•	•				
enemy in land battle reliefs		*Peleset*-type								
enemy in sea battle reliefs		*Peleset*-type						*Sherden*-type		
Ramesses III, other		•	•	•			•	•		
horned helmet								•		
Turban							•			
Merenptah					•		•	•	•	•
pre-Meren. raiders or mercenaries								•		•
'-esh' name					•	•	•			
'of (the countries of) the sea'						•	•	•	•	

Figure 1. Table of the 'Sea Peoples' groups from the time of Merenptah and Ramesses III. (From James 1995a; 2017, 81.)

(see James 2015a, 237). To take an example, Shoshenq I used vague, formalised statements such as 'Smiting the chiefs of the Nubian tribesmen, of all inaccessible foreign lands, of all the lands of the Phoenicians [Fenkhu], and foreign lands of the Asiatic back-country.' (trans. Ritner 2009, 201). And this at a time when there were certainly organised states in the Levant – on the conventional model Shoshenq's main enemy is thought to have been the kingdom of Judah under Solomon's successor Rehoboam. On the *CoD* model the main goal of Shoshenq's campaign would have been to help the Israelites liberate their lands from the domination of the Aramaean king Hazael of Damascus *c.* (820/819–803 BC), during the reign of Jehoahaz of Israel (James and van der Veen 2015).

Further, in the case of the Year 8 inscription of Ramesses III, the clash between the aggressors and the Hittite Empire is immediately preceded by the statement which is often translated (following Edgerton and Wilson) as a 'conspiracy' and generally followed throughout here for convenience. As noted above, Redford translated the word as 'manifesto', a proclamation for an attack.

At the outset, it should be clear from the analysis given in *Figure 1* that the alliances fought by Merenptah and Ramesses III were different and should not be simply lumped together under the term 'Sea Peoples' – though I will continue to use it below for ease of reference. Second, the blanket term 'Sea Peoples' or 'Peoples of the Sea' for the invaders in year 8 of Ramesses III is somewhat of a misnomer. It ('peuples de la mer') was coined by French archaeologist Maspero in the 1870s (see Drews 1993, 53–61). Strictly speaking there is no such term in Egyptian texts. The nearest one can find in Egyptian texts are expressions such as 'of the lands of the sea' applied to the Ekwesh (*ARE* III §601), who were allies of the Libyans in the earlier time of Merenptah and were not in the coalition fought by Ramesses III, or the simple 'of the sea' possibly applied by him to the Teresh on one inscription (see below under **The 'Vile Chief of Amor'**). Such terms are never applied to the main group of adversaries in the time of Ramesses III (***Group A*** below).

There are three groups of invaders (see *Figure 1*).[8] With respect to those mentioned by Ramesses III these are: ***A***. Peleset, Denyen and Tjekker, the groups most often mentioned, and shown with identical costume and equipment, including the conspicuous 'feathered' helmets. Although the Tjekker of Dor are known to have had a navy (story of Wenamun), none of these three are ever described as being 'of the sea' and I have suggested (James 1995a), somewhat ironically, that this group should be referred to as the 'Land Peoples'. ***B***. Shardana, depicted with similar equipment but different (horned) helmets. ***C***. The *-eš* group, Teresh, Weshesh and Shekelesh (appearance unknown except for the turbaned head of a Teresh chief shown on one relief, see *Figure 3* below).

Group A: The 'Land Peoples'

Peleset. Though Drews (1998, 2000) would opt for a broader meaning of 'Palestinians', the universally agreed identification with the Philistines (Heb. *plštym*) is followed here.

Denyen *(d3-în-îw-n3)***.** The Denyen are depicted as wearing identical costume to the Philistines, so would likely have been a neighbouring people.[9] They are very likely to be identified with the nearby Hebrew tribe of Danites, whose traditional tribal allotment is described as including Joppa and, in the hinterland, the cities of Eltekeh, Aijalon, Gibbethon, Timnah and nominally Ekron (Joshua 19:40–46). Cyrus Gordon, Yigael Yadin, Wolfgang Zwickel and others have argued that the Danites were an Aegean 'Sea People' (the Greek Danaoi of the Argolid) who joined the Israelite amphictyony (see Zwickel 2017). This however is not the direction which the biblical traditions suggest – rather they describe a Hebrew tribe who were becoming Philistinised; see for example the story of Samson's marriage to a Philistine woman (Judges 14:1–19). At some point, much of the tribe migrated north to Laish/Dan, remotely from the other tribes, where they were to show signs of becoming Phoenicianised (see below, under **Allies, Mercenaries and Refugees from Troy)**.

Tjekker *(T̲3-k3-rw/ T̲kr).* Given the above two identifications, the third related group, Tjekker, depicted identically to the *Plst* and *Denyen* would also have been a neighbouring people. The best clue to the identity of the Tjekker comes from the name of Tjekker-Ba'al (*T̲3-k3-rw-B-ᶜ-r'*), the king of Byblos in the late 20th-dynasty Egyptian story of Wenamun. The name is also recognisable in that of Sicharbas (variants Acerbas, Sychaeus), the priest of Melkart and uncle and husband of Dido (Elissa) from the *Tyrian Annals* tradition. The Tjekker are often identified with Greek Teukroi (a tribe from Salamis) or Sikeloi (Sicilians). Yet a reading of the Byblian king's name as 'Teukrian of Ba'al' or 'Sicilian of Ba'al' would be absurd. In Phoenician *zakar* means 'male, man' and the reading of the name Tjekker-Baal as 'man of Ba'al' makes better sense, in line with other theophoric names. Goedicke (1975, 175–182) argued that Zakar/Tjekker, 'the men' was a self-determinative used by the people of Phoenicia, understood as an ethnonym by the Egyptians. Similar examples are commonplace such as 'Teutones/Deutsch' and 'Inuit', which mean the 'populace' or 'people' in their respective languages. Dor in southern Phoenicia is described as a town of the Tjekker in the Wenamun papyrus. The toponyms of Thutmose III include a Tjekker and an 'y-Tjekker' (Simons 1937, I, 136 and 196), where the latter might mean 'coast or island of the Tjekker'.[10] These references reinforce the idea that they were not a 'new tribe' who had arrived in the time of Ramesses III.

From an analysis of the archaeology and pottery repertoire of Dor, Gilboa (2005, 67, emphasis added) has developed a similar understanding to that of Goedicke:

> So what did the Egyptians have in mind when they wrote SKL? Based on the preceding analysis, it seems that they were referring to the mixed, *but largely autochthonous population of the Phoenician coast*.

And more recently (Gilboa and Sharon 2017, 292):

> ... the Dor evidence indicates that what the Egyptians called *Tjeker* should largely be understood as coterminous with what scholarship designates (or should designate) as early Iron Age Phoenicians. Similarly, an examination of the literary record pertaining to the *Tjeker* seems to indicate that this Egyptian term, rather than denoting any specific intrusive 'ethnic' population, was a geographical one, referring to a concrete (yet currently only loosely defined) region or regions and people in the Syro-Phoenician sphere.

A possible objection to this interpretation might be raised from one of the later letters from Ugarit, which refers to trouble with the people of the land of *Šikala(yu)* 'who live on ships'. Whether that means that the letter is referring to those *Šikala(yu)* who were maritime as

[8] For a comprehensive (and accurate) listing of all the mentions of the 'Sea Peoples' in Egyptian and other texts, see Adams and Cohen (2013).

[9] I have avoided here embroilment in the debate over a Cilician origin for the Denyen (based on the toponyms Danuna, Adana and Hiyawa), or their possible connection with the Greek Danaoi, on which an inordinate amount of (conflicting) literature has been published in recent years – though I hope to address this elsewhere. Suffice to say that there may be other ways of reading the evidence.

[10] For some rather muddled objections to the association with the Tjekker of Ramesses III's time see Redford (2006–2007, 10).

opposed to those who were not is difficult to say.[11] The letter also mentions the 'land of the city [kur.uru] of *Šikala*' (Dietrich and Loretz 1978). Written in Akkadian, it was sent by Suppiluliuma II of Hatti and the king's main request was for Ugarit to hand over one Ibnadušu (or Lunadušu), who had previously been abducted by the *Šikala(yu)*, so that the Hittite king can question him about the land of [the city of] *Šikala*. It has been argued by some that *Šikala(yu)* is a good match for Egyptian Tjekker. Yet it would seem most unlikely that *Šikala* is an Akkadian spelling for Canaanite Zakar, 'male'. (Though the letter is not from a native Babylonian speaker but was dictated by the Hittite king or minister to a scribe trained in Akkadian). Alternatively, Woudhuizen (2006, 113, 116), sees no reason to identify the *Šikalayu* with Tjekker, preferring to see them (as do Cline and O'Connor 2003, 113) as the Shekelesh.[12] On balance, we simply do not know whether the *Šikalayu* were anything to do with the Shekelesh or Tjekker, or were a different people entirely.

Group B

Shardana/Sherden *(Šrdn)***.** Undoubtedly homonymous with the Phoenician name for Sardinia: *Šrdn*, known from the mid-ninth century Phoenician 'Nora Stone' found on the island. Bronze statuettes from the island also show warriors with identical weapons and helmets to those of the *Šrdn* known from Egyptian reliefs from the reign of Ramesses II onwards (see conveniently James *et al.* 1991, 47, Fig. 2:8). There must undoubtedly be a connection, but whether the island gave their name to the people, or the people its name to the island, is an unsolved riddle. Vagnetti (2000, 319) drew attention to the tradition that the original name of the island was Ichnussa (Pausanias 10.17.1–2). There is also a hint that the word 'Shardana' may be from a West Semitic word meaning 'servant'[13], in which case one wonders whether it was simply a word for 'mercenary'. In any case, it seems likely that the Shardana gave their name to the island, and not *vice versa*.

The Shardana appear to have been a piratical/mercenary group, perhaps originally from the Aegean (Emanuel 2017). The long swords they are depicted as wielding by Ramesses II were initially a European/Aegean type before becoming commonplace in the Levant (Sandars 1978, 90–94; Drews 1993, 64, 192–208). We should also remember the horned helmet of the 'ingot god' from Enkomi, which might suggest a Shardana influence. In any case the Shardana were very familiar figures in the Eastern Mediterranean during the later LBA. They raided Egypt under Ramesses II and were allies of the Libyans against Merenptah. Ramesses II used some as mercenaries, but most importantly in this context, so did the LBA kings of Phoenicia before him. Three of the Amarna letters (81:16; 122:35; 123:15) from the king of Gubla (Byblos) mention that *Širdanu*-people, evidently Shardana mercenaries, have been attacked and taken by enemies (cf. Emanuel 2013, 14). Ras Shamra texts from the following century mention Shardana (Ugaritic spellings *ṯrtnm* and *srdnnm*) as mercenaries and detail their equipment, rations and land-plots; though apparently foreigners, they seem to have been so acculturated that some could have Canaanite names (Rainey 1979, 179–181). Perhaps this would account for the fact that those Shardana acting as mercenaries of the Libyans are referred to as being circumcised in the records of Merenptah (see below). The Phoenician connection is thus strong. Within a *CoD* chronology, which would date these instances of 'early Phoenician' employment of Shardana to the twelfth–tenth centuries BC, one can see a continuation of this relationship – in the Phoenician interest in Sardinia known from the ninth century BC onwards.

While they do not appear in the Year 8 inscription of Ramesses III, the *Šrdn* are mentioned in the historical introduction to *Papyrus Harris I* (composed in the time of his son Ramesses IV), in association with other names known from the coalition that attacked in that year (trans. Wilson 1969, 262):

> I extended all the frontiers of Egypt and overthrew those who had attacked them from their lands. I slew the Denyen in their islands, while the Tjeker and the Philistines were made ashes. The Sherden and the Weshesh of the Sea were made nonexistent, captured all together and brought in captivity to Egypt like the sands of the shore. I settled them in strongholds, bound in my name.

Warriors of the *Šrdn*-type with the characteristic horned helmets are conspicuous in the sea-battle depicted at Medinet Habu (alongside the *Plst* type). As the caption to the sea-battle does not specifically mention Shardana, some of these may well have belonged to ***Group C***.

Group C

The **'*-eš*' group** seem to have less connection with the Levant. The Teresh, Weshesh and Shekelesh (together with the Ekwesh of Merenptah's Libyan campaign)

[11] My thanks to Ronald Wallenfels for discussion of the Akkadian here.

[12] Adams and Cohen (2013, 656) also doubt this identification and draw attention to the homonymous name of ^{m}*Ši-qi-la-â*, a 'fortress commander' who brought plunder to Tiglath-pileser III in the land of Hatti (Annals Year 13). See Tadmor and Yamada (2011, 46).

[13] See Albright (1950, 167, n. 18): 'The supposed Amarna references almost certainly contain the word *šerda* (accusative of a *šerdu*, 'servitor,' from the verb which appears in Ugaritic as *šrd* (Heb. *šrt*), 'to serve' (connected with Accadian *wardu*, 'servant,' and *urrudu*, 'to serve'), and have nothing to do with the Sardinians, who do not figure in Egyptian inscriptions as mercenaries for another century or more.'

apparently have names indicating an Indo-European plural ending (similar to English 's'), though they are also reminiscent of western Anatolian placenames with the common ending *-assos.* Either way, these peoples may well have been Anatolian in origin. The frequent suggestion that the Ekwesh were Achaeans has always suffered from the grave objection that Merenptah's inscriptions state that they, along with the Sherden and Shekelesh, had no *qrnt*, foreskins (Manassa 2003, 56, 163), making their penises taboo for the Egyptian soldiers to collect for the count of fallen enemy – hands were collected instead. Kopanias (2017, 126) recently restated this well-known problem:

> It is probable that circumcision was not practised in the Late Bronze Age Aegean; therefore, either the Akawasha did not have an Aegean origin or the inscription contains an intentional or unintentional error.

The jury is still out on the names in this group, for whom there is little controlling evidence beyond their names, which allow too wide a range of suggested possibilities, e.g. for the Shekelesh from Sicily (unlikely) to Sagalassos (more plausible) in south-central Anatolia, though the circumcision issue would prefer a Levantine origin.[14] There is only diagnostic evidence, linguistically and perhaps archaeologically for the Teresh.

Teresh (*Tw-ry-š*). They do not appear in the main account of the Year 8 of Ramesses III. They are mentioned, however, in his 'Rhetorical Stela' (Chapel C at Deir el-Medina): 'The Peleset and Teresh sailed in the midst of t[he s]ea' (trans. Peden 1994, 64–65, l.8) or '[There ⌜sail]ed⌝ the Philistines and Tursha from the midst of the Sea (*yam*)' (*RITANC* 91.1112). A captured chief of the 'Teresh of the sea' is depicted on Ramesses III's mortuary temple on a relief showing Egypt's major enemies (see *Figure 3* below). As distinct from the other 'captured' chiefs (of Peleset and Sherden) who are shown with their familiar 'feathered' and horned headgear, the Teresh leader wears a turban. A link between the maritime *Tw-ry-š* and Troy (Hittite Taruiša) is very attractive,[15] especially in view of the finds of Trojan-style Grey Ware (though in small quantities) from coastal Levantine sites of the LBA/IA transition, supported by some interesting traditions (see below, **Allies, Mercenaries and Refugees from Troy**).

Where there is the most diagnostic evidence, we seem to be dealing with a military coalition, primarily composed of the 'Land Peoples', the Philistines (Peleset), Phoenicians (Tjekker) and Danites (Denyen), plus Shardana-type maritime warriors and others possibly from the Aegean world, including the Troad. But coastal Canaanites are the most prominent in this confederacy, and in particular the Tjekker who often appear first in Ramesses III's lists of 'Sea Peoples' enemies.

The Cypriot Connection

From the archaeological perspective, the 'Aegean' elements that appear in the Levant (particularly on the southern coast of Canaan) in the 12th century BC have almost all been proved to be of Cypriot origin, or to have come via Cyprus. I.e. the locally made LHIIIC 'Middle', copied at Ashdod which eventually gave rise to 'Philistine' Monochrome pottery. For a summary of the evidence see Middleton (2015, esp. 58–59):

> Yet while a novel material culture that we name 'Philistine' may have appeared, which took its inspiration from Cypriot versions of Mycenaean, and was mixed with local Canaanite culture, the implication that this was due to a migration rather than local people's agency in constructing novelties in material culture is still not proved. Philistine culture is found among Canaanite culture, and so can be thought of as part of it, rather as the Late Helladic culture of Greece, with its initial strong Minoan influences, appeared among and alongside its Middle Helladic predecessor without any necessary migration. Different aspects of 'Aegeanizing' culture in the Levant may mean different things. Some of it may be an expression of some sort of deliberate status differentiation within Canaanite communities, other aspects may simply indicate transfer of technological or religious ideas.

Bikai (1992) pointed out that there is no evidence of site destructions in Phoenicia *c.* 1200/1175 BC. Because of political reasons, excavation at primary Phoenician sites is very limited. Nevertheless, where work has been done, everything suggests continuity, e.g. at Tyre from Str. XV of the LBA through to Str. XI of the 9th century. Likewise at Sarafand, between Tyre and Sidon, Str. F of the 12th century appears to be a continuation of the preceding LBA strata.

To cite Jesse Millek ('Phoenicia at the Late Bronze–Iron Age Transition') from these *Proceedings*:

> While some have claimed that sites in Phoenicia did suffer havoc in *c.* 1200, most Levantine archaeologists have rejected the traditional destruction and Sea Peoples narrative for this region, as evidence is clearly lacking. (pp. 205–217.)

[14] Nibbi's ingenious suggestions (1969, 24–26) that the Shekelesh were from Ashkelon, and the Weshesh and Teresh from Usu and Tyre face insurmountable philological problems.

[15] Though there may be some problem with this linguistically; the '*-eš*' ending appears to be a plural, so for the people of Troia/Taruiša one might have expected a form like **Tršš* rather than *Trš*. But one can allow for simplification on the part of the Egyptian scribes.

The term 'Phoenicians' should not be taken to mean a change of populace or culture *c.* 1200 BC. It is best taken to simply describe a continuation of the coastal Canaanites of the Lebanon; alternatively, into the Iron Age, and their culture was essentially identical to that of the coastal Canaanites of Ugarit, Tyre, etc. known from the LBA. There were some small changes in the material culture, as one would expect over time. Small amounts of LHIIIC (along with contemporary Cypriot imports) appear at some southern Phoenician sites such as Acco, Tell Abu Hawam, Keisan and Dor, and a very few at Tyre, Sarepta and Byblos to the north (Gilboa 2005, 49, 51, 54). While these have often been assumed to indicate the presence of Aegean invaders, there is insufficient evidence of destruction or major cultural break at these sites. This would appear to suggest peaceful relations between those who made and used such pottery and the local population. Most likely the LHIIIC style imports came from Cyprus. Conversely, as Bikai pointed out, there was traffic in the other direction – imports of Canaanite amphorae (known from neutron activation studies to have been produced in the middle to southern Levant) are known from Cyprus, e.g. at Maa-Palaeokastro, from the 12th century BC.[16]

All suggests cultural continuity in Phoenicia (including contact with nearby Cyprus) across the LBA/IA transition. Bikai (1992, 133) was justified in asking this question:

> In most discussions of the events of the period, questions about whether the Phoenician cities were destroyed may be mentioned... but in the main the Phoenicians are omitted as though they were by-standers. Why is it, if they were by-standers, that when it was all over, they emerged as the power on the coast? More to the point, why are elements of Phoenician, Cypriot and Philistine culture so similar?

Next Bikai drew attention (as had Nibbi) to the fact that the more general 'captions' to the sea-battle relief of Ramesses III mention the 'Fenkhu' and 'Hau-nebu' as the enemies. From a meticulous analysis of all the texts, Egyptologist Claude Vandersleyen (1971) argued that both 'Fenkhu' and 'Hau-nebu' were New Kingdom terms for the Phoenicians. Hau-nebu is the most difficult term, as in bilingual Ptolemaic texts it indicates Greeks. The term also occurs in Old Kingdom texts, where it is unlikely to indicate Aegeans, and the transition in the meaning of the term may have come about from it having originally meant Phoenicians, then Cypriots, then later by extension Greeks.

Bikai therefore offered a novel (and somewhat ignored) interpretation of the 'Sea Peoples'. In as many words, she argues that they were basically Phoenicians, who with Cypriot and Aegean allies, ran amok at the end of the LBA, raiding far and wide and throwing off the last vestiges of the Egypto-Hittite alliance that once controlled the Levant.

Bikai's model is supported by some other strands of evidence. Manetho (Fr. 50) wrote that the king 'Sethôs also called Rhamessēs' possessed a great navy and that he set out and defeated Cyprus and Phoenicia. From the context, Sethos-Ramesses is clearly Ramesses III, and here we seem to be looking at a Ptolemaic interpretation of the Medinet Habu reliefs (see Kokkinos 2013, 162, n. 52; James 2021, 71–72). Naturally, Hellenistic readings of pharaonic reliefs could produce odd results. In his remarkably accurate description of the monuments at Thebes, Diodorus Siculus (1.47.6) talks of a war against the Bactrians shown on the pylons of king 'Osymandyas' (= Usermaatre Setepenre Ramesses II). The misunderstanding is explainable, as we can trace the simple mistake of reading Kheta (Hatti = Hittites) as Bekhten[17], then further into Bactria. Likewise, the Ptolemaic reading of the names of Ramesses III's main enemies (Cyprus and Phoenicia) should not be regarded as fantastic and were probably based on readings of the names 'Haunebu' and 'Fenkhu' on the Medinet Habu reliefs.

This is not to suggest on the basis of Manetho that Ramesses III conquered or even attacked Phoenicia and Cyprus – only that Manetho's understanding of Ramesses III's reliefs may have preserved a core of truth (with or without the help of oral traditions which may have lingered on at Medinet Habu). Egyptian interest in Cyprus in the time of Ramesses III is demonstrated by a string of toponyms on his Great Asiatic List (Simons 1937, List XXVII, Nos. 7–11): 7. *s-r-m-s-k*; 8. *k-t-y-n*; 9. *i-y-m-r*; 10. *s-r*; 11. *i-t-r.* They do not occur in any earlier lists of foreign place names (at least in any that survives) and they seem to reflect the names Salamis, Kition, Marion, Soli, Idalion.[18] It was Heinrich Brugsch who first drew attention to these parallels, in an Appendix to Schliemann's *Ilios* (1880, 749). His suggestions were overlooked until Wainwright (1961, 76) drew attention to them again. It is also noteworthy that scarabs of Ramesses III have been found in LBA tombs at Enkomi on Cyprus (Warren and Hankey 1989, 162). His interest in Cyprus (evident from the placenames) might suggest that he conducted (or attempted) naval manoeuvres in support of the Hittites in their campaign to the

[16] See Mountjoy (2010) for a discussion of some similarities between Cypriot and Philistine ceramics, and even possible influence from Philistia on Cypriot styles.

[17] As one can see from the Ptolemaic *Bentresh Stela* (Wilson 1969, 29–31), which has Ramesses II marrying a princess of Bekhten.

[18] Cf. the Sillu'ua (Soli) and Edi'il (Idalion) with no final 'n' in Esarhaddon's list of Cypriot kingdoms (see Oppenheim in Pritchard 1969, 317). For number 8 as Kition (?) see *RITANC* 94:4. Redford (2018, 143) accepts the identifications given here of numbers 7, 8, 10 and 11.

island, or that some of the 'Sea Peoples' he captured on his own border came from Cyprus. Any of these cases might explain, for example through the interrogation of prisoners (or allied Shardana), how Ramesses III's scribes had knowledge of Cypriot toponyms.

We know from Hittite Empire records that Cyprus was of crucial importance to them for its rich deposits of copper; Ugaritic letters record the arrival of shipments of copper from Alashiya. Two campaigns were conducted to Cyprus (Singer 2000, 27; Bryce 2005, 321–323, 332–333, 344–345). Tudhaliya IV subdued it and (as we know from correspondence from Ugarit), Alashiya again became the seat of a vassal king – probably a Hittite prince, which shows how concerned the Hittites were to keep close control over the island. But this was a relatively short-lived affair as the last Hittite Emperor Suppiluliuma II (an older contemporary of Ramesses III) moved again to fight the 'people of Alashiya' and the 'enemies of Alashiya' by sea and then apparently land. These references leave it unclear who exactly he was attacking or defending. As there is no later evidence for a Hittite vassal kingdom in Cyprus, Suppiluliuma's efforts must have ultimately failed. But, *nota bene*, Alashiya was still represented by Ramesses III as part of the Hittite confederation in his description of the run-up to the 'Sea Peoples' attack on Egypt. It appears to have survived as a Hittite vassal state just up to the point described at the beginning of Ramesses III's narrative.

Military and Naval Technology

From both the textual and archaeological evidence there is strong evidence for considering Phoenicia and Cyprus as central to the 'Sea Peoples' question. As pictorial evidence one can cite the famous ivory gaming box from a 12th-century tomb at Enkomi (see conveniently Pl. 14 of James *et al.* 1991). It depicts a hunting nobleman in a chariot, and both he and the driver wear typically Canaanite costume. Behind the chariot is a footsoldier with an axe, wearing a kilt and headdress which bear very close comparison to that of the 'Sea Peoples' of Ramesses III's reliefs. In fact, this image is the best non-Egyptian artistic comparison to the costume of the Peleset/Tjekker/Denyen depicted at Medinet Habu. The kilt he wears (tapering to a point at the front) is traditionally Levantine (familiar from 9th-dynasty tomb paintings of Asiatics) – so there is no good reason not to see him as wearing the costume of a Phoenician foot-soldier of the time. The next best parallel also comes from Cyprus, a depiction of a soldier with shield and 'feathered' helmet on a seal from Enkomi (see Emanuel 2015/2016, 20, Fig. 1g). Emanuel offers many other possible comparisons to this kind of helmet, including one from Mycenae and a number already cited by Mountjoy (1998, 59, Fig. 13: 1,2,3,4) on pottery fragments from the islands off the coast of southwestern Turkey, the region he defines as the East Aegean-West Anatolian Interface. Where the fragments are more complete, spikey hats are worn by rowers on ships. Some of the examples cited by Emanuel, such as the hedgehog-like hat of one line of soldiers on the famous Mycenaean warrior-vase are unconvincing parallels to the Peleset/Tjekker/Denyen headgear; some may simply be long spikey hair. The headgear depicted on the Medinet Habu reliefs is more ordered and resembles a fluted helmet. Though one could imagine that stiffened horse or other hair, or perhaps reeds (secured together like the bristles on a broom) could resist blows, bronze would also be a likely material. As well as protection, the added height given to a warrior would have been a psychological advantage in battle.

Caution is needed with regards to some of the alleged Aegean parallels. It should not be assumed, as often casually done, that the Aegean examples – even if convincing – are somehow the prototypes of the Peleset/Tjekker/Denyen helmet. The micro-chronology needs to be considered. As Emanuel (2015/2016, 9) notes:

> There is no clear Late Bronze antecedent [for the Aegean examples] in the region; the first secure representations appear at the end of the 13th and the beginning of the 12th centuries BC, which in Aegean chronology is defined as Transitional Late Helladic (LH) IIIB2–IIIC Early and LH IIIC Early (= Late Bronze III/Iron Ia). The vast majority of examples date to the LH IIIC Middle, roughly 1130–1070 BC.

N.B. the earliest 'secure' parallel mentioned by Emanuel is from a fragmentary krater from Bademgediği Tepe (near Izmir), of the transitional LH IIIB:2–Early IIIC which, as he notes, spans the early 12th as much as the late 13th century BC. Thus the time difference with the Year 8 reliefs of Ramesses III when these helmets were first represented in Egyptian art is negligible and it may be that the Near Eastern examples were the prototypes, copied in the Aegean region.

Once we break free from the assumption that the 'Sea Peoples' were fresh arrivals to the Eastern Mediterranean in the early 12th century BC, we should consider the flow of military styles working in the opposite direction – or at least in terms of reciprocal influence. The same is true of the ships which the 'Sea Peoples' are shown using on the Medinet Habu sea-battle relief, usually imagined to be a 'new' type from the Aegean. While a full discussion of this complex subject is well beyond the scope of the present study, the remarks of Vinson (1993, 133, my emphasis added) are of note:

> One of the most important innovations in ancient nautical technology was the invention of brails, specialized lines that Mediterranean sailors employed to shape and furl the square sails of

> their ships, used from some point in the Late Bronze Age until the close of antiquity. The 20th Dynasty relief at Medinet Habu of the sea battle between an Egyptian fleet and the so-called 'Sea Peoples' has usually been treated as the first appearance of brails in the iconographic record. *However, new study of less well-known material shows that Egyptians were acquainted with the technology no later than the Amarna period, though it seems doubtful that they themselves invented it.*

In fact the evidence is clear that while *every* element of the 'Sea Peoples' ships (hull, loose-footed sails, crow's nests on the mast) have good Canaanite antecedents much earlier than the 12th century BC, scholars have been very reluctant to accept this – see Emanuel (2016, 271–273), who admits that 'the search for the first "domino" in the process of maritime innovation is akin to considering the chicken and the egg.' Nevertheless, the Canaanite examples *are the earliest known* for all the relevant elements, going back to the 14th century BC. The significance of the ship-model from Gurob in Egypt has been somewhat skewed by the desire to see in it direct Aegean influence from Shardana mercenaries (Emanuel 2014, following Wachsmann 2013). Dating from the New Kingdom and by some to the late 13th–early 12th century BC, it is likely earlier to judge from the Mycenaean and Cypriot pottery (14th–early 13th centuries BC) found at the site (Gasperini 2014). It is an oared galley like those used by the Egyptians in the sea-battle relief at Medinet Habu. (Though surprisingly those of the 'Sea Peoples' are not depicted with oars in the surviving reliefs.)

Overall the details of ships from the Near Eastern region which compare to the 'Sea Peoples' galleys put in the shade other comparisons that have been made, such as those from as far away as the Central European Urnfield culture and even Denmark (Wachsmann 2000, 122–137).[19]

A Tenth-Century BC Focus

The Cypriot connection with the Philistine culture of the early Iron Age, and the probable role of Cypriots (whether Aegean or Phoenician in origin) in the 'Sea Peoples' episode under Ramesses III, does not need to be laboured further. But echoing Bikai, the role of Phoenicia *per se* needs to be put centre stage. How does Bikai's idea that the Phoenicians were the driving force behind the 'Sea Peoples' episode in the time of Ramesses III shape up on the *CoD* model? If Ramesses III reigned in the late tenth century BC, then we need to consider the consequences, however surprising. Ramesses III would have been a later contemporary of Hiram I of Tyre. The logic here forces us to consider the unheard-of possibility that Hiram was effectively the organiser of the so-called 'Sea Peoples' attacks on the Hittites and Egypt.

While our sources are meagre, it is clear that under Hiram I the power of the Phoenicians grew exponentially – to a peak they never seem to have achieved again as a unified people. It typifies the growth of a local regional state, as opposed to a city state, during the 10th century BC. During the LBA, as we can see from the Amarna letters, the kings of Sidon, Tyre and Byblos were frequently at odds, often warfare, with each other. But from references to Hiram of Tyre as 'king of the Sidonians' we know that he also ruled Sidon. Further 1 Kings 5:18 tells us, regarding the stones used for the building of the Temple in Jerusalem that: 'Solomon's builders, and Hiram's builders did hew them, and the Gebalites'. While some versions (such as the King James) translate 'Gebalites' as stone-squarers, the preferred translation here is people of Byblos (Gebal). If Hiram could command workmen from a vassal Byblos, then he must have ruled the entire Phoenician coast, from south of Tyre through Sidon to Byblos and probably further north as well.[20]

According to the *Tyrian Annals* apud Josephus (Kokkinos 2013, 41, 45), Hiram conducted a massive programme of temple and other civic building at his capital, Tyre. The tradition should not be taken lightly, given the city's earlier history. An Amarna letter (EA 89: 47–56, trans. Moran 1992, 162) from the ruler of Byblos compared the size and grandeur of the palace-complexes of Tyre and Ugarit. Concerning the 'mayor' (*ḫazannu*) of Tyre it states that:

> His property is as great as the sea. I know it! Look there is no mayor's residence like that of the residence in Tyre. It is like the residence in Ugarit. Exceedingly great is the wealth in it.

Assuming this was no exaggeration, we should also note that this was the nature of the palace at Tyre even when it lacked the resources of the hinterland that Hiram controlled – the Amarna-period island of Tyre must have drawn its wealth solely from trade and its resources of fish and murex.

[19] The comparison made was between the bird-head protomes on the prow and sterns of both the 'Sea Peoples' ships and ship iconography from central and northern Europe.

[20] While he agreed that the Gebalites were Byblites, Katzenstein (1997, 101 and n. 134) was misled by the dating of the Byblian inscriptions on statues of Shoshenq I and Osorkon I (Albright 1947) into thinking that Byblos was an independent kingdom in the 10th century BC. For a corrected dating using palaeography and a synchronism with firmly dated Assyrian chronology (rather than the unsound dates for the Egyptian 22nd Dynasty) see Wallenfels (1983; 2019, 494–498); 'Redating the Byblos Inscriptions' in these Proceedings, pp. 249–263; James *et al.* (1991, 248–251); van der Veen (2015b, 191–192).

The brilliance of the Phoenician builders in the time of Hiram is also testified in the biblical record, according to which the kings of the nascent monarchy at Jerusalem were dependent on Hiram and his craftsmen for advice, materials and expertise, first in building the palace of David, then in the construction of Solomon's Temple (plus presumably the house of the cedars of Lebanon). As an 'egyptianising' monarch, effectively the viceroy of Egypt in the Levant by virtue of his marriage to Pharaoh's daughter (James 2015a, 251–253; Niemann, 2018, 341, 346), we would expect to see many Egyptian elements in Solomon's more lavish buildings, such as the house he built for his Egyptian wife. However, with most of the builders and materials coming from Phoenicia (with even the casting of the trappings for the Temple being supervised by a Tyrian craftsman), the descriptions of Solomon's lavish buildings show them to have largely been within a Levantine tradition, with some Egyptian cultic elements (see Zwickel 2015). Cypriot influence also comes into play here, for example with the wheeled stands built for Solomon's temple whose description is closely paralleled by excavated Cypriot examples conventionally dated to the 12th century BC (James 2015a, 250).

Hiram's naval might is well known enough. His ally Solomon controlled the port of Joppa, which received wood and other materials from Phoenicia for his building programme in Jerusalem; his alliance with Hiram would have facilitated his navy to operate the arms trade that he ran between the Hittites and Egyptians (1 Kings 10:29): 'And a chariot came up and went out of Egypt for six hundred *shekels* of silver, and an horse for an hundred and fifty: and so for all the kings of the Hittites, and for the kings of Syria, did they bring them out by their means.' The Septuagint offers here a better reading, stating that Solomon's merchants 'came out by sea' (κατα θάλασσαν) an improvement on the Masoretic – i.e. *bayyam*, by sea, rather than *b'yadam*, by their means). Katzenstein (1997, 114) accepts this reading, which would suggest another joint enterprise between Hiram and Solomon. Hiram's fleet would have been much greater than anything commanded by Egypt, which so far had always relied on northern vassals for many of its seagoing ships. In the retrospective account of the achievements of Ramesses III in *Papyrus Harris I*, great stress is placed on his efforts to build up the Egyptian navy (trans. Wilson 1969, 260). As Wilson remarked, these sea-going vessels were 'perhaps all cargo ships'. The *qerer*-ships and *menesh*-ships were destined to bring goods back from 'God's Land', a broad (sacred) term for the East which included Punt (most likely in Somalia). The third kind of vessel, the *bari*-ships were equipped with bowmen, for use on the 'Great Green' (which despite Nibbi's courageous efforts can only have been the sea, both Mediterranean and Red Seas; see **Afterword**, n. 42). These would have been the ships used to repel the attempt by the 'Sea Peoples' to penetrate the Nile-mouths. They were probably Asiatic-style vessels. Before the late reign of Necho II (610–595 BC), the Egyptians seem to have had little naval capacity, relying instead on land routes to reach the Levant (James 2015b, 359–360).[21] Ramesses III may have been an exception in this respect, perhaps recruiting the help of the maritime Shardana who performed a similar role in late New Kingdom Egypt to that the Greeks and Carians played in Saite Egypt.

As the viceroy in Asia during the recession of the Egyptian empire at the close of the 19th Dynasty (under Sethos, Siptah and Twosret), Solomon would have had authority over the important port of Joppa, once controlled by the Egyptians. as well as nominal control over the Philistine cities (1 Kings 4:21). Hence, between them, Solomon and Hiram would have controlled the Levantine ports from Gaza to Byblos.

Hiram's navy was not only active in the Mediterranean but in the Red Sea, by arrangement with Solomon through his port of Ezion-geber, near Elath (1 Kings 9:26–28; 2 Chron. 8:18). Regarding its location, Bimson (in Bimson and Tebes 2009, 101–102) explained:

> If Tell el-Kheleifeh was not founded until the 8th century BC at the earliest, we must look elsewhere for Solomon's port of Ezion-geber. A likely candidate is the island of Jeziret al-Farun, a few kilometres away down the west coast of the Gulf of Aqabah. The straits between the island and the coast provide a sheltered anchorage and the island itself has a natural harbour. Significantly, surveys of Jeziret al-Farun have produced QPW [Qurayyah Painted Ware], leading Rothenberg to suggest it served as a harbour for the mining expeditions of the 13th–12th centuries BC.

In the *Centuries of Darkness* chronology this pottery would be compatible with occupation during the Solomonic period.

Hiram and Solomon's joint maritime project in the Red Sea was aimed at obtaining gold, ivory and exotic animals (1 Kings 10:22) from Ophir, which seems to have lain in southern Arabia or Somalia. Katzenstein (1997, 111) remarked:

> This attempt to break the Egyptian monopoly on gold by taking gold directly from Ophir was certainly regarded by Egypt as a hostile act, and was one of the many factors in the worsening of relations between Egypt and the northern kingdoms of Tyre and Israel.

[21] Necho II initiated a new *zeepolitiek*. This shift towards a sea-based policy was a natural one as the Saites (26th Dynasty) became increasingly reliant on Greek mercenaries, with naval as well as military expertise.

A consequence, as Katzenstein argued, were the Egyptian machinations under 'Shishak' to support the rebellions against Solomon (Hadad in Edom and Jeroboam in the northern kingdom). Given the *CoD* identification of 'Shishak' with Ramesses III several of his claimed major achievements are significant here: to have campaigned against the Shasu of Seir (Edom), restored the Egyptian Red Sea route to Punt (surely the Egyptian equivalent of Ophir) and to have renewed access to the mines in the southern Arabah.[22] Thus the two political-economic narratives, one from the late tenth and the other, conventionally, from the early 12th century BC, fit hand in glove. Katzenstein was right, except for his identification of Shishak with the later Libyan ruler Shoshenq I. He is not known, for example, to have made an expedition to Punt and the extent of his control, or interest in the mines of Edom is debatable.

In many respects Hiram was the 'senior partner' in the alliance with Solomon. He was senior in terms of age, as he had helped David build his palace in Jerusalem, though he also appears to have outlived Solomon. Rabbinic tradition remembers him as a hubristic megalomaniac who built himself a city in the sea (Ginzberg 1909, 104–105.).

Legend aside, Hiram must have been an extremely powerful and rich king, and very likely Solomon's tutor in 'wisdom' when it came to the benefits of trade. A historic problem with Tyre, however, was its lack of agricultural hinterland. At worst, as seen from the Amarna letters, Tyre could be restricted to its historic island. Though it may have been rich in fish and murex shells (for production of the famous Tyrian purple), fish and dye alone do not an economy make. Fresh water was always the main concern, and without control over the adjacent palaeo-Tyre on the mainland, the inhabitants of the island would have either been dependent on collecting rainwater in cisterns or importing it from neighbours. For this reason, at least, Tyre was constantly maintaining the struggle to control mainland Tyre (Usu) and other areas with resources of water, wood and stone. The prosperity of Tyre under Hiram shows that mainland resources (such as timber and stone) were under his control; but the same prosperity must have produced a rolling effect during Hiram's long reign (34 years) – with a growing population and industry, Tyre would have needed more agricultural land and other resources. This is exactly what we see in the Hebrew Bible. After twenty years Solomon had to hand over a strip of territory south of Tyre (the 'Kabul') to Hiram – to pay his debt for the help with building and materials (1 Kings 9:12–13; cf. 2 Chron. 8:1–2). The biblical accounts say that Hiram was dissatisfied with the land he was given, but Kitchen's balanced analysis (2003, 113–115) shows that Hiram acquired from the deal 'a good arable plain, with access to the ports of Accho and Shihor-Libnath (Tell Abu Hawam).' The latter port was of major commercial importance: more Mycenaean and Cypriot pottery has been found here than at any other site in the Levant other than Ugarit.

Demanding even further land from Israel would have strained the friendship between Hiram and his ally Solomon. This would have left Syria and Cyprus as the likely goals of further territorial ambition. Indeed the *Tyrian Annals* tradition, as reported by Josephus, reports that Hiram campaigned against 'rebels', the name of whom is best restored (see conveniently *CoD* 146–147 and 365, n.12) as 'Kitians', the inhabitants of Kition (or Cyprus generally?).

Allies, Mercenaries and Refugees from Troy

The identity of the 'Sea Peoples' falls comfortably into place if we assume that their instigator was Hiram. Of the three main protagonists, the Tjekker have been argued above to be Phoenicians, the Denyen Danites. while the Peleset are naturally the Philistines. There are some biblical indications of a Phoenician/Philistine alliance about this time. Traditionally composed by David's musician Asaph, Psalm 83:6–7 lists the enemies of Israel, specifically linking the Philistines with the Phoenicians: 'The tabernacles of Edom, and the Ishmaelites; of Moab, and the Hagarenes; Gebal [Byblos?], and Ammon, and Amalek; the Philistines with the inhabitants of Tyre.' Josephus (*Antiquities* 4:17.74) also speaks of the Phoenicians as being allies of the Philistines when they attempted to crush David's kingdom at Jerusalem. If historical, this would presumably have been before the alliance between David and Hiram (Katzenstein 1997, 74). The Danites long had a close relationship with the sea. In listing the Israelites who did (or did not) join the forces of Deborah in battle against the Canaanites, her song asks: 'Dan, why did he linger by the ships?' (Judges 5:17). This presumably reflects the northern branch of the Danites and is also in evidence in the time of Hiram. The craftsman, Huram or Hiram, that he sent to help Solomon was half Danite, half Phoenician (2 Chron. 2:1–14). Hiram delivered timber to Solomon through the port of Joppa, a Danite port. So it would not be surprising to see Danites (either or both northern and southern) in a coalition of troops organised by Hiram.

Regarding the Shardana, as noted above, they were employed by Phoenician kings during the LBA. If there is any truth in the Greek traditions of the 'wanderings' of their kings after the Trojan War, displaced from their homes when they 'returned' by insurrection, famines, plagues, storms, wrath of the gods, etc., then there would have been plenty of seasoned warriors seeking employment, fortune and new land to settle. I would

[22] As related in the historical retrospect to *Papyrus Harris I* – Peden (1994, 216–219).

read the traditions as closely as possible. They do not describe a mass migration of Mycenaean populations to the Eastern Mediterranean. Rather, they talk of displaced warlords and their warbands who, for one reason or another were unable to return to their home cities. An association with the destruction of the Mycenaean palace economy at the end of LHIIIB is commonplace and I think is largely explainable by an environmental upheaval – climate change and earthquakes – accompanied or followed by social upheaval.

The *CoD* model places the Trojan War *c.* 940 BC (at the end of LHIIIB) in the time of Hiram. Two of the traditions of the 'Returns' (or non-Returns) of the Achaean victors at Troy are particularly interesting. The displaced monarchs Agapenor of Arcadia and Teucer of Salamis were said to have settled on Cyprus, founding kingdoms at Paphos and (a new) Salamis (Apollodorus, *Epitome* 6.15a; Pausanias 8.5.2; 1.3.2). Bikai (1992, 137) drew attention to a story about Teucer, alluded to briefly by the Roman poet Virgil (*Aeneid* 1.613ff):

> Now I bethink me of when Teucer came to Sidon, exiled, and of Belus' power desired a second throne. For Belus then, our worshipped sire, despoiled the teeming land of Cyprus as its conqueror and king.

The story is fascinating, as it specifically claims that an exiled Mycenaean warlord from the Aegean Island of Salamis came to Sidon, became the ally of its king and assisted him in conquering Cyprus, while winning himself a new throne. Though it might reflect the name of Hiram's son Baalazar, Belus ('lord') is more likely a generic name for a Phoenician king (cf. the earlier Belus of Greek legend, brother of Agenor the king of Phoenicia – Apollodorus. *The Library* II.i.4). Given the *Tyrian Annals'* statement that Hiram put down a 'rebellion' of 'Kitians', 'Belus' here is likely to have been Hiram.[23] As a source Virgil is late, but the likelihood that he was retelling a genuine tradition finds support in some of the earliest post-Homeric sources. Writing in the early and late 5th-century BC respectively, both the poet Pindar (*Nemean Ode* 4.76) and the playwright Euripides refer to Teucer's resettlement in Cyprus. Euripides, who was a competent mythographer and folklorist, gives the most detail. In the play *Helen*, Teucer is described as wandering the Mediterranean for seven years after the fall of Troy; he lands on the coast of Egypt where he meets Helen and attempts to consult an Egyptian seer (*Helen* 143–150):

> Concerning the matters for which I came to these royal halls, needing to see the prophet Theonoe, you, be my proxenos, so that I may obtain prophecies about how I should set forth the wings of my ship on a fair wind toward the land of Cyprus on the sea, where Apollo has declared by oracle that I am to found a colony, giving it the island name of Salamis for the sake of my fatherland there.

The only element missing from Virgil's story is the alliance formed between Teucer and a Phoenician king.

In Euripides' play Teucer also reports to Helen that her husband Menelaus is missing, presumed dead. Yet on the fall of Troy, according to Herodotus (2.119), Menelaus first went to Egypt in search of Helen, but was driven out and went on to Libya. Homer adds that Menelaus, like Teucer, also visited Cyprus and Phoenicia; in the *Odyssey* (4.80-85) he roams the Mediterranean for eight years before finally returning home to Sparta, where he describes to Telemachus (the son of Odysseus) how:

> ... after many woes and wide wanderings I brought my wealth home in my ships and came in the eighth year. Over Cyprus and Phoenicia I wandered, and Egypt, and I came to the Ethiopians and the Sidonians and the Erembi, and to Libya, where the lambs are horned from their birth.

Menelaus tells Telemachus how he had collected much booty from Troy and Egypt on his travels, and had received a lavish gift from the king of Sidon (*Odyssey* 4.613–619 and 15.113–119):

> Of all the gifts that lie stored as treasures in my house, I will give you that one that is most beautiful and costliest. I will give you a well-wrought mixing bowl. All of silver it is, and the rims of it are finished off with gold, the work of Hephaestus; and the hero Phaedimus, king of the Sidonians, gave it to me, when his house sheltered me when I came there on my way home, and now I am minded to give it to you.

This passage was analysed by Caroline van der Brugge (2016), who describes it as 'a small piece of Tyrian history in the *Odyssey*'. She continued (van der Brugge 2016, 325–327) by proposing that the name Phaedimus (Greek 'shining') echoed that of the eighth–seventh-century BC Tyrian king Elulaios, but on very tenuous grounds. Rather, there would seem to be no good reason for not considering Phaedimus as a Greek epithet for Hiram, whose reputation for splendour must have been widely known. According to later traditions (Menander) his rebuilding of Tyre even boasted a golden pillar (Kokkinos 2013, 42–43; cf. Herodotus 2.44).

The traditions concerning Teucer, Menelaus and Odysseus have, of course, been compared many times to the piratical activities of the so-called 'Sea Peoples'. But what has received less emphasis is the connection of the 'Sidonians' in these matters. Bikai noticed the

[23] Elsewhere the *Aeneid* (1.729–730) refers to 'the first Belus and all his successors', as ancestral figures of Dido of Tyre (see Mackie 1993).

Phoenician ('Belus') connection with Teucer in Virgil, and the Sidonian connection with Menelaus is also strong (Kokkinos 2013, 43–45). As it happens, the Hiram connection with Menelaus is perhaps even more significant than the case of Teucer. They are specifically linked by Clement of Alexandria (*Stromata* 1.21/114.2; cf. 117.6), on the authority of earlier Greek histories of Tyre:

> Hiram gave his daughter to Solomon about the time of the arrival of Menelaus in Phoenicia, after the capture of Troy, as is said by Menander of Pergamus, and Laetus in The Phoenicia.

The same is said by Tatian (*Address of Tatian to the Greeks*, 37), also referring to Menander, and repeating on his authority that Hiram reigned at the time of the Trojan War (see Kokkinos 2013, 43, 45).[24] The 'synchronism' given here matches the *CoD* chronology perfectly. On the low dating for the Trojan War advocated by some ancient Greek chronographers, the Trojan War would have taken place *c.* 940 BC rather than the canonical (Eratosthenes) date of 1184 BC (Kokkinos 2009).

These traditions suggest that Hiram 'king of the Sidonians' acted as a magnet for temporarily landless Mycenaean warlords. Why is clear from the Teucer story: 'Belus'/Hiram needed allies for his conquest of Cyprus, somewhere which Menelaus was also said to have visited on his travels.

For the Teresh, displaced Trojans seem a good possibility. An interesting tradition reported by Athenaeus (*Deipnosophistai* 6.68), following Clearchos of Soloi (4th–3rd century BC), is that the aristocratic family at Salamis known as the Gerginoi said they were descendants of 'those Trojans whom Teucer took as slaves, having selected them from the captives, and then brought and settled in Cyprus, going along the sea-coast with a few companions, sailed towards Aeolis, in order to seek out and re-establish the country of his ancestors; and that he, taking some Mysians to himself, [formerly?] inhabited a city near the Trojan Ida, which was formerly called Gergina, from the name of the inhabitants, but is now called Gergitha.'[25]

These traditions find remarkable confirmation both from philology and archaeology. Simon (2015, 2016) has argued convincingly that the Hittite term Karkiša/Gargiša does not refer to the Carians in southwestern Anatolia but to a people located in the northwest; Oreshko (2017, 57–58; 2019) has identified them with the Gergithes of the Troad area. With respect to archaeology, wheel-made (or handmade) burnished grey wares (Anatolian Grey Ware or Trojan Grey Ware) of the kind well known from the Troad have been found throughout the Eastern Mediterranean at various sites dating to LBA/IA transition: at Ekron (Allen 1994; Na'aman 2000) and many sites on Cyprus including Enkomi, Hala Sultan Tekke, Kition, Kition-Bamboula, Maa Paleokastro, Pyla-Kokkinokremmos and Pyla-Verghi. Much of the pottery collected from these sites by Allen (1991) has been examined using neutron activation analysis and has been confirmed as being made from clay known from the Troad (Mommsen and Pavúk 2007).

Motivations

Drews' mercenary rebellion model for the 'Sea Peoples' (Drews 1993) has much to recommend it in terms of the rapid changes in military technology and strategy that arose near the end of the Bronze Age. But the idea that lightly armed 'runners', though they may well have been able to challenge in battle the chariot forces of the great empires (Hatti, Egypt, Ahhiyawa), would have simultaneously rebelled throughout the Eastern Mediterranean and then been able to have sacked large cities on the scale he envisages seems unlikely. This part of Drews' model also lacks a serious naval component. 'Runners' who were once adjuncts to aristocratic chariot-forces are far from being seamen. The last texts from Ugarit clearly describe threats of attack by sea, from enemies who are (unfortunately and intriguingly) nameless (Singer 1999, § 7.14). Drews' model also lacks a flashpoint. Where would such a movement have cut its teeth? An attack by Hiram on Cyprus or a Hittite possession in Syria such as Ugarit may have sparked off the events reported by Ramesses III in the Year 8 inscription, as part of a wider rebellion against the authority of Egypt.

So what, hypothetically, could Hiram's motives have been for organising a coalition of Phoenicians, Philistines and 'Sea Peoples'? The need for territorial expansion has been discussed above and the primary aim may have been to seize territory claimed by the ailing Hittite Empire. The Hittite hold over Cyprus was shaky: Tudhaliya IV conquered Cyprus, but Suppiluliuma II apparently had to reinvade it. When he did so, he may well have conflicted with Tyrian interests on the island, possibly ejecting a Tyrian vassal at Kition, and presumably replacing him with a pro-Hittite one.

[24] Manuel (1963, 308–309) remarked that Sir Isaac Newton (1728), though he was aware of them, might have used these references in his own reduction of chronology – had he not been obsessed by his doctrinaire ideas that the Hebrew was the 'original monarchy', leading him to lower the date of the Trojan War to some sixty or seventy years after the reign of Solomon. Not to mention Newton's attempt to date the voyage of the Argo by astronomy.

[25] Herodotus (5.122) identified the Gergithes who were still in the Troad in his own times as 'a remnant of the ancient Teukrians', by which he meant Trojans. Elsewhere (7.43) he refers to the tribe under the double-name 'Gergithes-Teukrians' locating them to the immediate east of Troy. See Oreshko (2019) for a thorough discussion of sources on the Gergithes. Teucer himself was thought to have been half-Trojan (by his mother Hesione, sister of king Priam – Apollodorus 2.6.4; 3.12.7)

The 'Tyrian Annals' refer to Hiram campaigning against a 'rebellion' of a people whose name is most likely to be restored as 'Kitieis' and generally agreed to be the people of Kition (see James *et al.* 1991, 365, n. 12; Katzenstein 1997, 84–85, 220, 227; Kokkinos 2013, 30 and n. 27). Commercial and maritime rivalry with the Hittites and their vassal Ugarit would also have played a part. From the very end of Ras Shamra, letters survive describing a shipping dispute between Ugarit and Tyre, with Sidon apparently acting as a go-between (see Vita 2017).

The Israelites as a whole would have played little role in these events, hence there is no mention of a 'Sea Peoples' invasion in the biblical record of tenth-century BC history. Its compilers were not interested in events in Anatolia, Syria, Cyprus or even Phoenicia and Philistia unless they had some direct bearing on the Israelites. Their theme for the second half of the tenth century BC was the decline of Solomon, both in spiritual and political terms (worship of foreign gods and the enemies that were gathering against his kingdom), the rise of Jeroboam, the death of Solomon, the breakup of the United Monarchy and the subsequent antipathy between Israel and Judah. The writers of the Hebrew Bible would have been totally preoccupied with these momentous, internal, events and not so with a failed naval raid on Egypt by the Philistines and others or the defeat of the Hittites by the same coalition.

All the same, it also seems likely that the death of Solomon could have provided a flashpoint for aggressive action by Hiram. By default, his southern ally would have been the faltering Rehoboam. On the *CoD* model, Ramesses III was backing the split of Solomon's kingdom, through his protegée Jeroboam in the north and his establishment of Egyptian centres at Beth-Shean and Megiddo (see Bimson 2015). Hiram would have no longer been obliged to respect the treaty obligations of his ally Solomon (towards Hatti and Egypt). In *CoD* (James *et al.* 1991, 386) we suggested that Ramesses III's Asiatic campaign of his Year 11/12 = the invasion of Shishak in year 5 of Rehoboam (c. 925 BC). Given this, Solomon would have died in Year 8 of Ramesses III:

Solomon last/Rehob 1	Ramesses III 08 ∞ 'Sea Peoples' invasion
Rehob 2	Ramesses III 09
Rehob 3	Ramesses III 10
Rehob 4	Ramesses III 11
Rehob 5 ∞ 'Shishak' ∞	Ramesses III 12

It is difficult to be precise at this stage of research and alternatively, it may be that Ramesses III began his incursions into Judah in his Year 8 (a model with which Nikos Kokkinos and I have been experimenting):

Solomon last/ Rehob 1	Ramesses III 04
Rehob 2	Ramesses III 05 ∞ 'Sea Peoples' incursion[a]
Rehob 3	Ramesses III 06
Rehob 4	Ramesses III 07
Rehob 5 ∞ 'Shishak' ∞ Ramesses III 08 ∞ 'Sea Peoples' invasion	

[a] This refers to an earlier, very brief, allusion by Ramesses III to a seaborne raid by the Peleset and Tjekker in his Year 5 (*RITANC* 25.5–11; Redford 2018, 15). Many think that this is simply a doublet of the events from Year 8, added to the Year 5 inscription as it was being completed (see Kitchen 2012, 14). Alternatively one might consider that the Philistines and Phoenicians had an alliance with the tribes to the immediate west of Egypt and timed a land and sea raid on Egypt to coincide with the Libyan incursion into Egypt in the Year 5.

Either model provides interesting 'coincidences' between Israelite and Egyptian history and suggests the idea that the death of Solomon provided a flash point for the activities of Hiram and his allies.

A Copper War

Economic considerations would have been paramount in these events. Somewhere in her voluminous writings, Nibbi made the perceptive remark that we should see the upsets at the end of the Near Eastern LBA in terms of wars over copper supplies.

During his alliance with Solomon, Hiram would have depended on supplies of copper from the mines in the southern Arabah. So Zwickel (2015, 150):

> The Edomite copper industry was rather important during the time of Solomon, because the trade on the Mediterranean Sea was likely not extensively re-established by the Phoenicians before the 10th century BC. Only those sea-faring trade activities made it possible to transport the cheaper Cypriote copper to the Levantine coast. As long as the sea trading activities were

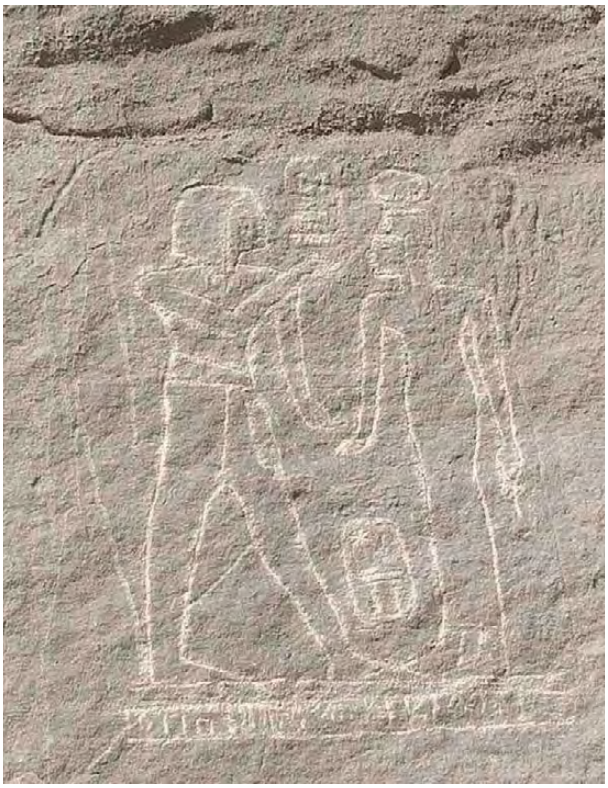

Figure 2. Relief of Ramesses III from Timna, showing the pharaoh making offerings to the goddess Hathor, patroness of mining (courtesy of Meretseger).

> not really well established, Solomon could earn money from selling copper to Levantine people.

On the *CoD* model Solomon's control of the southern mines happened during a recession of Egyptian control – under Merenptah's successors Seti II, Siptah, Twosret, Amenmesses and Setnakht ((James *et al.* 1991, 203; James 2015a, 246–247); see also Bimson in this volume, pp. 161–166. Near the end of Solomon's kingdom Edom rebelled (under Hadad), while his successor, the *roi fainéant* Rehoboam, is vanishingly unlikely to have recovered it, and along with it Israelite control of the copper sources. Rather, as we can see from the inscriptions at Timna, Ramesses III re-established sole Egyptian control of its mining resources after the gap since the reign of Merenptah (Avner 2014, 107).[26] Once Ramesses III had re-established Egyptian control of the southern Arabah, Cyprus would have remained the only credible source of copper for the Phoenicians.

The 'Vile Chief of Amor'

The 'chief of Amor' is referred to disparagingly a number of times in Ramesses III's texts (see Kahn 2010). His Year 5 inscription (*ARE* IV: §39) claims that:

> Gored is the chief of Amor, in his blood his seed is not; all his people are taken captive, carried off, spoiled. Everybody in his land comes with praise that the great sun of Egypt may look upon them, that the sun-disk may be turned to them, the Sun... coming forth, rising upon the earth, the warmth of Egypt, which is in heaven.[27]

As Rainey notes (2001, 58; cf. Kitchen 2012, 14–15): 'Scholars have long known that this 75-line inscription contains material from the eighth regnal year, although its opening date is the fifth year.' The defeat of the chief of Amor and 'all his people' may thus reflect events from the year 8 of Ramesses III, added to the text dealing with the first Libyan invasion of year 5 while it was in completion.

Breasted (*ARE* IV, 21, n. c) thought that this reference to Amor reflects the Year 8 inscription which describes the devastation of one place in Amor by the 'Sea Peoples'. This seems unlikely. The context suggests that Ramesses III is here claiming (rightly or wrongly) that he and his troops had defeated Amor and taken captives. The other most detailed reference to this figure suggests the same, from a record relating to Ramesses III's Syrian campaign (of year 11–12), which accompanies the relief of two captive chiefs (*ARE* IV, §127):

> Utterance of Amon, Lord of Thebes, to his son, Lord of the Two Lands, Usermare-Meriamon: 'Come thou in peace, O Good God, lord of might, captor of the Nine Bows, at thy return having crushed the countries. Thou hast slain their chiefs, who were beginning to trespass thy boundaries, they fell by thy blade. The countries that knew not Egypt come to thee, (with) their tribute upon their backs, from the victories which I have decreed to thee forever.'
>
> Utterance of the wretched chief of Amor, and the wretched, vanquished chief of Libya: 'Breath! O

[26] The finds of an elaborate faience vase of Twosret at Sidon must be a prestige gift, showing friendly relations with Phoenicia, while the discovery of another at Deir 'Alla may be significant as the site seems to be the biblical Succoth – it was between Succoth and Zarethan (plausibly Tell es-Sa'idiyeh) that Solomon cast the bronze furnishings for the Temple (1 Kings 7:46). Considerable evidence for a bronze-casting industry has been found there from the LBA/IA transition (see James 2015a, 250–251). If the vase from Deir 'Alla represents a visit from Twosret to this region it would support the deduction (on the *CoD* chronology) that she was the historical prototype of Solomon's ally the 'Queen of Sheba'/a.k.a. the 'Queen of Egypt and Ethiopia' of Josephus (see Kokkinos 2013, 163–165).

[27] Rainey (2001, 58) translates: 'While the one of Amor (Amurru) is ashes, his seed is not, all his people are captured, every survivor from his land comes with praises to behold the Great Sun of Egypt above them. All his people are scattered and his land is no more.'

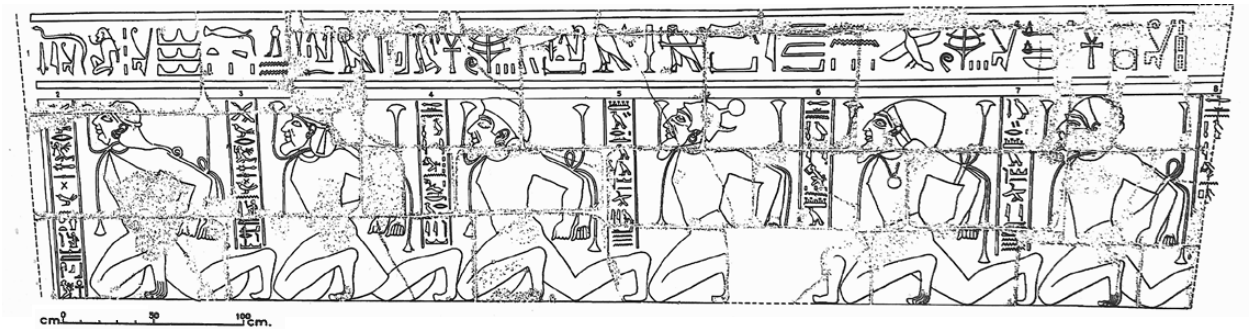

Figure 3. The row of captives from the façade of the north tower, eastern High Gate, Medinet Habu. On the far right, the image of the seventh figure, the Chief of Pe[leset], is missing. From MH VIII, Pl. 600 B (© Oriental Institute Chicago).

> Good ruler, strongarmed, great in might. Thou art verily the son of Amon, thy form is like him. He has assigned to thee every land, together overthrown beneath thy feet. Thou art like the sun, when he rises; men live at thy appearance. Give to us the breath, which thou givest, that we may hail thy double serpent-diadem, that we may speak of thy might to our sons' sons ....'

The composition summarises Ramesses III's achievements in defeating his northern and western enemies, 'trespassers' of his boundaries, as typified in this scene by two arch-enemies, the kings of Amor and Libya. Another tableau (*ARE* IV, §129; Simons 1937, 197, List XXXI; Medinet Habu Vol. VIII, Pl. 600 B; *RITANC*, 104.14), composed in the Year 12 and evidently drawn up after northern campaigning, depicts seven pinioned enemies (*Figure 3*, left to right), headed by a man of Hittite appearance: (1) 'Wretched chief of Hatti as a living captive' (2) Wretched chief of Amor (3) Chief of Tjekkery (4) Sherden 'of the sea' (5) Chief of Sha[su][28] (6) Teresh 'of the sea' (7) Chief of Pe[leset].[29]

It should not be automatically assumed that captive (1) necessarily represents the central polity at Hattuša; other candidates would be the rulers of the Hittite cadet-states of Tarhuntassa [Pamphylia], and Carchemish where 'Great Kings' were assuming more power towards the close of the Hittite Empire. Despite this question, in a period of shifting alliances, the list overall looks like a fair representation of Ramesses III's northern enemies.[30]

Interpretation of the texts mentioning Amor, particularly those relating to the Syrian campaign of yr 11/12, is a minefield. The reliefs for this campaign contain stock images (including attacks on two fortresses one manned by Canaanites, the other by people of Hittite appearance) and were once thought to be simply copied from those of Ramesses II. In terms of imagery they were to a certain extent, but opinions regarding their originality have changed in recent years (Kahn 2010, 16; James 2017, 63–87). Redford (2018, 143) also defends these scenes against the charge of being 'generic and unhistorical...slavishly copied from the military art of Ramesses II.' Even if some images were copied from 'stock footage' from Ramesses II, this does not in itself mean that Ramesses III had no Syrian campaign. In one of the battle scenes a 'city of Amor' is shown besieged, while the utterance 'of the chief of the city of Amor' is given.[31] The following battle and captive scenes show typical Asiatics ('Syrians'), with another more generalised one showing captives from other campaigns admixed with some of Libyan and Peleset costume.

[28] Contra Wainwright (1961, 83). Wainwright himself gave the reasons why his restoration of 'Shekelesh' was unworkable. First: 'The lacuna at the end of the man's name is so small that it must remain a mystery whether so long a name as Shekelesh could ever have been fitted.' Wainwright unfortunately resorted to the idea of bad planning by the sculptor, suggesting that he misjudged the space available. The restoration Sha[su] obviates the need for this speculation. Second, he noted that the 'headcloth' was worn by enemies encountered by Seti I in his Shasu campaign (first year) but offered no explanation of what Shekelesh may have been doing in the battle. Here and elsewhere this headdress is characteristic of Shasu people (see Wente 1963). Otherwise, the appearance of a Shasu chief in this scene is to be expected from the stress given in *Papyrus Harris I* to Ramesses III's campaign against the Shasu (see James 2017, 74–77), control over whom allowed the Egyptians to access the mineral wealth of the Sinai, southern Arabah and the trade routes into Arabia.

[29] The panel to the right depicting the Philistine chief is largely broken off.

[30] The Ramesses III version of the Blessing of Ptah (also Year 12) makes subtle changes from the earlier Ramesses II version which it copies: replacing 'Hatti' with 'every land' and instead of a Hittite bride mentions 'their sons and daughters as slaves to thy palace' (*ARE* IV, §134; Kitchen 1996, 107). It omits the final part altogether, which referred to friendly Egypto-Hittite relations (Kitchen 1996, 107; 1999, 161). One can only conclude that by the Year 12 the Egypo-Hittite alliance, still active under Merenptah and perhaps earlier in the reign of Ramesses III, had finished (see James 2017, 89).

[31] Redford (2018, 141) has suggested from a slight variance in spelling from that regularly used for Amor, that the city in question here is Emar. While his philological question may be valid, his further argument that this cannot be Amurru, as it had been 'annihilated' in the Year 8 is reading far too much into that earlier text.

Where exactly such battles may have taken place is hard to tell, but the first 69 names of Ramesses III's Great Asiatic toponym list (which form an original composition) include places that were within the Carchemish region and the city itself occurs as *k-r-k-m-s* (No. 29), just after *m-t-n* (No. 28), Mitanni (Simons 1937, 165–166, List XVII). While it is extremely unlikely that he attacked Carchemish itself, perhaps his troops or scouts reached its environs.

There are thus good reasons for not rejecting out of hand Ramesses III's reliefs depicting a 'Syrian campaign'. Assuming that this campaign in yr 12 = the Shishak event, one could reconstruct events as follows. In his year 8, as he claimed, Ramesses would have already begun reprisals on the Philistines and Denyen by raiding some of their towns. The way would then have been prepared for an attack on Rehoboam's 'fenced cities' in the Shephelah and southern Judah, which may have occupied Ramesses III for three or four years. He could then have moved rapidly northwards into the territory of his ally Jeroboam of Israel, establishing Egyptian military bases at Beth-Shean and Megiddo – and from there, apparently skirting Phoenicia, further north to make incursions into Syria. While it now seems clear that Ramesses III campaigned in Syria, perhaps on the periphery of Phoenicia, there is no evidence that any of the sites he attacked lay in its heartland, as Sayce (1912, 206) noted regarding the great Asiatic toponym list: 'One point about this list is very noticeable. None of the great Phoenician cities of the coast are mentioned in it. Acre, Ekdippa, Tyre, Sidon, and Beyrout are all conspicuous by their absence. Even Joppa is unnamed.'[32]

As to the despised 'chief of Amor', I have tried to work out through a process of elimination whether he can be identified as a known historical figure from the late 10th century BC. I have already touched on the idea of identifying Solomon with another *bête noire* of Ramesses III's records, the *Sw* or *Irsw* mentioned in the historical introduction to *Papyrus Harris I* (James 2015a, 248–249; see also Wallenfels 2019, 494; James 2021, 71). According to Goedicke (1979) this individual had not taken over Egypt *per se* as is usually thought, but the Egyptian empire in Asia – in the 'empty years' before the rise of Setnakht and Ramesses III. However, *Irsw*/*Sw* is not described as from Amurru, but as a 'Kharu', and hence a Canaanite, suiting the location of the heartland of Solomon's kingdom in the southern hill county. Further, on the *CoD* model, Solomon's reign would have overlapped only with Ramesses III's earliest years. The idea of his son Rehoboam unrealistically claiming to have ruled in Syria is vanishingly unlikely; he was small fry once the United Kingdom had redivided.

A better candidate would be the last Hittite vassal ruler of Amurru, Shaushga-muwa. While Amurru always had a more generalised meaning in Akkadian (the 'west', and Syria as a whole), the activity associated with Amurru (in the Amarna letters and Ramesses II's Qadesh records) suggests that its focus was the coastal region of Syria (including Phoenicia). Shaushga-muwa's capital ('the city of Amurru') was almost certainly Simyra in northern Phoenicia (Stieglitz 1991). But this Hittite-ruled state seems to have long gone by the mid-tenth century – perhaps by as much as two generations. Shaushga-muwa is last attested in the time of Tudhaliya IV (generationally *c.* 1000 BC/*CoD*), and no subject kings of Amurru are known from the time of the last Hittite Emperors Arnuwanda III and Suppiluliuma II.[33] Possible successors to the title 'king of Amurru' could have been the rulers of the nascent Aramaean kingdom of Aram-Zobah/Aram Naharaim. Yet this was an inland kingdom in northern Syria which stretched to the Euphrates, more in the region of the erstwhile kingdom of Mitanni than the coastal region that comprised LBA Amurru.[34]

Further information about Amurru comes from the records of Tiglath-pileser I (1114–1076 BC/*CoD c.* 1000–960 BC). Mid-reign, he crossed the Lebanon range and marched into the 'country of Amurru', naming its cities as Byblos, Sidon, Arvad and Simyra (RIMA 2, 37, A.0.87.1).[35] As he was a later contemporary of Šaušga-muwa on the *CoD* chronology, he may have been referring to the vassal-state of Amurru in its last days, though he does not name any kings. Tiglath-pileser I did not seem to have marched as far south as Tyre to force it into tribute, but it was included in a subsequent Assyrian record of Assurnaṣirpal II (883–859 BC). He listed the following cities as those of the seacoast of the 'Great Sea of the land Amurru': Tyre, Sidon, Byblos, Maḫallatu, *Ma-i-za*, *Ka-i-za*, Amurru (most likely = the city of Simyra) and Arvad (RIMA 2, 218, A.O. 101.1). The position of Tyre at the head of the list, though in a south to north listing, shows its importance within the coastland of Amurru. The continued use of Amurru as a geopolitical term for Phoenicia may be supported by other evidence, from the 'Proto-Canaanite' inscriptions. If two unprovenanced bronze arrowheads with alphabetic inscriptions of a Zakar-baal 'king of Amurru' are genuine (pers. comm. van der Veen) it is

[32] Katzenstein (1997, 62) states that Tyre was included in his long toponym list (No. 121) but the context is too uncertain to be sure (see James 2017, 94, 103).

[33] *Pace* Singer (2010) whose extra king Maḫḫaza is not certainly attested as a king of Amurru.

[34] Until Aram-Zobah was usurped by Damascus in the ninth century BC; yet again Damascus does not seem to have had direct control of states nearer the coast, and only through vassals (see James and van der Veen 2015, 130–132).

[35] In a separate passage (RIMA I, A.0.87.1) describing his defeat of the wide ranging Aḫlamu-Aramaeans as far as the borders of Karduniash, Tiglath-pileser I names the other extreme of 'Tadmar in the desert' as being in the land of Amurru. This either uses Amurru in its broader sense or may mark the eastern extremity of the political state of Amurru.

tempting to see them as belonging to Zakar-baal, the king of Byblos of the Wenamun story, which would date to the 860s BC on the *CoD* chronology, in step with van der Veen's redating of the inscriptions.[36]

If, as argued above, the Hittite vassal state of Amurru had finished early in the 10th century (say, in the first quarter), then its natural successor would seem to have been Phoenicia, and more specifically the kingdom of Abibaal and Hiram, based at Tyre and Sidon but including Byblos, once a part of Amurru. On balance, the most likely candidate for the troublesome 'Chief of Amor' in the texts of Ramesses III was a king of Phoenicia and, inevitably (on the *CoD* dating) Hiram of Tyre. As noted above, there are strong suggestions in the Egyptian texts that this 'Chief of Amor' was implicated in the troubles that started in Year 8.

The Foreign Countries and Their Isles

To sum up with a concluding experimental scenario. The 'conspiracy' conceived in or with 'their isles' by the 'foreign countries' seems to have begun when Hiram of Tyre gathered together in Phoenicia a large army of force including troops drawn from Philistia and other coastal areas, as well as sea rovers from the Aegean. 'Isles' is a particularly apposite term for the Phoenician cities Tyre, Arvad, Byblos and Sidon, which were all situated on islands and/or peninsulas – something that made an impression on both the Egyptians and Assyrians. The late 19th-dynasty satirical letter (*Papyrus Anastasi* I, 21.1–3; Gardiner 1911, 23*) includes this observation: 'They tell of another city in the sea, 'Tyre-the-port' is its name. Water is taken over to it in boats, and it is richer in fishes than in sand.' Esarhaddon not only described Tyre as '(an island) amidst the sea' but Sidon as well: 'Sidon which lies (on an island) amidst the sea' (trans. Oppenheim *apud* Pritchard 1969, 286).

To the north of Byblos, the city of Arvad lay on a small island off the coast of southern Syria. It had two harbours, perfect for manning fleets and trading but, for the same reasons as Tyre, was otherwise short of resources. Its naval activity was characterised as illegal and piratical by some of the Amarna correspondents. Knapp (2018, 47) summarises:

> The main activities of the ships and men of *Arwada* were: (1) an attack against and a naval blockade of the Amurru port town of Ṣumur (EA 104, 105); (2) intercepting three Byblian ships at sea, bound for Ṣumur (EA 105); (3) the sacking of Ullaza (northern Lebanon), another, nearby port (EA 105); (4) blockading, or at least controlling the ports of Šigata and Ambi to prevent the delivery of grain to Ṣumur (EA 98); and (5) an attack against Tyre's port (?) of Ušu in support of Sidon, thus threatening the seaborne activities of Tyre itself (EA 149). In EA 101, a difficult letter to interpret (Liverani 1998), the author (probably Rib-Adda, ruler of Byblos) asks pharaoh (Amenhotep III or IV) to seize the ships of the men of *Arwada* to prevent them from attacking Byblos.

Emanuel (2020) has also discussed the Amarna Arwadites, their 'piratical' activities and possible relationship to the enigmatic Miši people (Mycenaeans?) with whom they seem to be linked.

From a later period, Ezekiel (27:8 and 11) mentions the men of Arvad as sailors and soldiers employed by Tyre. While there is no way of telling whether Hiram recruited islanders from Arvad into his forces, there is no reason to imagine that they changed the nature of their maritime activities during the rest of the Late Bronze Age and the 'crisis years' that closed it. More generally, Stieglitz (1990, 9–10) made a good observation here regarding the Amarna references to Canaanite fleets:

> It was that shipping heritage, coupled with the fact that Egypt steadily lost its political grip on the Phoenician littoral, which accounts for the rise of those Phoenician city-states in the Early Iron Age.

Again we see how, on a lowered Egyptian chronology, developments at the end of the LBA dovetail with those of the late 10th century BC – with the declining power of Egypt in the Levant coinciding with the rise of Phoenician naval power.

The Attacks on Alashiya, Amurru and Egypt

The purpose of the coalition envisaged here as brought together by Hiram would have been, initially, to challenge the waning Hittite authority over Cyprus and the north Syrian coast. One aim would have been to avenge potential loss of Tyrian interests during the recent Hittite invasion of Alashiya, to secure access to the copper deposits of Cyprus (after Ramesses III had resecured the Timna deposits for Egypt) and to knock out the commercial rival of Ugarit (protected by both Hatti and Egypt – see **A Copper War**, above). A conflict with Hittite forces on the island may be remembered in the Tyrian Annals as Hiram's subjugation of the 'Kitians' and in classical legend as the conquest of Cyprus by 'Belus' and Teucer, the Mycenaean from Salamis (see **Allies, Mercenaries and Refugees from Troy** above). The struggle is most likely reflected in the account of the last Hittite Emperor Suppiluliuma II. It seems that his campaign to Cyprus was unsuccessful;

[36] Though they are usually dated to the eleventh century BC, van der Veen (2015b, 195) argues on palaeographic grounds that a date in the mid-ninth century is preferable. Other arrowheads, of equally uncertain authenticity refer to men (mainly soldiers) from Sidon, Tyre and Kition (!).

despite his claim to have won a sea battle off its coast, Suppiluliuma then had to fight (anonymous) enemies on dry land, possibly in Syria or the Anatolian coast of Cilicia: 'It would seem that despite the alleged victory in the open sea, Šuppiluliuma was followed back to his own haven by the enemy hordes.' (Singer 2000, 27.)

The loss of Cyprus (Alashiya) and the neighbouring seaways could have dealt a major blow to the Hittite Empire. It would have deprived the Hittites of a vital resource of copper and interrupted communications with Egypt. The importance of Cyprus on the route for supplies to Hatti at a time of famine in Anatolia was stressed by Bryce (2005, 323):

> Under the control of a native or foreign regime hostile to Hittite interests, it had considerable potential for disrupting transhipments of grain from Egypt and Syria to a port on the southern Anatolian coast.

Hence Hiram's conquest of Cyprus (perhaps accompanied by a blockade of the Hittite port of Ura in Cilicia) would have dealt a double blow to the Empire. And Hiram would now have had access to Cypriot copper deposits, for re-arming his forces and vital for the metal fittings for maintaining his fleet.

Ramesses III relates that a 'camp' was set up at 'one place' in 'Amor', which was made desolate. While this is usually understood to mean that the 'Sea Peoples' destroyed the land of Amurru (see e.g. Cline and O'Connor 2003, 110, 117), it is actually far from clear what was devastated: Amor, the unnamed 'one place' or the camp? The caution of Breasted (*ARE* IV: §64, n. e) regarding the damaged text should not be overlooked:

> The lacuna is hardly large enough for a verb. The end of the plural suffix *(n* of *slt)* is visible before '*camp.*' If we read 'their' *(p' ysn),* it would fill the lacuna, and we should necessarily render: '*Wasted was their camp, etc.,*' meaning the camp of the allied Syrians, which was wasted by the Northerners.

In short, it is unclear whose 'camp' (*ihy/ihw*) it was and whether it was this or Amor that was 'wasted (*fḫ*)'.[37]

If it was 'one place' in Amurru that was destroyed, this might be a reference to Ugarit, a likely target for Hiram as a commercial rival. One might question whether Ugarit was strictly speaking within Amurru/Amor,[38] though Hittite and Egyptian geopolitical terms would have differed. But there were other valuable cities, to the south of Ugarit, which technically were in that geographical region – such as Simyra, the 'city of Amurru'. Had he taken it, Hiram on these grounds alone could legitimately claim the title 'king of Amurru'.

Next the coalition moved southwards with the aim of attacking Egypt. Whether Hiram (or his son) actively led the attack on Egypt is another question. One cannot rule out the possibility that Hiram's partly mercenary army had, by now, developed its own agenda and become beyond his control. Modern parallels in the Levant, and particularly the Lebanon readily come to mind. Yet ultimately the assault on Egypt would have been part of the same organised conspiracy, called by proclamation (*šdtt*) and instigated by Hiram. The motive would *not* have been to conquer or settle in Egypt, as the standard versions of the 'Sea Peoples' often have it. As Drews (1993, 1998, 2000) has stressed, booty from the gold-rich Egyptian temples of the Delta would have been the motive for the attack, had the raiders managed to successfully penetrate the river mouths. Second, having effectively dealt a crippling blow to the Hittite Empire (in Syria and Cyprus), the coalition may have wanted to inflict a humiliating defeat on its old ally Egypt, first to ensure that the Hittite-Egyptian stranglehold on the Levant was finished once and for all, second to ensure the independence of the Philistines who played a major role in the coalition.[39] Nor is there any way of telling how many such troops actually advanced on Egypt by sea. The original coalition may have bested the combined might of the Hittite Empire, but Ramesses III certainly did manage to stop them at his border. The naval force was defeated in the Nile mouths and in an impressive geoarchaeological study Hoffmeier (2018) has convincingly located the sea battle in the palaeo-lagoon of Shihor which fed into the eastern (Pelusiac) branch of the Nile. However, Hoffmeier also attempted to place the land battle at the same location, whereas Ramesses III clearly says that this took place in Djahi (Canaan). The interpretation of Drews (2000) here is convincing: the reliefs of the land battle show Ramesses III driving northwards against the Philistines, Tjekker and Denyen in retaliation for the raids; the families depicted in ox carts were not part of a migranting force from the Aegean or Anatolia planning to live in Egypt, but refugees running from their settlements in southern Canaan attacked by Ramesses.

[37] Redford (2018, 36, n. 15) prefers 'his' (the ruler of Amor) to 'its' or 'their' and gives the camp the active role: 'A camp was set up within one spot in Amurru, devastating his people and his land as though they had never existed.'

[38] While it is difficult to discern whether Ugarit was geographically within Amurru (though this seems likely), during the Amarna period it temporarily came under its domination. In a highly advantageous treaty with Ugarit Aziru of Amurru effectively subjugated Ugarit and made it a tributary (Singer 1991; Halayka 2010, 298, n. 3).

[39] That they succeeded can be seen from the almost total absence of any Egyptian remains from the Early Iron Age Philistine cities, compared to other parts of Canaan. See for example Bietak (1993, esp. maps pp. 294–295). Both Ramesses III and Ramesses IV seem to have used a route through the Shephelah (largely skirting Philistia) to reach the Judaean hill country and the Jezreel Valley beyond (James 2017, 128–129; James and Bimson in prep.).

Ramesses III incorporated many captives from the coalition into his own army, placing them in military camps within his own territories, but not in Philistia – as distinct from the old Albright/Alt paradigm which would have Ramesses III effectively founding the cities of the Philistine pentapolis by means of captured Aegean migrants. His reliefs show he settled some captives on his western and southern borders, as they depict warriors of the Peleset/Tjekker/Denyen type assisting his troops in battles with the Libyans and Nubians.

Many other considerations are left to informed guesswork. For example we are told that the kings of Philistia were vassals of Solomon (reasonable in the *CoD* model in which he was the Egyptian viceroy in the Levant). His son Rehoboam, whose kingdom was restricted to Judah and Benjamin, would certainly have lost that authority. When Shishak invaded Judah in Rehoboam's yr 5, the Pharaoh must have already been taking the Shephelah out of Judahite control. As noted above, if Rehoboam 5 = Ramesses III Year 12, then Ramesses' Year 8 reliefs showing an attack on the Philistines, coincides with the death of Solomon. On the death of Solomon, Hiram may have also extended his sway over Philistia, in the power vacuum created by Solomon's demise.

What would have been the fate of erstwhile Israelite possessions on the coast, north of Philistia? The grudging grant of Kabul by Solomon shows that they were always coveted by Hiram. And, as noted above, Hiram delivered wood to Solomon via Joppa, in the traditional territory of the Danites. One assumes that Hiram's merchants had other interests in the port (i.e. they did not return home with empty ships and were at least picking up supplies of olive oil, wine, copper and other produce). When Solomon died, who did Joppa 'belong' to, or who was it claimed by politically? There is no evidence that either Jeroboam or Rehoboam claimed this territory, whereas Hiram had a powerful enough fleet to back a claim during a political hiatus.

The fact that Joppa lay within the traditional territory of Dan raises an interesting possibility. As noted above, Hiram seems to have had connections with both the southern and northern Danites and it has been argued above that they played a role (as the Philistine lookalikes Denyen) in the 'Sea Peoples' affair. When the United Monarchy split into Judah and Israel, where did the Danites stand? On the model suggested here they joined neither Judah nor Israel but, because of their long-standing affinities with Phoenicia and Philistia, became part of Hiram's party.

Displaced Warlords

For those who might prefer a more Aegean slant to the matter, further twists can be made to the Rubik's cube while preserving Hiram's centrality. For example, were we to eschew (ill-advisedly) the Tjekker equation with Phoenicians, then the suggestion that they were connected with the 'Teukroi' of Greek tradition could be considered.[40] This would link with the tradition that Teucer brought the Gergithes from the Troad to Cyprus and the evidence regarding the Teresh (see above), while another tradition (Nonnus *Dionysiaca* 13.61) relates that Teucer fortified Tarsus in Cilicia. Such very late texts need to be treated with extreme caution but generally speaking the traditions are worthy of careful sifting and analysis, to understand the development of the legend involved, by textual criticism and using the earliest available texts (preferably pre-Hellenistic) – as I hope to have done regarding the stories of Teucer's wanderings.

The travels of another legendary figure, Mopsus and his half-brother and fellow seer Amphilochus, have long been invoked in support of the standard 'Sea Peoples' model (see e.g. Wainwright 1961, 79–81; Redford 2018, 128–129). The stories of Mopsus are particularly hard to analyse and show signs of the multi-layering of different periods (see Astour 1965, 61–66 for an analysis). While some (late sources) have Mopsus fighting at Troy with Agamemnon, and subsequently travelling to Colophon in Ionia and Cilicia, others make him one of the Argonauts a few generations earlier. A Lydian tradition calling him Mopsus or Moxos says he arrived at Ashkelon, but again they place this earlier than the tales in the Trojan War cycle. An adventurer called Mukšaš is known from the time of Tudhaliya II (c. 1400 BC conventional chronology), while the bilingual inscription at Karetepe confirms that his name is Anatolian and not Greek.

Many of these traditions are regularly employed as evidence in the traditional treatments of the 'Sea Peoples', to imagine sweeping migrations of populations from the Aegean across Anatolia, Cilicia, Syria and into Canaan. In interpreting the stories we need to first understand what they actually say and not what we wish or imagine them to say. Again, as stressed earlier, the Greek traditions of the 'Returns' of the warriors after the Trojan War (or rather *non*-Returns) do not describe a mass migration from the Aegean into the Near East. As an analogous case, one might cite the misleading ideas that Martin Bernal promoted in his *Black Pharaohs* series of books (1987, 1991, 2006). Bernal took the stories of pre-Trojan War immigrants such as Danaus, Cadmus and Pelops as evidence that Greece had been colonised by Egyptians, Semites and Anatolians. But the stories say no such thing. Rather they relate how displaced aristocrats from the Near East moved to Greece where they were accepted as kings – as neutral outsiders who could arbitrate dynastic squabbles and

[40] One might also speculate that the Teukroi took their name from the Phoenician Tjekker at some point during the LBA.

land disputes, but who also arrived with sufficient status, wealth and sometimes cult statues that assured their kudos, sometimes reinforced by marriage with a local princess for further legitimation. The ideas that a 'Cadmus' arrived from Tyre and a 'Pelops' from Arzawa, and that they ended up becoming kings of Thebes and Elis do not seem fanciful in terms of the interconnected world of LBA diplomatic relations.[41] The Bronze Age élites were intimately connected in terms of dynastic marriage – attested between Hatti and Egypt, Hatti and Babylonia, Mitanni and Egypt, and most likely Hatti and Ahhiyawa as well. It is accepted that Mitannian blood ran in the Amarna royal family. Zannanza, a son of Suppiluliuma I of Hatti nearly became king of Egypt, at the request of Tutankhamun's widow (Bryce 1990). But had Zannanza not been murdered on the way and perhaps briefly become a foreign pharaoh, this would not allow us to think in terms of a Hittite 'invasion' of Egypt. By the same token, we should not see the stories of Danaus, Pelops and Cadmus as reflecting 'invasions' or 'colonisation' which Bernal claims is the 'ancient model'. It manifestly was not.

The traditions that Bernal appeals to relate to the very early LBA (or late MBA) and concern journeys in the opposite direction to those of the post-Trojan War wanderers. There we are dealing with displaced Aegean aristocrats and their war-bands wandering eastwards and I see no reason why the major players (kings) from Ahhiyawa (Mycenaean Greece) may not have been known individually (by name and repute) in the ancient Near Eastern world. After all, some of them could claim an alleged Levantine or Anatolian ancestry. Yet it is unlikely that they would have received much welcome in areas controlled by the Hittites. If the Trojan War tradition has any value, combined forces of Mycenaean kings had destroyed and sacked Wilusa (Ilios), which we know to have been a longstanding ally of Hatti. Matching this, Homer (*Odyssey* 11.520–521) relates how troops of the 'Ketoi' (Κήτειοι) arrived to support the Trojans. The Ketoi were clearly Hittites to judge not only from their gentilic but also from the thinly hellenised names of their leader Eurypylos and his father Telephos (= Hittite equivalents Urballa and Telepinus). The Hittites and Myceneans were clearly at odds near the end of the LBA. By contrast, the traditions talk of a warm welcome for displaced Mycenaean aristocracy in Phoenicia: Menelaus was received and given rich gifts (by Hiram/'Phaedimus'); and Teucer was welcomed, then employed as an ally, by 'Belus' in his subjugation of Cyprus.

[41] As Hall (1909, 20) observed long ago: 'We need not believe in the actual existence of "Pelops" any more than in the actual existence of "Minos," but it is by no means impossible that, as the kings of Khatti certainly reached the Aegean, one of them, or some sub-king or general, reached Greece and founded a dynasty there.'

Even were we to augment the Aegean component in the 'Sea Peoples' story (e.g. by awkwardly making the Tjekker Teucroi), the centrality of Hiram's role would remain. He had the motivation for organising an attack on both the Hittites and Egypt at a time when both were suffering from internal difficulties, while the death of his ally Solomon would have led to the collapse of the tripartite alliance and trading arrangements between a (temporarily) united Israel, Egypt and Hatti. The vassals of Hatti in Syria would have been a prime target for Hiram's collected forces – to destroy Tyre's main commercial rival Ugarit and the last vestiges of Hittite control over Amurru and subdue some parts of Cyprus for its vital copper resources. As for the naval attack by the 'Sea Peoples' on Egypt, this was an attempt to penetrate the Nile mouths and raid the gold rich centres such as Pi-Ramesses. Ramesses III represented his repulse of this raid as a great victory, and his records show (and depict) how in his Year 8 he then moved into Djahi to take vengeance on the aggressors; and then, in the *CoD* model, to move northwards to support the cause of Jeroboam's independence from Solomon's politically hopeless successor in Judah, Rehoboam.

Concluding Remarks

While satisfying the major concerns of a 'minimalist' approach to the 'Sea Peoples' problem which John Bimson and the present writer have pursued for many years (i.e. no mass migrations of Sicilians, 'Pelasgians', Danaoi and the like into the Levant at this period), the speculative scenario argued above comes some way to a compromise with the more traditional views of the 'Sea Peoples'. Stripped down to the basics, the main elements are the necessary importance of Phoenicia and Cyprus in the events described by Ramesses III and the possibility that Hiram of Tyre played a key role in organising the confederacy that fought first the Hittites and then Egypt. The idea that Hiram, with territorial ambitions in both Cyprus and the Levantine mainland, was a magnet for mercenaries and combined his forces not only with the Philistines but roving Mycenaean warlords in search of fresh pastures (particularly Cyprus) and booty, is an attractive one.

There can be no 'proof' of such matters at present. Our sources are simply not adequate. Had they been, there would have been no 'Sea Peoples' controversy in the first place. All we have are a number of disparate clues which are little squares on a Rubik's cube, one which I have been turning endlessly for some forty years. The *CoD* chronology allows a number of sides on the cube to match, with the novel idea that it was Hiram who issued the 'proclamation' that brought together the 'Sea Peoples' confederacy. Or to coin a new term he could be considered as the 'king of the Sea Peoples'. A point stressed by Bikai (1992) is that after all the apparent

confusion in the Eastern Mediterranean, it was the Phoenicians, who came out on top. As well as extending their influence over Cyprus in the tenth century BC, in the ninth-eighth we find them in Sardinia (the land associated with the Shardana), North Africa, Italy and beyond. Many commentators, beginning with Bikai, have noted the transference of political, regional and economic power from Ugarit to Sidon and Tyre. One of the current excavators of Tyre (Núñez 2017, 277) wrote:

> In any instance, what fell at the end of the Late Bronze Age was the particular diplomatic and economic system that characterized that period, but probably not the essence, which continued to exist later, obviously on a smaller scale, thanks to a certain number of politic entities that survived the crisis. Some of those entities are the Phoenician cities, especially Sidon in the first place and later Tyre, which knew how to profit from the situation by taking over the commercial role that other sites like Ugarit or Byblos had before it.

Why, one has to ask, did it take so long for the 'essence' of Canaanite coastal civilization to reflourish?; or the Sidonians and Tyrians so long to take over the commercial advantages opened by the destruction of Ugarit? Were there really over two centuries or more of blank history in Phoenicia, or should this particular 'dark age' be shortened, lowering the date of the fall of Ugarit from *c.* 1180 BC to *c.* 940 BC?

Afterword

I welcome criticism of the model I have sketched out here, particularly from Egyptologists – providing they can first clear their heads from adherence to the standard model of mass migrations from the Aegean, or indeed the conventional chronology of Egypt.

I do not need to ask the opinion of my old sparring partner Kenneth Kitchen as he has effectively already published it. Without naming the culprits,[42] Kitchen (2012, 15) lashed out against the possibility of the 'Sea Peoples' being of basically Levantine origin, in a tirade characteristic of his colourful style:

> Ramesses III speaks of 'the foreign lands that came from their land in the isles amidst the Sea.' The suggestion, occasionally made, that these peoples had been native to Canaan from of old is nonsense, contradicting both the clear statement of these firsthand texts and the evidence of these peoples' material culture once they did settle in southwest Canaan under Ramesses III and onward. Such a suggestion owes everything to the sociological/anthropological idiot dogma that nobody in antiquity ever emigrated anywhere (especially in any quantity), in the teeth of abundant evidence to the contrary at all periods in recorded human history. It owes nothing to the facts of the case.

For the archaeological record in this case see above – Kitchen seemed to have missed recent developments regarding Philistine Monochrome ware. It has long been known that the closest parallels to Monochrome come from Cyprus, notably the LHIIIC 'Middle' from Sinda, Enkomi, Maa-Paleokastro and Kition (Dothan and Zukerman 2004, 44), showing that the influence for its manufacture came from nearby Cyprus and not the Aegean (see also Middleton, above). And the only 'firsthand text' that Kitchen cites in support of his extreme opinion does not come from the Year 8 inscription itself, as one might imagine, but from a different context, that of the famous sea-battle (*RITANC* 33.4; Edgerton and Wilson 1936, 42, Pl. 42, l. 3). It occurs in the caption to the scene where the Pharaoh is shown standing near the Migdol (a fortress in the Delta) while his officials present to him captives from the sea battle depicted in the previous scene (*MH I*, Pl. 41).

The phrase cited by Kitchen (his translation) is 'the foreign countries that came from their land, from their isles in the middle of the Great Green (sea)'.[43] The focus in this context may have been less on the Philistines, who were never a sea-going people, and perhaps more on the *Sherden*-type who crew three out of five attacking ships (see detail in Emanuel 2013, 21, Fig. 5). Here Ramesses III may have partly been trying to emulate his role model Ramesses II, who waxed lyrical on his Tanis II Stela about his defeat and capture of Shardana from the midst of the sea (see Emanuel 2013, 15). Otherwise one cannot stress enough the importance of the Levantine islands associated with the Phoenicians, including Tyre, Sidon, Arvad and Cyprus.

As to Kitchen's sharp remarks on the 'idiot dogma that nobody in antiquity ever emigrated anywhere', I have some sympathy in that when the 'new archaeology' was in vogue in the 1980s discussion of things like hyper-diffusionism (such as the global spread of a Sun-cult from Egypt) and invasions became somewhat taboo, as they smacked of the extreme ideas promulgated in the middle of the last century regarding 'master races'. To quote myself (James 1995b, 49):

[42] Kitchen (1978) wrote a highly critical review of Nibbi (1975) and a scathing one (1991) of *Centuries of Darkness*, including the classic line: '... this work is a manifest failure and should be consigned to the same oblivion as Velikovsky.' Otherwise he may have been thinking of Robert Drews.

[43] The word for sea here is *w3ḏwr*. Initiated by Nibbi in the 1970s, there has been much debate over whether the Egyptians actually had a word for sea, but despite her efforts and those of Vandersleyen (2008) to show that *w3ḏwr* ('Great Green') meant the Delta and *ym* the Wadi Tumilat, it is clear that could both be used for the Red Sea and the Mediterranean. The reason for the duplication in nomenclature is that while *w3ḏwr* was the original Egyptian term, *ym* is a Semitic loan word.

> This reaction against diffusion was necessary. It led, however, to a trend to reject the significance of invasions generally throughout the archaeological record. Though perhaps 'politically correct', this line of reasoning is not correct politically. If we now find the concept of master races in the past rather distasteful, it does not follow that ancient people had a similar understanding. Julius Caesar and Genghis Khan would laugh in the face of anyone who thought so.

Drews (2017) has said much the same, at length, and has made a convincing case for the role of militarism in the spread of the Indo-European language into Greece and elsewhere by an aristocratic charioteer class from the steppes of the Ukraine. But with regard to ancient invasions, there is no 'one size fits all' answer to different situations, which need to be evaluated on the available and pertinent evidence, despite one's prejudices. Kitchen prefers the traditional mass invasion from the Aegean model for the 'Sea Peoples', which he is entitled to do, but the point remains that the 'firsthand' textual evidence he cites in its support fails to do so.

Abbreviations and Ancient Sources

ARE	Breasted 1906
BICANE 3	James and van der Veen (eds) 2015.
MH I	Epigraphic Survey 1930
MH II	Epigraphic Survey 1932
MH VIII E	Epigraphic Survey 1970
RIMA 1	Grayson 1987
RIMA 2	Grayson 1997
RITANC	Kitchen 2008

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‘Redating the Byblos Inscriptions’: Ultra-Low Chronologies and the Dating of Early 1st Millennium BCE Levantine Material Culture and History[1]

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In a review article I recently published in the Journal of the American Oriental Society *(Wallenfels 2019) focusing on P. James and P. van der Veen’s edited volume,* Solomon and Shishak *(2015a), I remarked that although there was an image on that volume’s front cover of King Aḥirām of Byblos enthroned, there was no reference to Aḥirām or to his relief-carved and inscribed sarcophagus to be found within the volume. Since the dating of Aḥirām is, in fact, intimately connected to the chronological issues discussed at BICANE 3, I briefly summarized in an Appendix there some of the earlier arguments as well as more recent developments in the on-going debate surrounding the dating of Aḥirām. Here, in BICANE 4, I re-examine, now in the full light of the Centuries of Darkness (= CoD; James et al. 1991) ultra-low chronology, the soundness of situating the reliefs and inscription on the Aḥirām sarcophagus in the mid-9th century BCE. At the same time, I make the more general case for the dating of the full corpus of the so-called Old Byblian linear Phoenician inscriptions to c. the mid-9th through the middle of the third quarter of the 8th centuries BCE.*

Introduction

The ruins of Byblos, ancient Gubla, are located in modern-day Jbeil, a small Lebanese seaport situated on the Mediterranean coast at the foot of the Lebanon mountains some 40km north of Beirut. The first controlled excavations there began in 1921 under the direction of P. Montet (1928, 1929). A landslide on the site the next year exposed the royal necropolis which proved to have a total of nine tomb-chambers. Two groups of Tombs, including I and II and V, VIII and IX, were connected to each other underground by robbers’ tunnels. All, with the possible exception of Tomb III, appear to have been looted in antiquity and all suffered from significant intrusions of water, mud and gravel. The interpretation of the dating of the royal necropolis is not directly affected by the difficulties created by Montet’s now-discredited excavation method,[2] however, the tombs and their recovered contents do present their own problems, some of which have direct bearing on the larger chronological issues in question here.

Tomb V and Aḥirām’s Sarcophagus

Tomb V was half-filled with mud when it was discovered (Montet 1928, 217). Uniquely, it contained three sarcophagi, two plain (V_1, V_3), one richly decorated (V_2). None was likely in its original position within the tomb chamber as a result of local tectonic activity which is ‘capable of generating earthquakes of magnitude greater than 7 with a return period of 1000 years’ (Makhoul *et al.* 2016, 15). Sarcophagus V_2 (Beirut National Museum 2086; Montet 1928, pls 128–141) was covered on all four sides and on its lid with painted narrative relief carvings. The front panel (*Figure 5*) depicts a king seated on a sphinx-throne before an offering table and six attendants, while the remaining sides depict a parade of female and male mourners; the

[1] The following remarks were first presented at the *Seminar für Biblische Archäologie bei Schwäbisch Gmünd* in September of 2019 at the kind invitation of that meeting’s organizer, P. van der Veen. All conventional dates not directly linked to the Julian calendar are marked herein with an asterisk (*c.* *1200 BCE); all *CoD* chronology-based dates are marked with an exclamation point (*c.* 950! BCE). Phoenician inscriptions are identified herein by their sigla in Donner and Röllig (1971 = *KAI*).

[2] The French excavators, rather than employing the now-standard method of selectively excavating downward from the surface within a rigidly defined grid system – a method developed earlier by German archaeologists – attempted instead a radical procedure in which they literally shaved off successive 20cm-deep layers across the site, irrespective of the local topography, believing that they could reconstruct the full stratigraphy of the site afterwards. Quite simply put, they could not. Nor has anyone else since, despite substantial efforts (e.g. Kilani 2020), been able to satisfactorily “re-contextualize” all of the recovered remains.

lid shows two men facing each other. Pairs of antithetical lions lie in side view beneath the scenes front and back forming pairs of protomes at either end, while on the lid a third pair, these seen from above, their heads also forming protomes, lie between the two men. An incised linear alphabetic Phoenician inscription (*KAI* 1) commencing on the upper edge of the short south side of the sarcophagus body and continuing on the bevelled lower front edge of the lid identifies the deceased as Aḥirām, king of Byblos, who was laid to rest by his son whose name has been restored as either [Itt]⌜ō⌝-Baʿal or [Pi/u]⌜lsi⌝-Baʿal (Lehmann 2015). The robber's tunnel at the south end of the tomb chamber leads to Tomb IX.

Other Royal Byblian Linear Alphabetic Inscriptions

Aḥirām's sarcophagus inscription and the accompanying tomb-shaft graffito (*KAI* 2), however, were not the first such royal Phoenician linear alphabetic inscriptions known from Byblos. In 1894, a fragment of an Egyptian throne base was excavated that identified in hieroglyphics the name of the Libyan founder of the 22nd Dynasty, Shoshenq I. A linear Phoenician dedicatory inscription (*KAI* 5) identifying one Abibaʿal, king of Byblos, patronym not preserved, had been incised on one side of the throne base surrounding on three sides the twin cartouches naming the Egyptian king (Dussaud 1924, 145–147, Pl. 42). The Phoenician inscription identifies the statue as a gift from Abibaʿal to the city's patron deity, Baʿalat-Gubla, the 'Lady of Byblos', and likely originally stood in a chapel in her temple.

The following year, 1895, saw the publication of another fragmentary Egyptian royal statue, then in a private collection. The incised hieroglyphic inscription identifies the second king of the 22nd Dynasty, Osorkon I, the son and successor of Shoshenq I (Dussaud 1925). The accompanying incised linear Phoenician dedicatory inscription (*KAI* 6) names one Elibaʿal, son of Yeḥimilk, as kings of Byblos. This statue too was donated to Baʿalat-Gubla; several additional fragments of it, none of stratigraphic value, were later excavated in her temple in 1925 (Montet 1929, 53–54) and again in 1939 (Dunand 1939, 17–18).

Two early royal linear Byblian building inscriptions also came to light in the course of the French excavations. An incised limestone building block, found in 1929, was originally used in the restoration of the city's temples carried out by one King Yeḥimilk (*KAI* 4). No patronym is given, but he is likely the father of the preceding Elibaʿal.[3] Another incised limestone block from a wall (*KAI* 7), found in 1935, names one King Šipiṭbaʿal, whose patronym identifies him as the son of the foregoing King Elibaʿal son of King Yeḥimilk. Together, all of these inscriptions are conveniently termed 'Old Byblian', to distinguish them from the philologically and palaeographically distinct inscriptions from Byblos from the latter half of the 1st millennium BCE (e.g. *KAI* 9–12) and from the contemporary so-called Standard Phoenician dialect (Wallenfels 1983, 101–111).

Dating the Inscriptions

The dating of these objects and their Old Byblian royal inscriptions did not initially cause much controversy. The Aḥirām sarcophagus and tomb-shaft inscriptions were immediately dated to the conventional *13th century BCE on the basis of inscribed Ramesside and contemporary Aegean ceramic fragments found while clearing the tomb floor, while the re-inscribed 22nd Dynasty statues were assumed to be contemporary gifts by the named pharaohs to the respective Byblian kings, thus products of the later 10th and early 9th centuries BCE in the conventional chronology. Given the overall similarities in the scripts of the remaining Old Byblian royal inscriptions and two additional palaeographically similar non-royal inscriptions (*KAI* 3, 8), all were seen as typologically intermediate between the earlier so-called Proto-Sinaitic linear inscriptions from Serabit el-Khadim (cf. Gardiner 1916), dated conventionally to the early 2nd millennium BCE, and the classical lapidary letter forms of, e.g. the historically dated mid-9th century BCE Mesha inscription from Moab (*KAI* 181). But as additional examples of the Byblos linear alphabetic script appeared, the specific dates of, even the relative order of these inscriptions, initially proved difficult to establish. Finally, in 1947, W. F. Albright, recognizing the close similarities in the scripts of all of these inscriptions, assigned the Aḥirām inscriptions to *c.* *1000 BCE and re-ordered and redistributed the remaining Old Byblian inscriptions across the balance of the conventional 10th century BCE – the unspoken underlying assumption here, of course, being the identification of Shoshenq I with the biblical Egyptian king Shishak who raided Judah, besieging Jerusalem, in year 5 Rehoboam, *c.* 926 BCE (Champollion 1868, 81). Albright's rearrangement became the basis for a new conventional chronology for all subsequent discussions of West Semitic palaeography.[4]

The first real challenge to Albright's dating did not come until 1973, and then not from a philologist, but rather an art-historian, namely E. Porada, perhaps the

[3] The inscription is a palimpsest, written over an underlying so-called pseudo-hieroglyphic inscription (Martin 1961, 63–68; cf. Martin 1962a, 1962b); for the redating of these inscriptions to *c.* 900–*c.* 830 BCE, see now Sass (2019).

[4] Albright's greatest proponent was the late F. M. Cross, Jr., who for decades to come painstakingly integrated older inscriptions as well as new into Albright's framework (e.g. Cross 1967, 1979, 1980).

greatest scholar of her generation in the field. In a series of tight arguments examining both the iconographic motifs depicted in the Aḥirām tomb reliefs, and, more importantly, the style in which those reliefs were carved, Porada – although acknowledging as at least possible Albright's dating for the inscription *per se* to the 10th century BCE – nonetheless went on to argue for a date for the figured reliefs in the latter half of the 9th or following 8th centuries BCE. Then, ten years later, under the direction of Professor Porada and my then-M.A. advisor, the philologist Professor J. Huehnergard, I argued in my 1982 Columbia University Master's thesis from the points of view of philology and palaeography in support of Porada's later date for the Aḥirām reliefs as well as the inscriptions, and by extension, for all of the Old Byblian inscriptions (see Wallenfels 1983). A fixed anchor point for the royal Old Byblian inscriptions, building on a previous proposal by B. Mazar (Maisler 1946, 178), was provided by the identification of the Šipiṭbaʿal in the inscriptions (*KAI* 7) with the later eighth-century BCE Byblian king of the same name (Šipiṭbaʿal 'II'[5]) identified in the annals of the Assyrian king Tiglath-pileser III for 738 BCE (Tadmor and Yamada 2011, 46 [14:11]). This in turn suggested that all of these inscriptions were products of not the tenth, but rather of the later 9th and 8th centuries BCE. Although at the time I raised serious doubts about the identification of Shoshenq I with the later tenth-century BCE biblical Egyptian king Shishak, the lower dates I proposed for the corpus did not require that the Libyan Egyptian and Byblian kings be contemporaries. Rather, the dates of the Egyptian kings, regardless of what they might actually turn out to be, merely establish the *termini post quem* for the floruit of Abibaʿal and Elibaʿal – which, interpolating from the Assyrian datum, is most likely in the earlier 8th century BCE. Nonetheless, some art-historians continue to date the Aḥirām reliefs, if not the inscriptions, to the conventional *13th century BCE (e.g. Aruz 2014, 9–10; cf. Lehmann 2020, 70). G. E. Markoe (1990, 21 and n. 15) on the other hand, although he appears to have misunderstood Porada as having assigned 'a tenth-century date for the sarcophagus as persuasive', he nonetheless went on to add two further specific iconographic parallels on two bronze bowls, one Cypriote (Markoe 1985, 171–172, no. CY3), the other Athenian (Markoe 1985, 203, no. G1), both well-dated to the last half of the 9th and 8th centuries BCE, thus in fact confirming the lower dates Porada actually intended.

[5] Cf. the sarcophagus inscription (*KAI* 9) identifying an unnamed son of a king of Byblos, Šipiṭbaʿal 'III', dated palaeographically to the first half of the 5th century BCE (*c.* 500 BCE: Donner and Röllig 1973, 10; 'first half of the fifth century BCE': Peckham 1968, 53). In light of the foregoing identification of Šipiṭbaʿal I = II, this king should now be identified as Šipiṭbaʿal II[I].

Dating the Sarcophagus Reliefs

Porada's arguments (1973) may be summarized in part as follows: the sarcophagus lid, not carved as well as the body reliefs, depicts two men facing each other separated by a pair of lions executed in a distinctly Neo-Hittite-style. The men, both bearded, wear ankle-length fringed garments of a type unknown in the late Bronze Age and are assumed to be Aḥirām at the right and his son, at left. Other examples of a royal father and son depicted together include an early eighth-century BCE wall relief showing king Yariri of Carchemish leading his eldest son Kamani into a temple (Akurgal 1962, no. 121), and eighth-and-seventh-century BCE depictions of the Neo-Assyrian kings Tiglath-pileser III and his son Ululaya, the future Shalmaneser V (Radner 2012), and Esarhaddon and the crown princes Šamaš-šum-ukin and Assurbanipal (Pritchard 1969, nos. 447–449).

The reliefs on the front side of the Aḥirām sarcophagus depicting a funerary banquet for the deceased king who holds forth a drinking cup in his right hand have occasioned much comment (*Figure 5*). Two Canaanite royal banqueting scenes featuring drinking found among the carved ivories of wide-ranging origins recovered in the treasury in Megiddo Level VIIA are often offered as comparanda (Loud 1939, nos 2, 160; pls 4, 32. *Figures 2, 3*). Porada insisted however that although all of these scenes are similar in motif, that is, subject matter, to wit a banquet featuring drinking, they differ dramatically from each other in the respective styles of their execution. Loud (1939, no. 2, *Figure 3*), is a large fragment of an incised ivory plaque perhaps intended as an inlay for a voluted footstool, whereas no. 160 (*Figure 2*) is the right half of a long (40.0cm) slender (h. 5.5cm), thin (1.0cm) relief-carved bar with a single dovetail mortise cut-out at either end. With a *terminus ante quem* of *c.* *1130 BCE for the final destruction of the Megiddo treasury in the conventional chronology (Ussishkin 1995), both ivories, not surprisingly, exhibit strong late New Kingdom Egyptian stylistic influences as exemplified in particular by the slender, elegant, youthful appearance of the human figures and of, in the case of no. 2, the sphinx. Together, these are quite unlike the stocky simplified corresponding figures of the tomb relief which much more closely resemble the style first introduced into the wall reliefs in the Northwest Palace of the early ninth-century Neo-Assyrian king Aššurnaṣirpal II at Nimrud, ancient Calah (Akk. Kalḫu, Heb. Kalaḥ (Layard 1853, Pl. 5; see conveniently, Paley 1976, 103, pl. 19b. *Figure 6*); the palace was dedicated with great fanfare in Aššurnaṣirpal's fifth regnal year in 879 BCE (Grayson 1991, A.0.101.30).

The Nimrud-style figures themselves are all-the-more distinctive when compared with the more slender figural representations on the so-called White Obelisk (BM 118807), itself an earlier relief-carved monument of perhaps the same king (Grayson 1996, A.0.101.18;

*Figure 1. Divine (?) banqueting scene: painted vase (facsimile, reversed; h. 21.5cm), South Acropolis Trench, House of the Magician-Priest, Ras Shamra (ancient Ugarit), c. *1300 BCE (RS 24.440; after Schaeffer 1966, 3, Fig. 1).*

*Figure 2. Royal banqueting scene: Late Canaanite-style relief-carved ivory plaque (facsimile, detail; h. 1.7cm). Treasury, level VIIA, Tell el-Mutesellim (ancient Megiddo), c.*12th century BCE (after Loud 1939, Pl. 32, no. 160b, detail).*

*Figure 3. Royal banqueting scene: Late Canaanite-style incised ivory plaque (h. 6cm). Treasury, level VIIA, Tell el-Mutesellim (ancient Megiddo), c. *13th–c.*12th century BCE (after Loud 1939, Pl. 4, no. 2b).*

Figure 4. Royal banqueting scene: North Syrian 'Flame and Frond'-style relief-carved ivory pyxis, gold overlay (facsimile; h. 6.4cm). Well AJ, Northwest Palace, Nimrud (ancient Calah), mid-9th–8th century BCE (IM 79513; after Herrmann and Laidlaw 2009, Pl. 47).

Figure 5. Royal funerary banqueting scene: Aḥirām sarcophagus relief, front side, painted limestone (l. 216cm). Tomb V$_2$, Byblos, c. mid-9th century BCE (Montet 1929, Pl. 130, detail).

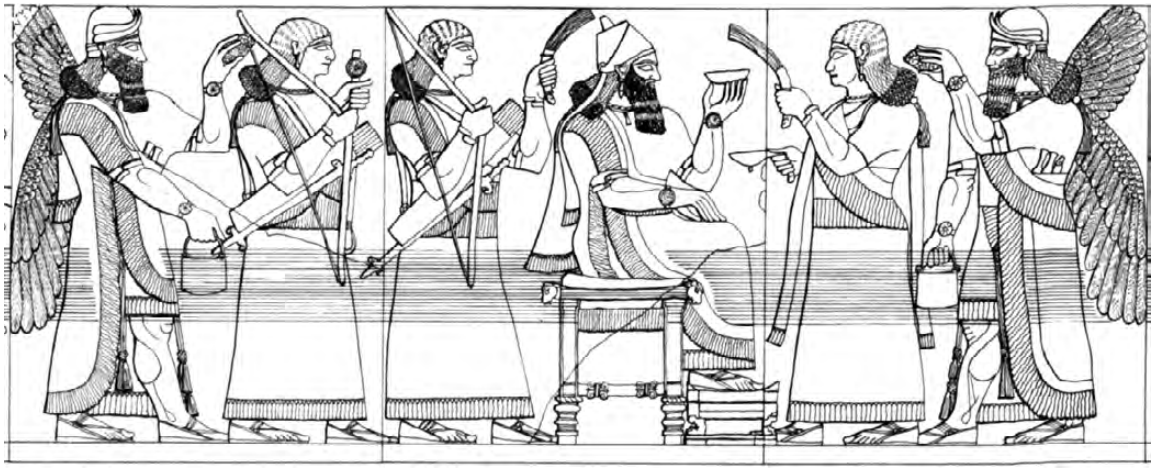

Figure 6. Royal banqueting scene: Neo-Assyrian palace wall-relief (facsimile, reversed), painted and relief-carved alabaster (h. 234cm). Room G, Nimrud, 883–859 BCE (after Meuszyński 1981, Table 8, 1).

contrast Frahm 2009, 117–123; 2017, 165–167; cf. Russel 2017, 471), but executed by artisans in Nineveh in the now-about-to-be-antiquated later Middle Assyrian style (cf. Pittman 1997).[6] Although the origins of the similar stocky simplified style as seen in first-millennium BCE North Syrian ivories is usually attributed to late Imperial Hittite influences as seen in reliefs in the so-called Neo-Hittite states with which Aššurnaṣirpal II must have come into contact during his earliest campaigns to the West, the impact of the stocky style of royal figures observed in Isin II reliefs might also have been of significance. Particular attention may be drawn to the short-sleeved, ankle-length tunic surmounted by a small, fringed shawl wrapped around the hips and secured by a broad waist-belt as worn by Aššurnaṣirpal II in a bull-hunt scene (BM 124533; see conveniently, Paley 1976, 102, Pl. 18b). This particular type of costume is first identified in an entitlement-monument ('*kudurru*') relief of Marduk-nadin-ahhe (*BBSt* no. 7; Houston 1954, 128–134) of the Isin II Dynasty in Babylonia, whose own newly-proportioned body-style, which can only be described as somewhat plump, would be the basis of the style that would remain the standard through the end of the Neo-Babylonian period. Given the secure links spanning many decades between the Isin II and later Middle Assyrian dynasties, the regnal dates of the Isin II rulers in a *CoD*-like chronology would be reduced similarly, thus barely a century would now separate Marduk-nadin-ahhe from Aššurnaṣirpal II.

Perhaps the single most important piece of comparanda for the front side relief of the Aḥirām sarcophagus to appear in more recent years is a carved ivory pyxis[7] just 6.4cm tall (*Figure 4*) recovered from Well AJ in Aššurnaṣirpal II's Northwest Palace in Nimrud. The piece has been identified by G. Herrmann (1989) as an archetypal example of what she has defined as the North Syrian 'Flame and Frond' style-group, firmly datable to the mid-9th through 8th centuries BCE.[8] However, the close similarities between the banqueting scenes on both the pyxis and on the Aḥirām sarcophagus – despite the differences in the hardness of their respective media and an order of magnitude difference in their respective heights – imply a much closer temporal connection between these two objects than dating the sarcophagus to the conventional *13th or *12th or even *10th centuries BCE would reasonably allow (Herrmann 1989, 103). Among the extraordinary similarities noted are the specific type of offering table, a tripod with zoomorphic S-shaped legs and a distinctive vertical prop between the stretcher and the underside of the table's blade, datable to the historical mid-9th through 8th centuries BCE (Gubel 1987, 251–61, Type VIII-d), and a footstool with voluted supports, datable between the 8th and 6th centuries BCE (Gubel 1987, 231–38, Type VII-a). Gubel (1987, 252) did note two earlier examples of the table with zoomorphic legs, the first, one of the Megiddo ivories (Loud 1939, no. 160) mentioned above (*Figure 2*), the other a painted cartoon on a pitcher from Ugarit (RS 24.440; *Figure 1*); the conventional *c.* *13th–*12th century BCE dates for these two objects may be reduced to *c.* 1050!–950! BCE making these the immediate forebears of the so-called Iron Age Phoenician examples[9]; note the table in the Megiddo relief lacks the distinctive vertical prop beneath the table top and the pads beneath the feet; on the other hand the table legs are more angular, emphasizing especially the ankle joint.[10]

Dating Tomb V Detritus

But what about the inscribed Ramesside alabaster vase and Late Cypriot II milk-bowl fragments noted earlier found in the tomb chamber, or the broken relief-carved Late Cypriot IIIA ivory plaque described as having been found at the 'bottom of the tomb shaft'? Assuming that none of these apparently earlier pieces recovered

[6] Assigning the White Obelisk to the reign of Aššurnaṣirpal I, whose revised regnal dates are *c.* 929!–*c.* 911! BCE, places him barely a half-century earlier than his later namesake (883–859 BCE) and does not materially affect the argument here. See now Edmonds 2023, 252, n. 22.

[7] The pyxis IM 79513 was first published in 1987 (Herrmann and Laidlaw 2009, 184–186, no. 234).

[8] For the stylistic and chronological distinctions between North Syrian and Phoenician ivories of the 9th–mid-8th and mid-8th–7th centuries BCE, respectively, see Winter (1976); cf. Herrmann and Laidlaw (2009, 2012–2013).

[9] Recall H. Kantor's (1956, 174) remark that 'despite the passage of three centuries without any known links' Iron Age ivories inexplicably retain features 'which can be clearly traced in the ivories of the 13th century' BCE. Cf. the assemblage of carved ivories from Iron Age I–early Iron Age IIA Tel-Miqne-Ekron in Philistia, some described as showing *c.* *12th–*c.* *11th century BCE Egyptian influences and others, possibly heirlooms, *c.* *14th–*c.* *13th century BCE Canaanite traditions (Ben-Shlomo and Dothan 2006). However, in a *CoD*-like chronology the historical Iron I period in Canaan is reduced to an approximately forty-year-long period, *c.* 926–884 BCE, thus there is no 'gap', certainly not one some 300 years in length, to fill; these ivories thus complement, but are non-the-less tangential, to the central discussion.

[10] Compare further the comparably dated cylinder seal from Ugarit (RS 24.106; Amiet 1992, no. 213) displaying a similar table with angular zoomorphic legs and the vertical prop; however, the table's proportions – significantly narrower and taller – have been seriously compromised to accommodate the available space between the seated celebrant and two attendants (Paradiso 2011, 83); see additionally note 20, below. Kantor's remarks noted in the previous note are equally applicable here as is the solution to the dilemmas posed otherwise by the conventional dating. Yet another example of a Type VIII-d table appears on a recently published Neo-Hittite funerary stele in the Kahramanmaraş Museum (Inv. no. 2011-1), excavated, but without context, believed to be from the Maraş Group, dated to *c.* 9th–8th c. BCE (Çifçi and Başpinar 2023).

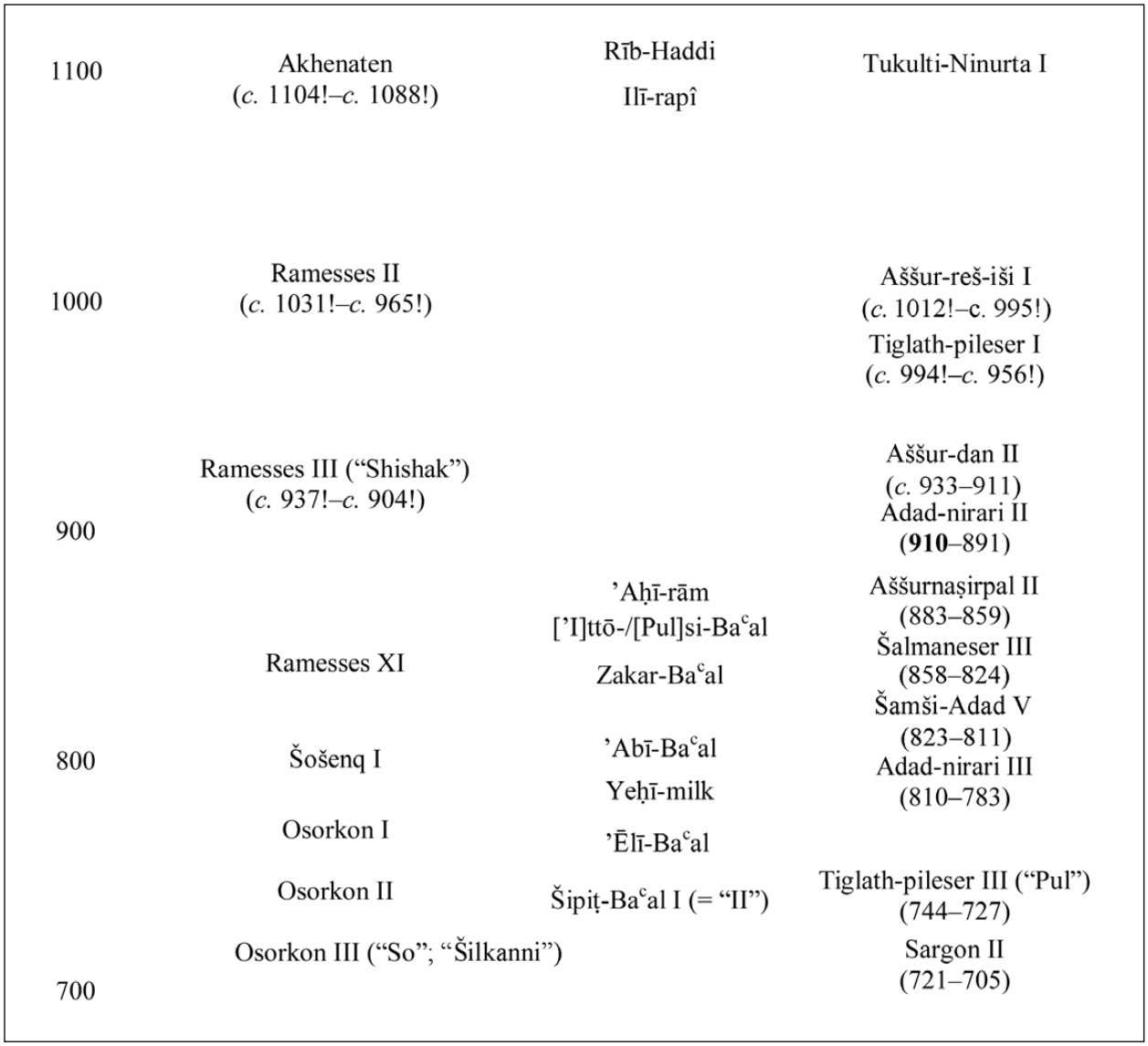

Figure 7: A CoD-like chronology of the kings of Byblos and their Egyptian and Assyrian contemporaries.

in Tomb V were heirlooms when placed in or around the sarcophagus at the time of Aḥirām's burial, it must be recalled that Tomb V contained two other empty undecorated sarcophagi when found, so it is also quite conceivable, in fact now likely, that any or all of these Late Bronze and Iron Age I objects may be detritus left behind, originating in or around either of the plain sarcophagi, when the tomb chamber was swept out in preparation for Aḥirām's interment. Additionally, one or more of these pieces may be intrusive to the tomb having originated in either of the other tombs, IX and VIII, connected to Tomb V by robbers' tunnels, and later transported into V either as a result of water percolating for centuries if not millennia through the necropolis – recall that the tomb chamber was described as half-full of mud when first opened – or through human or animal activity. In any case, regardless of how these earlier objects came to be in Tomb V, their association with the Aḥirām sarcophagus *per se* is merely fortuitous and thus they have no bearing on its date for which all internal evidence clearly supports a date no earlier than the historical mid-9th century BCE.

Dating the Tomb Shaft

One last point for discussion is the access shaft to Tomb V, specifically the arrangement of the fill in that shaft (4.0×4.2m). The upper half of the shaft, above the four square holes in both the west and east walls which appear to have supported an intermediate wooden floor, was filled with pottery sherds, predominantly a type of Cypro-Phoenician pottery known as 'Black-on-Red', or B-o-R, ware, which is otherwise firmly dated to the historical mid-9th through mid-8th centuries BCE (Kleiman *et al.* 2019; cf. Schreiber 2001; 2003, 169 and n. 51). Below the intermediate floor, however, the shaft-fill was pottery-free. This still suggests to me that

the last time Tomb V was accessed via its shaft, it was refilled with ritually 'pure' sherd-free soil to the level of the intermediate floor. Then, following the laying of the intermediate wooden floor and the incising of the brief three-line graffito (*KAI* 2) on the shaft's south wall in the same script as the sarcophagus inscription, the remainder of the shaft was filled with local soil which abounded, in particular, with contemporary mid-9th to mid-8th century BCE pottery fragments.

Palaeography and Dating

Although the palaeographical arguments I raised in 1983 are still sound, the number of relevant West Semitic inscriptions that have been recovered from archaeologically controlled contexts has grown dramatically (see e.g. Finkelstein and Sass 2013). At the same time great advances in our understanding of the stages of evolution of the linear West Semitic alphabet have also been achieved. The publication of G. Hamilton's 2006 monograph, *The Origins of the West Semitic Alphabet in Egyptian Scripts,* is among the most significant.[11] The earliest linear alphabetic script, termed Proto-Canaanite, is now understood to have developed in Egypt according to the so-called acrophonic principle wherein a selection of some 30 or so Middle Kingdom Egyptian hieroglyphic and hieratic signs were selected for their Canaanite vocalization wherein the initial sound of the Canaanite word for the object depicted in the hieroglyph, always a consonant, is now represented by that sign. Only during the latter half of the historical 9th century BCE did the various and distinctive 'national' monumental twenty-two-letter Canaanite and Aramaic scripts begin to evolve across the Levant.

In 2005, the Israeli palaeographer B. Sass, in his monograph, *The Alphabet at the Turn of the Millennium*, fully acknowledged my priority (p. 16) for substantiating the lowering of the dates of the Old Byblian royal inscriptions. Beginning with the recognition of the close relationship between the Aḥirām sarcophagus reliefs and the Nimrud ivory pyxis relief, Sass went on to argue for the antiquity of the Arabian and Greek and Phrygian alphabets (cf. Rollston 2008). Although he made substantial headway, Sass's adherence to the conventional chronology at large has blunted his efforts. Nonetheless in 2013, Sass, publishing in cooperation with I. Finkelstein (Finkelstein and Sass 2013), who had previously echoed the older notion first proposed by K. Kenyon that the Levantine archaeological horizon termed Iron Age IIA was in fact 'Omride', not 'Solomonic' (Finkelstein 1995, 1996; cf. Kenyon 1979, 258–285; Chapman 2015), reassigned and re-dated some 70 different linear inscriptions, convincingly demonstrating that the Old Canaanite-style of script (which Sass and Finkelstein inexplicably continue to term 'Proto-Canaanite') was still in wide use in the early decades of the historical ninth century (Sass and Finkelstein 2016).[12]

The publication in 1991 of P. James *et al.*, *Centuries of Darkness*, in which it was proposed that a total of just under 250 years be removed from the Egyptian Third Intermediate Period and that some 110–125 years be removed from the Middle Assyrian Chronology produced an entirely new framework onto which the full corpus of linear alphabetic scripts prior to the mid-9th century might be recast and reexamined (cf. *Figure 3*, Wallenfels, p. 18, elsewhere in this volume). Perhaps most important of all is the fact that the Iron Age I period, which is some 200 years long in the conventional chronology and some 300 years long in Finkelstein's Low Chronology, is but some 40 years long in a *CoD*-like chronology.[13] Thus, for example, the dates for the reign of Ramesses II, *c.* *1279–*c.* *1213 BCE in the conventional low Egyptian chronology, are now reduced to *c.* 1031!–*c.* 965! BCE, making him a contemporary of the redated late Middle Assyrian kings Aššur-reš-iši I and Tiglath-pileser I, rather than of Shalmaneser I and Tukulti-Ninurta I, as in the conventional chronology.

A Revised History of Early Iron Age Byblos

I would like now to explore the implications of this realignment and redating on the discussion at hand. If the excavations at Byblos have shown little else, through much of its history the orientation of the city's elites towards its greatest trading partner, Egypt, and those elites' appreciation of things Egyptian are quite clear, perhaps overwhelming.[14] But there is certainly

[11] I follow Hamilton's (2006, 6) terminology for the various script phases throughout: 'Proto-Canaanite' for scripts prior to *c.* *1400 BCE and 'Old Canaanite' for scripts between *c.* *1400 BCE and the appearance of the distinctive so-called national scripts of the 9th century BCE.

[12] This conclusion was already reached in the 1981 publication on the ninth-century Tell Fakhiriya Aramaic-Akkadian bilingual inscription (Abou-Assaf *et al.*, 1982), the protests of J. Naveh (1983, 1987) that the script was of the conventional *11th century BCE notwithstanding. Finally, in 2017, Sass once more approached the Old Byblian inscriptions, again accepting my later dating (129, n. 32), but still from the conventional chronological perspective, now saddled with a three-hundred-year-long Iron Age I period.

[13] In the *CoD*-model, the historical Iron Age I period represents the span from the break-up of the United Monarchy under Rehoboam to the rise of the Omrides, not the initial entry of the Israelites into Canaan under Joshua (cf. Bimson 1978; 2015, 99–103; see now James *et al.* 2022).

[14] 'The coastal city of Byblos enjoyed a long and prosperous relationship with Egypt, one that lasted the entire length of pharaonic history. This special connection is reflected in the number and quality of Egyptian objects found at the site. Among them are at least 54 statues or statue fragments; these pieces form the largest percentage (38%) of Egyptian statues

evidence of contacts with Mesopotamia in all historical periods, in the form of cuneiform writing and cylinder seals, although on a far more modest scale. Thus, the obvious question arises here: why is Aḥirām's sarcophagus decorated in a decidedly non-Egyptian, specifically North Syrian reflection of the Nimrud reliefs, especially when at least two of his presumed eventual successors at the very least rededicated contemporary or nearly-contemporary Egyptian royal statuary to the Lady of Byblos?

The previous outline of events (see *Figure 7*), set entirely upon a *CoD*-like foundation and framework, presents a clear answer.[15]

Ramesses II, in the fourth year of his reign, *c.* 1028! BCE, donated a stele[16] to the temple of the Lady of Byblos and left the first of his three carved cliff reliefs at Nahr el-Kalb, some 35km to the south of the city.[17] Just a year later, Ramesses faced the forces of the Hittite king Muwatalli III at Kadesh on the Orontes. Ramesses' defeat, his protestations to the contrary notwithstanding,[18] and his signing of a peace treaty years later in year 21 Ramesses II, or *c.* 1011! BCE, with a new Hittite king, Hatusilli III, appears not to have immediately impeded Egyptian access to Byblos.[19]

Nonetheless, with the effective withdrawal of Egypt from North Syria and a weakening Imperial Hittite state, northern Syria now faced threats from the east, the result of the newly expansionist policies of the late Middle Assyrian kings Aššur-reš-iši I, reigning *c.* 1012!–*c.* 995! BCE, but especially of his son and successor, Tiglath-pileser I, reigning *c.* 994!–*c.* 956! BCE. Tiglath-pileser claimed to have reached the Mediterranean and to have received tribute from three Phoenician cities: Byblos, Sidon and Arvad.[20]

But the seriously deteriorating contemporary climate, identified today as the '3.2 kiloyear (ka) BP Aridification Event',[21] appears to have reached its nadir during the first half of the 10th century. The resulting threat posed by displaced starving Aramaean climate-refugees appears to have forced Tiglath-pileser to withdraw from Syria and tend to far more pressing matters closer to home. The Assyrian King List suggests problems in the line of succession following Tiglath-pileser's death culminating in the overthrew of Tiglath-pileser's sitting grandson, Eriba-Adad II, by another of Tiglath-pileser's sons, Šamši-Adad IV, who had apparently spent the previous 22 years in Babylonia before returning to Assyria *c.* 935! BCE. Although the line of late Middle Assyrian kings first established by Ilī-iḫaddâ following the murder of Tukulti-Ninurta I would continue for several more decades in almost complete obscurity, it was this event which I posited elsewhere in this volume (on p. 65) that brought about the transfer of power to the collateral line now dominated by Aššur-dan II, who would quickly move to retake Aramaean-held territory and lay the groundwork for the restoration of Assyrian control in the west.[22]

In Egypt, the reign of Ramesses III, *c.* 937!–*c.* 904! BCE, is now more-or-less contemporary with that of Aššur-dan II, *c.* 933–911 BCE, and of his son and successor, Adad-nirari II, 910–891 BCE. Whether Ramesses III's foray against the Philistines at Djahi during his eighth regnal year, *c.* 930! BCE, really occurred as far north as the Hatay, as has been suggested by D. Kahn (2015; 2016; cf. Drews 2000), remains to be demonstrated. Nonetheless, an inscribed fragment of a statue of this king, a surface find, was recovered at Byblos (Kilani 2020, 47–48, no. 7), suggesting renewed, if only brief, diplomatic contact with Egypt.

During the early 870s BCE, the Assyrian king Aššurnaṣirpal II, now on his ninth military campaign, led his forces from Calah to the Mediterranean, seizing the Phoenician cities' 'grain bowl' (Cifola 1997–1998)

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from Levantine contexts for any site examined in this study. The sheer number of statues likely represents only a fraction of what was once displayed in Byblos' temples . . .' (Arico 2016, 325).

[15] Cf. Abou-Abdallah (2018); Morris (2005, 343–690).

[16] Montet (1928, 48; 1929, Pl. 34, Figs 2–25); Kitchen (1999, no. 63).

[17] Yr. 4, Middle Stele, Nahr el-Kalb (Kitchen 1999, no. 1).

[18] Yr. 5, Battle of Kadesh (Kitchen, 1999, no. 3).

[19] Yr. 10, South Stele, Nahr el-Kalb (Kitchen 1999, no. 5; also note the undated North Stele, no. 2).

[20] Note the cylinder seal excavated in Tyre (Porada 1978, 80, Fig.1a) depicting a female worshipper standing before a flaming altar and offering table with leonine legs and pads and a horizontal stretcher set in front of a ziggurat upon which a god stands brandishing a lightning fork. This seal, carved in low flat relief with incised linear details, has been closely compared in both motif and style of execution with a seal from Assur (VAR 591), the latter dated as early as the reign of Tukulti-Ninurta I and as late as the 9th century (e.g. Frankfort 1939, 193–195 and Pl. XXXIVf; Moortgat 1940, 139, no. 591; Porada 1978; 1979, 8–10; Ornan 2003). The offering table common to both seals strongly recalls the table in one of the Megiddo ivories, above (*Figure 2*), and should be dated similarly, i.e. *c.* mid-10th! century BCE. Among other comparanda for the Tyre seal, Porada (1978, 78) singled out Esarhaddon's Succession Treaty Seal C, which I have elsewhere dated to the reign of Tiglath-pileser I (Wallenfels 2022), whose revised regnal dates are *c.* 994!–*c.* 956! BCE.

[21] Cf. e.g. Neumann and Parpola (1987); Divon (2008); Kaniewski *et al.* (2015); Finkelstein *et al.* (2017).

[22] 'Still, if Assyria had been absent from Hanigalbat for over a century [vs. 22 years in *CoD*] ... then the notion of its inherent ownership of that territory is difficult to understand. This is not to suggest that a rationale for expansion would not have been exaggerated or even fabricated, but the specific program of repopulation of Assyrian cities undertaken by the early Neo-Assyrian kings *seems to reflect a recent memory of occupation in those cities*. In short, the chronology of Middle Assyrian expansion and decline leaves many questions and inconsistencies that must be addressed' (Szuchman 2007, 31, emphasis added).

along the way and receiving tribute from unnamed kings of Byblos, Sidon and Tyre. His successor was initially less successful. Shalmaneser III was confronted by a coalition of a dozen Levantine rulers, foremost the kings of Damascus, Hama and Israel, who, together with their allies, according to Assyrian records (Grayson 1996, A.0.102.2: ii 89b–102), mustered a total of nearly 4000 chariots, almost 2000 cavalry, 1000 camels and more than 50,000 troops at the Battle of Qarqar in northwest Syria in 853 BCE. Byblos contributed a contingent of 500 troops while an unnamed Egyptian king contributed 1000 troops.[23] Although claiming victory, Shalmaneser had to confront the Damascus-led coalition again in 849, 848 and 845, and was unable to advance further into the southern Levant, until the Israelite king Jehu's bloody rebellion permitted the Assyrian's return. Jehu's humiliating subjugation to Shalmaneser in 841 BCE was memorialized among the reliefs and inscriptions on the Black Obelisk (BM 118885), now in London. That same year, Shalmaneser took tribute from one Baali-manzeri of Tyre (RIMA 3 A.0.102: iii, 10–11), who can be equated with Balezeros II son of Ithobalos I of the Tyrian Annals (*apud* Josephus); Balezeros's short six-year reign, presumably beginning no earlier than 846 and no later than 841 BCE, provides the basis for a secure, if not quite exact, link between the chronologies of Assyria and Segment 1 of the Tyrian King-List, thereby establishing the latter's near-absolute dating (Kokkinos 2013, 46).

The Report of Wenamun is a canonical Egyptian papyrus mentioning yet another king of Byblos, one Tjekerbaʿal (*T'-k'-rw-B-ᶜ-r'*), in Phoenician 'Zakar-Baʿal' or the like.[24] The text is usually considered to have been written at the end of the 20th Dynasty, which together with the conventionally ensuing Third Intermediate Period (TIP), is a rather confused time in Egyptian history.[25] *Wenamun* opens with a chronological notice referring to 'Year 5', usually reckoned to be that of the 'Renaissance Era', which is typically believed to have been introduced by the High Priest of Amon, Herihor, in yr. 19 Ramesses XI (*c.* *1080 BCE; Lichtheim 2003, 90, n. 1; cf. Dodson 2012, 18–24). Assuming the conventional regnal lengths for the remainder of the 20th Dynasty following Ramesses IV, when the reigns of his successors are reduced by the same 248 years proposed earlier, then yr. 5 of the Era of Repeating Births, that is yr. 23 Ramesses XI, is *c.* 828! BCE. A. Thijs (2005, 73, n. 4), however, has argued that in fact the reigns of Ramesses IX, X and XI partially over-lap resulting in the raising of the regnal dates of Ramesses XI by 12–13 years; thus, yr. 19 Ramesses XI, that is yr. 1 of the Era of Repeating Births, is *c.* 845/44! BCE, and Wenamun's journey would now be datable to *c.* 841/40! BCE. Whether the apparent coincidence of the dates of the return of Shalmaneser III to the southern Levant and Wenamon's journey north by sea both in 841 is 'real' or an artifact of Thijs', as well as my own, assumptions and approximations remains to be determined.

But events in Assyria threatened Shalmaneser III's eventual gains – by 826 BCE, a son, Aššur-da'in-apli, had risen in rebellion against his father,[26] leaving the kingdom weakened for many decades to come. Nonetheless, within a few years, another of Shalmaneser's sons, Šamši-Adad V (823–811 BCE), was able to put down the revolt. A cuneiform-inscribed and relief-carved blue stone *lamaštu*-plaque belonging to one Ili-ittiya, eunuch of Šamši-Adad and governor of Ashur, was recovered in Byblos (RIMA 3 A.0.103.2001). And when Šamši-Adad's son Adad-nirari III (810–783 BCE) began campaigning in the West in 796 BCE, he was still able to extract tribute from Israel, Sidon and Tyre (RIMA 3 A.0.104.7: 8–9). Adad-nirari also threatened Damascus, apparently forcing the Damascene king Hazael to withdraw from his own siege of Samaria. If my understanding of 2 Kings 7:6–7 is correct (Wallenfels 2019, 492; cf. James and van der Veen 2015b), at about the same time that Adad-nirari III was approaching from the north, Shoshenq I was marching north from Egypt, following a route through Israel and the trans-Jordan apparently retaking the very land recently seized from Israel by Hazael and his predecessor, Ben-Hadad 'III'. The fragment of a victory stele of Shoshenq I excavated in an unstratified spoil heap at Megiddo is likely the product of exactly this campaign (cf. Chapman 2009). This renewed Egyptian presence in the Levant, especially at Byblos, appears to have lasted until the resurgence of Assyrian power under Tiglath-pileser III beginning in

[23] Given the uncertainties in the chronology of the late Ramesside kings and the contemporary dissolution of the country into competing power centers north and south during the Third Intermediate Period, the identification of the Egyptian king who sent troops to Qarqar remains to be determined (cf. e.g. Dodson 2012, 3–38); see further n. 25, below.

[24] *Papyrus Pushkin* (Moscow) 120 (Breasted 1905; for a recent edition, see Lichtheim 2003). Cf. two linear alphabetic inscribed bronze weapon points (*ḥṣ*), neither with archaeological provenience, each identifying one 'Zakar-Baal / king of Amurru' (Starcky 1982; Deutsch and Heltzer 1994, no. 1); a third point, inscribed 'Zakar-Ba[al] / son of Ben-Ana[t]' (*KAI* 22), also without archaeological provenience, was dated palaeographically by Sass (Sass and Finkelstein 2016, 33–37) to what he termed late Iron Age IIA/1, *c.* mid-9th century BCE. It should be well-noted that of the four dozen such inscribed weapon points known to this writer, just one (*KAI* 20) has a claimed archaeological provenience (Guigues 1926)!

[25] For a general statement of the issues, see James *et al.* (1991, 231–259); for the proposed overlap of the whole of the 21st Dynasty with the 22nd and 23rd Dynasties, see Furlong (2010, 143–193); for additional proposals for redating aspects of the TIP, see further, e.g. Thijs (2005, 82–91; 2015); Morkot and James (2015); contrast Hagens (1996).

[26] Grayson (1996, A.0.103.1: i 39, 52). During the revolt of Aššur-da'in-apli son of Shalmaneser III (826–820 BCE), eponyms for those years are marked *sīḫu* 'revolt'; 27 cities, including Assur and Nineveh, but not Calah, participated; the revolt is not noted in AKL.

the middle of the 8th century – recall the payment of tribute by Šipiṭbaʿal (I = 'II') to Tiglath-pileser in 738 BCE (Grayson 1996, A.0.102.14: 103–104). In this light, the presence of statues of three different Twenty-second-dynasty kings at Byblos may well reflect contemporary royal gifts, although without confirmation, it cannot be discounted that any or all of them were exported from Egypt by looters prior to the reestablishment of a strong central authority in Egypt under the Saite kings of the 26th Dynasty. The specific wording of the added Phoenician inscriptions on two of them does not acknowledge that either was a (repurposed) royal gift – in fact, Abibaʿal claims, depending on the restoration of the relevant verb, either to have had the statue brought from or made to his order in Egypt,[27] while Elibaal states quite clearly 'he made' the statue.[28] A third royal statue fragment, that of Osorkon II, no longer possesses a Phoenician inscription, assuming it ever did. Nonetheless, we may now situate the beginning of Osorkon II's reign prior to 738 BCE, when Byblos was once again a vassal of the Assyrians and unlikely to have been in a position to receive contemporary royal Egyptian gifts for some time to come.

Conclusion

As this brief revised historical reconstruction demonstrates, the recasting of the disparate Byblian, Egyptian and Assyrian data upon an ultra-low *CoD*-like framework founded upon the unequally down-dated Egyptian and Assyrian chronologies described elsewhere herein, produces a significantly tighter interlacing of events, is without conflicts or unexplained gaps, and, more to the point, produces a coherent and internally consistent – and dramatically new – picture of the northern Levant and its two most powerful neighbours in the early Iron Age. In this light, it seems more than likely that Aḥirām's sarcophagus was commissioned by his son Ittobaʿal during the reign of Aššurnaṣirpal II after his dedication of Nimrud or during the reigns of either of his immediate successors, Shalmaneser III and Šamši-Adad V. The presence of the Libyan Egyptian statues at Byblos likely reflects both the nascent ambitions of the new Dynasty and the six decade-long absence of Assyria from the west prior to the resurgence of Assyrian power under Tiglath-pileser III, when Byblos was subject once again to Assyria. The overall picture is, of course, far from complete, with at least as many new questions arising as we have answered. One last observation: it would appear that the conventional picture of the years immediately preceding the earliest certain links to the Julian calendar in the Egyptian (664 BCE), Assyrian (910 BCE) and Babylonian (747 BCE) chronologies are in fact far less well understood than previously imagined.

[27] *KAI* 5:1 [*... z y*]⌜*b*⌝*.ʿbbʿl ... bmṣrm* '... which Abibaal caused to be brought ... from Egypt'; cf. Lemaire (2006, 1700, ll. 1–2) [*... z b*]⌜*r*⌝*.ʿbbʿl ... bmṣrm* '[*Statue qu'a scu*]lptée Abibaal ... en Égypte ...'.

[28] *KAI* 6:1 *mš.z pʿl.ʾlbʿl* '... which Elibaal made'.

Abbreviations

KAI	Donner and Röllig 1971
RIMA 3	Grayson 1996
CoD	James *et al*. 1991

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Chronological Data and Mid-Ninth Century BC Israel

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This article will concentrate on chronological biblical data from the 9th century BC and will combine it with other historical data known from other sources. All sources must be examined critically. They sometimes may nevertheless confirm these chronological data.

Introductory Considerations

Scholars writing histories of ancient Israel are rarely very precise about their dates for specific historical events. For instance, in one of the most prominent German textbooks on the subject we find the following statement:

> [Es] sei … darauf hingewiesen, dass es für die vorhellenistische Zeit kaum Eckdaten gibt, die es erlauben würden, die relative Chronologie eindeutig mit absoluten Zeitangaben zu verbinden. Generell wird man bei den absoluten Daten für Palästina im 1. Jt. v.Chr. mit einem Unsicherheitsfaktor bis zu einzelnen Jahren … zu rechnen haben. (Berlejung 2016, 88) [1]

Another recent monograph (Knauf and Niemann 2021, Appendix 3) simply combines various chronological approaches without even discussing their reliability. The book does not yield any explanation as to why these dates remain uncertain: [2]

Israel		**Judah**	
Jeroboam I	937/922 – 913/901	Rehoboam	937/922 – 921/910
Nadab	913/901 – 912/900	Abijah	921/909 – 916/907
Baasha	912/900 – 889/877	Asa	916/906 – 876/866
Elah	889/877 – 888/876	Jehoshaphat	877/868 – 853/845
Zimri	888/876	Jehoram	853/847 – 845/841
Omri	888/876 – 877/869	Ahaziah	845/840
Ahab	877/869 – 854/850	Athaliah	845/839 – 840/831
Ahaziah	854/850 – 853/849	Joash	842/832 – 803/793
Joram	853/849 – 845/840	Amaziah	805/796 – 800/767
Jehu	845/839 – 822/814	Azariah/Uzziah	792/772 – 747/734
Jehoahaz	821/814 – 805/798	Jotham	759/747 – 744/732
Jehoash	805/798 – 790/782	Ahaz	744/737 – 729/715
Jeroboam II	793/785 – 753/745	Hezekiah	728/715 – 700/686
Zechariah	753/745 – 753/745	Manasseh	698/687 – 644/640
Shallum	752/745	Amon	643/640 – 642/639
Menachem	752/745 – 742/736	Josiah	641/639 – 610/609
Pekahiah	746/736 – 740/735	Jehoahaz	609
Pekah	752/734 – 732/731	Jehoiakim	609/608 – 598/597
Hoshea	732/730 – 724/722	Jehoiachin	598/59
		Zedekiah	598/596 – 587/586

Table 1 . Various approaches combined in one table after Knauf and Niemann 2021.

[1] English translation: 'It should be … pointed out that there are hardly any key dates for the pre-Hellenistic period that would allow the relative chronology to be clearly linked with absolute times. In general, the absolute dates for Palestine in the 1st millennium BC must be reckoned with an uncertainty factor up to some single years.'

[2] The summary is based on Begrich (1929), Albright (1945), Jepsen and Hanhart (1964), Thiele (1983), Hayes and Hooker (1988), Hughes (1990), Galil (1996) and Laato (2015). While the chronological system remains undiscussed in the textbook, some arguments are mentioned in Knauf (2016, 69–88). Cf. also Weingart 2020.

Biblical Kings and Events	Year BC	Neo-Assyrian ruler	Reference[a]	Reference
Ahab of Israel joins an anti-Assyrian coalition at Qarqar	853	Shalmaneser III	106–107	–
Jehu of Israel pays tribute to Assyria	841	Shalmaneser III	112–113	–
Jehoash of Israel pays tribute to Assyria	796	Adad-nirari III	122	–
Menahem of Israel pays tribute to Assyria	738	Tiglath-pileser III	140	2 K 15:19
Ahaz of Judah pays tribute to Assyria	734	Tiglath-pileser III	140	2 K 16:8–10
Dethronement of Pekah of Israel, inauguration of Hoshea of Israel	732		147–149	2 K 15:30; 17:1
Conquest of Samaria	722		150–152	2 K 17:6
Assyrian Campaign against Judah	701	Sennacherib	181–183	2 K 18:13–19:36

Table 3. A correlation between Israelite and Judahite kings on the one hand and Assyria on the other hand.

[a] All references in this table follow Weippert 2010, the nos. being those used by Weippert in his main text.

Some dates proposed in older studies have become obsolete and should no longer be used. For as we know today, based on the Neo-Assyrian annals, king Ahab of Israel was still alive in 853 BC when he joined a coalition of forces against the Assyrians at the Syrian site of Qarqar. It is therefore difficult to appreciate why Knauf and Niemann (2021) still use higher dates.

A general scepticism shared by several biblical scholars towards more specific dates for historical events surely does not improve the overall situation. This markedly negative approach undoubtedly has had a major impact on how historical dates and archaeological evidence are considered as potential tools for reconstructing the history of ancient Israel.

But, while Egyptian chronology of the early 1st millennium BC cannot be trusted as an independent tool for dating – as its dates at least in part depend on biblical chronology – Neo-Assyrian chronology rather provides us with reliable data. For the 9th and 8th century, dates are based on a firm anchor point, namely the so-called Bur-Saggile solar eclipse dated to the tenth year of king Aššur-dan III, an astronomical phenomenon that occurred on the 15th of June 763 BC. Consequently, the following dates for the ninth-century kings of Assyria may be considered as firm:

Kings	Regnal years (BC)
Adad-nirari II	910–891
Tukulti-Ninurta II	890–884
Aššur-naṣirpal II	883–859
Shalmaneser III	858–824
Šamši-Adad V	823–811
Adad-nirari III	810–783

Table 2. Dates of ninth-century Assyrian kings.

These dates yield a solid framework for dating the historical events in the southern Levant during the 9th century BC. Table 3 presents correlations between Israelite and Judahite history on the one hand and Neo-Assyrian inscriptions of the 9th and 8th centuries BC on the other.

The events referred to in *Table 3* for the years 853, 841 and 796 BC are only known from Assyrian inscriptions and are absent from the Hebrew Bible. Even so, these dates are still relevant for biblical chronology as they provide a general framework for the regnal years of the Israelite monarchs: they show that both Ahab and Jehu of Israel were contemporaries of Shalmaneser III (858–824 BC) while Jehoash of Israel reigned at the time of Adad-nirari III (810–787 BC).

As a further point within our argumentation, we must consider the internal synchronistic chronology used by the redactors of the Hebrew Bible. Synchronistic chronicles were likewise used in Mesopotamia (i.e. in Assyria and Babylonia) as they were typical reference works employed at that time. *Table 4* lists the Israelite and Judahite kings and the number of their regnal years in line with the synchronistic chronology.

Multiple studies have been conducted to solve the remaining issues in biblical chronology. This is not the place to construct a new scheme to supersede older ones.[3] At any rate, the differences between the chronologies are minimal. A simple comparison between the two widely used chronologies in the German speaking world – that of Begrich and Jepsen (Begrich 1929; Jepsen and Hanhart 1964) and the recent study by the Finnish scholar Antti Laato (Laato 2015) – clearly demonstrate this (see *Table 5*).

Laato dates the death of Solomon four years earlier than Begrich/Jepsen, while by the death of Ahab (some 70 years later) both chronologies virtually coincide from

[3] For a recent overview, see Laato (2015, 5–13).

Kings of Israel	Number of regnal years	Became king in year X of	Biblical reference	Kings of Judah	Number of regnal years	Became king in year X of	Biblical reference
Jeroboam I	22	(yr 1 Rehoboam)	1 K 14:20	Rehoboam	17	(yr 1 Jeroboam I)	1 K 14:21
				Abijah	3	yr 18 Jeroboam I	1 K 15:1–2
Nadab	2	yr 2 Asa	1 K 15:25	Asa	41	yr 20 Jeroboam I	1 K 15:9–10
Baasha	24	yr 3 Asa	1 K 15:33				
Elah	2	yr 26 Asa	1 K 16:8				
Zimri	7 days	yr 27 Asa	1 K 16:15				
Omri[a]	12	(yr 27 Asa) yr 31 Asa	(1 K 16:21) 1 K 16:23				
Ahab	22	yr 38 Asa	1 K 16:29	Jehoshaphat	25	yr 4 Ahab	1 K 22:41–42
Ahaziah	2	yr 17 Jehoshaphat	1 K 22:52				
Joram	12	yr 2 Jehoram (Judah) yr 18 Jehoshaphat	2 K 1:17 2 K 3:1	Jehoram	8	yr 5 Joram (Israel)	2 K 8:16–17
				Ahaziah	8	yr 12 Joram (Israel) yr 11 Joram (Israel)	2 K 8:25–26 2 K 9:29
Joram of Israel and Ahaziah of Judah were killed in the same year							
Jehu	(29)[b]			Athaliah	6 or 7[c]		(2 K 11:4)
				Joash	40	yr 7 Jehu	2 K 12:1
Jehoahaz	17	yr 23 Joash	2 K 13:1				
Jehoash	16	yr 37 Joash	2 K 13:10	Amaziah	29	yr 2 Jehoash	1 K 14:1–2
Jeroboam II	41	yr 15 Amaziah	2 K 14:23	Uzziah	52	yr 27 Jeroboam II	2 K 15:1–2
Zechariah	6 months	yr 38 Uzziah	2 K 15:8				
Shallum	1 month	yr 39 Uzziah	2 K 15:13				
Menahem	10	yr 39 Uzziah	2 K 15:17				
Pekahiah	2	yr 50 Uzziah	2 K 15:23				
Pekah	20	yr 52 Uzziah	2 K 15:27	Jotham	16	yr 2 Pekah	2 K 15:32–33
Hoshea	9	yr 12 Ahaz	2 K 17:1	Ahaz	16	yr 17 Pekah	2 K 16:1–2
				Hezekiah	29	yr 3 Hoshea	2 K 18:1–2
yr 7 Hoshea = yr 4 Hezekiah: Shalmaneser V besieges Samaria							2 K 18:9
yr 9 Hoshea = yr 6 Hezekiah: conquest of Samaria							2 K 17:6; 18:10–11

Table 4. Dates of Israelite and Judahite kings in line with the synchronistic chronology.

[a] The problem is that Omri's rule was opposed by one Tibni son of Ginath. The 12 years started when Omri became king (i.e. in year 27 of Asa of Judah), while the civil war lasted for some four years. Hence, year 31 of Asa coincides with Omri's accession year as *sole* ruler over Israel.

[b] The 29 regnal years of Jehu are not directly mentioned in the biblical text but may be calculated by adding the 23 years of Joash's reign (2 Kings 13:1) and the seven years of 2 Kings 12:1. One year was subtracted because likely the first and last years were fully counted.

[c] The biblical text mentions seven years, but the first and last years are included. Hence a total of six years seems more likely.

Kings of Israel	After Begrich and Jepsen	After Laato	Kings of Judah	After Begrich and Jepsen	After Laato
Jeroboam I	926–907	930/929–909/908	Rehoboam	926–910	930/29–914/913
			Abijah	910–908	913/912–910/909
Nadab	907–906	909/908–908/907	Asa	908–868	910/909–870/869
Baasha	906–883	908/907–885/884			
Elah	883–882	885/884–884/883			
Zimri		884/883			
Omri[a]	882/878–871	884/883–873/872			
Ahab	871–852	873/872–852/851	Jehoshaphat	868–847	870/869–847/846
Ahaziah	852–851	852/851–851/850			
Joram	851–845	851/850–845/844	Jehoram	852/847–845	847/846–846/845
			Ahaziah	845	846/845
Jehu	845–818	845/844–818/817	Athaliah	845–840	845/844–840/839
			Joash	840–801	840/839–801/800
Jehoahaz	818–802	818/817–802/801			
Jehoash	802–787	802/801–787/786	Amaziah	801–787	801/800–787/786
Jeroboam II	787–747	787/786–747/746	Uzziah	787–736	786/785–735/734
Zechariah	747	747/746			
Shallum	747	746/745			
Menahem	747–738	746/745–737/736			
Pekahiah	737–736	737/736–735/734			
Pekah	735–732	735/734–732/731	Jotham	759–744	750/749–735/734
Hoshea	731–723	731/730–723/722	Ahaz	(744)736–729	745/734–725/724
			Hezekiah	728–700	725/724–697/696

Table 5. Dates according to Begrich and Jepsen after Laato (2015). [a] See n. [a] in *Table 4*

the mid-9th century onwards. The small differences that remain, mainly concern methodological issues inherent to the synchronistic chronologies. As for Judah, the chronological information is very similar during the 9th century. Only during the later years of the 8th century do we again face some differences where it seems co-regencies will need to be accepted for these years to solve the discrepancies. Despite the remaining uncertainties, the different interpretations are all compatible with the known Assyrian synchronisms.

Hence, we have no reason to doubt the overall information provided by the synchronistic chronology of the Hebrew Bible. Unlike the regnal lengths attributed to David and Solomon, both of whom are said to have reigned for 40 years – a round number that is regularly employed in the Hebrew Bible to describe a generation or a specific period of time (e.g. Ex. 16:15; Num. 14:33; Judg. 3:11; 5:31; 8:28; 1 Sam. 4:18) – the regnal years given for the succeeding monarchs are so specific that they are quite probably authentic numbers derived from official Israelite and Judahite records. And as a matter of fact, the numbers are by no means inferior to those found in the Egyptian and Mesopotamian records. The literary style ('Gattung') is the same and therefore the administrative systems with their official scribes in the royal bureaucracies of these contemporary cultures are comparable (cf. Zwickel 2020). Hence, we may surmise that the available chronological data reaching back to the final quarter of the 10th century BC (i.e. from the death of Solomon onwards) is generally trustworthy. The uncertainties which remain do not exceed more than four years in the scenarios cited above. These additional years may be solely due to mathematical problems related to the synchronistic chronologies.

For practical reasons only, the present author has decided to use the chronology of Begrich/Jepsen as his reference tool throughout this article, because the other chronologies at hand ultimately lead to very similar results.

Israel		Judah	
(Omri	882/878–871)	(Asa	908–868)
Ahab	871–852	Jehosha-phat	868–847
Ahaziah	852–851		
Joram	851–845	Jehoram	852/847–845
		Ahaziah	845
Jehu	845–818	Athaliah	845–840
		Joash	840–801

Table 6. Dates of the ninth century kings of Israel and Judah after Begrich/Jepsen.

We shall now seek to bring this basic chronology into line with additional contemporary information from both the biblical and extra-biblical texts to arrive at a closely knit scheme for the history of events during the period under consideration. While similar endeavours have been undertaken by other scholars, we seek to include here data from regions further afield to get the larger picture. Riots in the Levant against the Assyrians almost always occurred when the Assyrians were active elsewhere, when political problems had arisen in the Assyrian homeland, or when battles were fought nearer the central polity. Levantine rulers knew well when the risk of rebellion was low and when the right time to revolt had come. Considering the broader picture within the Assyrian empire and what happened during each single year will facilitate the construction of a more thorough sequence of events.

Within the framework of the chronological system used here, the present author will concentrate on what happened mainly in Israel during the middle years of the 9th century, from the accession of Ahab in 871 BC until the death of Jehu in 818 BC. The historical data from Judah for these years do not yield sufficient information as Judah was not yet in focus on the international political stage. It is our aim to combine as much data as possible from the ancient Near East to establish a sound chronological scenario.

Assyrian Sources

Aššurnaṣirpal II (883–859 BC), Shalmaneser III (858–824 BC) and Šamši-Adad V (823–811 BC) reigned in Assyria during the years under consideration. Based on Assyrian sources, we possess information about campaigns conducted by these monarchs into several foreign countries. Normally, biblical historians are only interested in campaigns carried out in the Levant. However, we must also consider *all* other military campaigns conducted further afield as well as political turmoil that may have arisen nearer the central polity of Assyria if we are to fully appreciate the history of the Levantine states when due to an inward-looking Assyria these states revolted, reformed their armies or fought battles against neighbouring states. The Assyrian annals also include helpful information on Israel, Phoenicia, Aram and Egypt.

During the years 876 to 867 BC Aššurnaṣirpal II conducted campaigns in northern Syria against the city-states of Bit-Adini, Carchemish and Hazazu. By crossing the Orontes, he entered the northern Lebanon (we follow the dates for these campaigns in Weißbach 1932 and Bagg 2011, 191–194, while we restrict ourselves to the campaigns after 871 BC). The cities of Tyre, Sidon, Byblos, Mahallutu, Maisa, Kaisu and Arwad consequently paid tribute to Assyria. These Phoenician towns lie near the Mediterranean shore. As its citizens wished to maintain their trading activities, they avoided involvement in military activities. As a matter of fact, they needed the Assyrians as trading partners in the hinterland. The Israelites too acknowledged Assyria as a military power. In 866 BC Aššurnaṣirpal II undertook yet another campaign to the north.

The succeeding Assyrian king, Shalmaneser III, campaigned on a nearly annual basis throughout his entire reign. In total he conducted no less than 35 military campaigns. *Table 7* provides an overview of these interventions and is based on Baker (2006–2008) and Bagg (2011, 194–205). We have marked the campaigns to the Levant in grey.

The Assyrian annals inform us about tribute paid by the Phoenician cities in 858 and 841 BC and that Israel too paid tribute in 841 BC. They also tell us about the famous battle fought by the anti-Assyrian alliance at Qarqar in the year 853 BC (Weippert 2010, nos. 106–107) and about other incursions into the southern Levant. Additionally, the Assyrian inscriptions refer to the death of Hadad-ezer [Adad-idri] of Aram in 842 BC and the usurpation of the throne by Hazael (Weippert 2010, no. 111). The seizure of the Damascene throne by Hazael is also attested in a biblical story (2 Kings 8:7–15).

After 837 BC, Shalmaneser III did not conduct any further campaigns to the southern Levant. This observation is crucial as the absence of incursions by the Assyrians allowed Hazael to engage in battle against Israel in the following years. While he had lost territory to the north, Hazael conquered new lands to the south by invading parts of Israel.

As for Šamši-Adad V, the years prior to the death of Jehu in 818 BC are of particular interest to us here. This Assyrian monarch did not initially conduct campaigns to the Levant as he was preoccupied with smiting rebellions in Assyria proper which first erupted in 826 towards the end of his father's reign. Although finally put down by 820 BC, it took Šamši-Adad several more years to consolidate his power. Only during the later years of

Year	Campaigns
858	Campaign to northern Syria (e.g. Til Barsip, Carchemish); the kings along the Mediterranean shore (including Sidon and Tyre) pay tribute; campaign to regions north of Assyria (including Nairi near Lake Urmia) and tribute paid by cities in Urartu and its vicinity
857	Campaign to northern Syria (e.g. Til Barsip, Carchemish); several towns pay tribute
856	Campaign to and tribute paid by Bit Adini (including Til Barsip); campaign to regions north of Assyria (including Nairi and Urartu)
855	Campaign to Bit Adini and the Zagros mountains
854	Campaign to a region to the north of Assyria; its cities pay tribute
853	Campaign to northern Syria including the famous battle at Qarqar against a coalition of anti-Assyrian forces (i.e. Aram, Hamath, Israel, Byblos, Egypt, Irqanat, Arwad, Ushanat, Siyanu, Arabia, Bit-Rehob); tribute paid by states in northern Syria (e.g. Carchemish, Kummuch, Bit-Gush, Patina) and Anatolia
852	Campaign conducted against northern Syria, conquest of Til-Abne; campaign to the source of the Tigris and tribute paid by Nairi
851	Campaign to Babylonia to suppress a rebellion there
850	Support given to the king of Babylonia
849	Campaign to northern Syria (including Carchemish, Aram, Hamath)
848	Campaign to northern Syria to suppress rebellions by Aram and Hamath; tribute paid by Unqi
847	Campaign to northern Syria, conquest of Paqarahubunu
846	Campaign to regions in the north
845	Campaign against northern Syria to suppress rebellions (Aram, Hamath)
844	Campaign to the upper Tigris against Nairi; tribute paid by Daiane
843	Campaign to the Zagros mountains
842	Peaceful campaign to northern Syria, kings of Hatti pay tribute
841	Rebellion by coalition headed by Aram; campaign to the southern Levant (including Senir in the Antilebanon, Mount Carmel [Ba'alira'asi], a siege of Damascus, while Israel, Tyre and Sidon pay tribute
840	Peaceful campaign to northern Syria
839	Campaign to Anatolia (Que) with support of some kings of Hatti
838	Campaign to Aram (Damascus) to subdue a rebellion; tribute paid by Tyre, Sidon and Byblos
837	Continuation of the campaign against Damascus, perhaps including some villages in northern Transjordan
836	Campaign to Tabal in Anatolia; tribute paid
835	Campaign to Anatolia; Tabal renews tribute payment and campaign to the Zagros mountains
834	
833	Campaign to Anatolia (Que); conquest of Muru of Bit-Agusi
832	Continuation of the campaign against Anatolia (Que); conquest of Tarsos
831	Campaign to Que in Anatolia
830	Campaign to countries in the north (Urartu)
829	Campaign to northern Syria
828	Campaign to countries in the north
827	Campaign to the Zagros region, tribute paid
826	Campaign to countries in the north and to the Zagros region (Musasir, Urartu)
825	
824	

Table 7. Campaigns of Shalmaneser III on a virtually annual basis.

his reign did he conduct two, albeit undated, campaigns (i.e. before 814 BC) against northern and eastern territories (e.g. against Nairi, Gizilbunda and Media) while campaigns are also listed against Babylonia, several of which are dated (814, 813, 812 BC).

Egyptian Data

As far as we can tell, Egypt seems to have engaged little in foreign affairs during the 9th century BC. Only the participation by a small military contingent at the battle of Qarqar is attested. This would have happened during the reign of Osorkon II, who is conventionally dated to this period, although his name is not mentioned in the Assyrian inscription. A statue of Osorkon II found at Byblos may have been offered by this king as a political gift (Schipper 1999, 174–177; Ritner 2009, 288; Bradl 2012, Table 20:1a–c; cf. Wallenfels, elsewhere in this volume p. 259), while two fragments of an alabaster amphora appear to refer to him at Samaria (Schipper 1999, 177–180). These sparse witnesses seem to demonstrate that some minor diplomatic and trade connections existed between Egypt and the Levant.

Tel Dan Stele

Besides the Assyrian inscriptions, the Tel Dan Stele (Weippert 2010, no. 116) also offers some information. Since the names of Joram of Israel and Ahaziah of Judah/Bet-David can be reconstructed, the stele describes the events of the year 845 BC. Hazael, who was the Damascene ruler who commissioned the inscription enumerating his military achievements, ascended the throne in 841 BC. However, his claim to have personally killed Joram and Ahaziah appears to be contradicted by the sequence of events described in 2 Kings 8:28–29 where these kings were murdered by the Israelite usurper Jehu (2 Kings 9:14–29). The Tel Dan Stele also refers to a conquest of Aramaean territory by Israel in the time before Jehu's reign.

Mesha Stele

Another stele dating to about the middle of the 9th century BC is the Mesha inscription (Weippert 2010, no. 105). The stele relates that Israel occupied territory of Moab during the reign of Omri and his son(s) for a period of about 40 years. Again, this round number appears to be symbolic and may represent either one generation or rather an extended period of time.

Biblical Chronological Data

Besides the information given in the synchronistic chronology, there is also further historical and chronological information from other passages in the Hebrew Bible:

1 Kings 17:1–18:46 (cf. 2 Kings 4:38; 2 Kings 8:1; Sir. 48:2): There was a famine in Israel and Phoenicia during the reign of king Ahab, which is said to have lasted between three (1 Kings 18:1) and seven years (2 Kings 8:1; Zwickel 2012). The seven-year famine should likely be taken as symbolic. Hence its exact length escapes us. (1 Kings 20:1-12 [Siege of Samaria] and 20:13-21 [Defeat of the Arameans]: These two texts belong to a later reign than Ahab's (cf. Zwickel 2019, 279–280, n. 60), and need no discussion here.

1 Kings 20:26–34: This passage describes the defeat of king Ben-Hadad at Aphek during a raid against Israel. It is stated that the event occurred after one year (1 Kings 20:26; cf. v. 22) but its point of reference is unknown to us. It does not appear to refer to what is said in 1 Kings 20:1–12 and 20:13–21, as this pericope likely refers to later events. It seems likely that the defeat of Ben-Hadad occurred around 855 BC or earlier. At that time Shalmaneser III fought a battle in northern Syria while Aram-Damascus was not (yet) affected by his campaigns.

1 Kings 22:1–38: After three years of peace (1 Kings 22:1) there was a battle of a Judahite-Israelite coalition against the Aramaeans at Ramoth Gilead. It was at this time that Ahab died in battle (1 Kings 22:38). If Ahab's death is dated to 852 BC, the three years of peace would be dated to *c.* 854–852 BC (i.e. partial years were reckoned as whole years). This seems to be a logical deduction in line with the geopolitical circumstances of these years. Already by 858 BC, when Sidon and Tyre had paid tribute to Assyria, all Levantine states were fully aware that the Assyrian threat was a real danger to their political independence. Hence, it became even more necessary to lay the intra-Levantine conflicts to rest and form a political alliance against Assyria. This resulted in the battle of Qarqar in 853 BC. This explains why Israel and Aram were not at war during these years. But once the battle was over, the Israelites formed a new coalition against the Aramaeans to increase their territory in the northern Transjordan.

1 Kings 22:49–51: Jehoshaphat of Judah built a ship to sail to Ophir and asked Ahaziah of Israel to participate in this venture. The expedition however never set sail due to a disaster at Ezion-Geber (V. 48). Ahaziah reigned only in 852 and 851 BC and thus preparations for the expedition would have been made during these years.

2 Kings 1:1; 3:5: Moab cast off the Israelite yoke of oppression after Ahab's death in 852 BC. Political independence often becomes possible after the death of an oppressor as his successor is initially preoccupied with establishing his own authority in his homeland before he is able to go to war himself. Therefore, the expansion of Moabite territory referred to in the Mesha stele likely commenced in 852 BC. It is thus very reasonable that Joram of Israel joined forces with the Judahite ruler (apparently king Jehoshaphat) to fight against Moab.

Since, according to the stele, king Mesha had conquered the region north of the Moabite homeland, it is now comprehensible why Mesha was attacked from the south (2 Kings 3:6–27): attacking Moab from the south only makes sense if Mesha's invasion to the north was still in progress. This campaign therefore likely happened in 851 (or a few years later).

(In 2 Kings 6:23–7:20 we are told that Ben-Hadad of Aram besieged Samaria. This is probably a later redactional comment [cf. Zwickel 2019, 276, n. 43] and hence there is no need to discuss it here).

2 Kings 8:20–22: Edom broke away from Judah during the reign of Jehoram of Judah. It remains unclear when exactly this happened but during his initial years Jehoram was only a co-regent. Subsequently Jehoram became sole ruler in 847–845 BC. Likely the Edomite liberation happened in Jehoram's first regnal year followed by a military reaction against Edom in the same or (likely) following year.

2 Kings 8:22: Libnah also liberated itself from Judah at the same time as Edom. This was likely in 847 BC.

2 Kings 8:28–29: Joram of Israel and Ahaziah of Judah conducted a campaign against the Aramaeans. Since Ahaziah only reigned for one year, this would have happened in 845 BC. To fight a battle against the Aramaeans in this particular year was significant as Hazael of Aram was still preoccupied with fighting the Assyrians in the north.

2 Kings 9: Jehu was anointed king over Israel and killed Joram of Israel and Ahaziah of Judah. According to the synchronistic chronicle this happened in 845 BC.

2 Kings 10:32–33: War(s) were fought between Jehu of Israel and Hazael of Aram-Damascus during which the latter considerably reduced Israel's territory. The final campaign fought by Shalmaneser III against Aram is dated to 838 and 837 BC. Hazael would hardly have attacked Israel while still being at war with Assyria, i.e. by creating a war on two fronts. Therefore, the Aramaean campaign(s) against Israel would not have begun before 836 BC. It is possibly that the wars between Jehu of Israel and Hazael of Aram lasted almost without interruption until 818 BC, the year that Jehu died, and into the reign of his successor Jehoahaz (2 Kings 13:3–7). These campaigns resulted in a virtually complete destruction of the Israelite army (2 Kings 13:7). Consequently, Hazael was able to traverse Israelite territory without being stopped by Israelite soldiers (2 Kings 12:18).

2 Kings 12:7: In his 23rd year Joash of Judah (= 818 BC) organized restoration works for the temple in Jerusalem.

Summary

Summarizing the results of the foregoing discussions, our knowledge of the historical events in the middle of the 9th century is not as meagre as some scholars may believe (see Table 8). For we possess reliable data for this period and a comparatively dense network of historical information. Some of this evidence is substantiated by the geopolitical circumstances of the relevant years. The combination of these data with the circumstances known from regions further afield, which indirectly influenced political and military activities in the southern Levant, confirms the overall correctness of the historical reconstruction, even if the proposed dates may deviate by a year or two. The synchronistic history in the Hebrew Bible therefore appears to be a reliable document for the centuries after Solomon's death. The general scepticism or even nihilism adhered to by some scholars is therefore unwarranted.

Table 8. Combines dates and events as discussed in this article.

Year	Assyria	Israel	Judah	Edom, Moab, Ammon	Arameans	Phoenicia	Egypt
871	Aššurnaṣirpal II invades northern Lebanon, all Phoenician towns pay tribute	Death of Omri, Ahab becomes king in Israel				Aššurnaṣirpal II invades northern Lebanon, all Phoenician towns pay tribute	
870							
869							
868			Death of Asa, Jehoshaphat becomes king in Judah				
867							
866	Campaign in the north						
865							
864							
863							
862							
861							
860							
859	Death of Aššurnaṣirpal II						
858	Start of Shalmaneser III's reign, campaign in northern Syria (e.g. Til Barsip, Carchemish), the kings along the Mediterranean shore (incl. Sidon and Tyre) pay tribute, campaign against regions to the north of Assyria (e.g. Nairi at Lake Urmia), cities in Urartu and its vicinity pay tribute					Sidon and Tyre pay tribute to Assyria	
857	Campaign in northern Syria (e.g. Til Barsip, Carchemish), several towns in northern Syria pay tribute						
856	Campaign against Bit Adini (e.g. Til Barsip), Bit-Adini pays tribute, campaign against regions to the north of Assyria (e.g. Nairi, Urartu)						
855	Campaign against Bit Adini and the Zagros region	War between Israel and Aram (1 K 22:1). By now, Ramoth-Gilead belongs to Aram			War between Israel and Aram (2 K 22:1). By now, Ramoth-Gilead belongs to Aram		
854	Campaign against regions to the north of Assyria, its cities pay tribute	Peace between Israel and Aram (1 K 22:1)			Peace between Israel and Aram		

Year	Assyria	Israel	Judah	Edom, Moab, Ammon	Arameans	Phoenicia	Egypt
853	Campaign against northern Syria including the battle of Qarqar (against Aram, Hamath, Israel, Byblos, Egypt, Irqanat, Arwad, Ushanat, Siyanu, Arabia, Bit-Rehob), tribute paid by states in northern Syria (e.g. Carchemish, Kummuch, Bit-Gushi, Patina) and Anatolia	Peace between Israel and Aram, Ahab of Israel joins forces with the anti-Assyrian coalition at Qarqar			Peace between Israel and Aram, Hadad-Ezer of Aram, Irkhuleni of Hamath, Irqanat and Baasha of Bit-Rehob join forces at Qarqar	Byblos and Mattinu-Baal of Arwad join forces at Qarqar, Tyre and Sidon do not releases	Egypt joins the anti-Assyrian coalition at Qarqar
852	Campaign against northern Syria, conquest of Til-Abne, campaign to the source of the Tigris, Nairi pays tribute	Battle by an Israelite/Judahite coalition against Aram to reconquer Ramoth-Gilead, death of Ahab, Ahaziah becomes king in Israel, expansion of Moabite territory at the expense of Israel	Jehoram becomes co-regent in Judah, battle by an Israelite/Judahite coalition against Aram to reconquer Ramoth-Gilead	Expansion of Moabite territory at the expense of Israel	Battle by an Israelite/Judahite coalition against Aram to reconquer Ramoth-Gilead		
851	Campaign to Babylonia to smite a rebellion there	Ahaziah of Israel joins the Judean expedition to Ophir, death of Ahaziah, Joram becomes king in Israel, Moab conquers parts of the territory of Reuben and Gad, Joram starts a campaign (with Judah) against Moab, the campaign fails	Jehoshaphat equipped a fleet to Ophir, Joram of Israel commenced a campaign (with Judah) against Moab, but the campaign fails	Moab conquers parts of the territory of Reuben and Gad, Joram starts a campaign (with Judah) against Moab, the campaign fails			
850	Support lent to the king of Babylonia						
849	Campaign against northern Syria (e.g. Carchemish, Aram, Hamath)				Shalmaneser III defeats the Aramaeans of Hadad-ezer in northern Syria		
848	Campaign against northern Syria to smite rebellions there (Aram under Hadad-ezer and Hamath under Irkhulena), Unqi pays tribute			Likely continuation of the conquest of Israelite territories by Mesha of Moab	Shalmaneser III suppresses a revolt by the Aramaeans under Hadad-ezer and by Hamath under Irkhulena		
847	Campaign to northern Syria, conquest of Paqaraḫu-buni		Death of Jehoshaphat, Joram becomes sole ruler in Israel, Edom liberates itself from Judah, Libnah liberated from Judah	Edom released from Judah			

Year	Assyria	Israel	Judah	Edom, Moab, Ammon	Arameans	Phoenicia	Egypt
846	Campaign to regions in the north of Assyria						
845	Successful campaign against northern Syria to smite rebellions there (Aram under Hadad-ezer, Hamath under Irkhulena)	Joram of Israel and Ahaziah of Judah conduct a campaign against Hazael of Aram, likely when the Aramaeans were engaged in battle against Assyria. Joram was wounded in battle and was killed by Jehu, Israel's new king	Death of Jehoram of Judah, Ahaziah becomes king of Judah, Joram of Israel and Ahaziah of Judah conduct a campaign against Hazael of Aram, Ahaziah is killed later by Jehu, Athaliah becomes queen of Judah		Aramaean troops are engaged in battle against Assyria in northern Syria, Joram of Israel and Ahaziah of Judah conduct a campaign against Hazael of Aram		
844	Campaign to the upper reaches of the Tigris, Nairi, Daiane pay tribute						
843	Campaign against the Zagros region						
842	Peaceful campaign to northern Syria, kings of Hatti pay tribute				Hadad-ezer of Aram dies		
841	Rebellion by a coalition led by Aram, campaign to the southern Levant (Senir = Anti-Lebanon, Mount Carmel = Ba'al ra'asi), siege of Damascus, Israel, Tyre and Sidon pay tribute, erection of stelae by Shalmaneser III at Nahr el-Kelb and Mount Carmel	Jehu of Bet-Omri (= Israel) pays tribute to Assyria, Hazael conquers the Israelite town of Dan and expands his territory at the expense of Israel			The usurper Hazael becomes king of Aram, Damascus is besieged, Hazael conquers the Israelite town of Dan and expands his territory likely as a reaction to the Assyrian conquest of Aram in the north, Hazael claims on the Tel Dan Stele that he killed Joram of Israel and Ahaziah of Judah	Sidon and Tyre pay tribute to Assyria	
840	Peaceful campaign to northern Syria		Death of Athaliah, Joash becomes king in Judah				
839	Campaign against Anatolia (Que) with the support of some kings of Hatti						
838	Campaign against Aram-Damascus after a rebellion, Tyre, Sidon and Byblos pay tribute				Assyrian campaign against Aram-Damascus after a rebellion	Tyre, Sidon and Byblos pay tribute	
837	Continuation of the campaign against Damascus, perhaps Assyrian troops reach villages in northern Transjordan				Continuation of the campaign against Damascus, perhaps Assyrian troops reach villages in northern Transjordan	Tyre, Sidon and Byblos pay tribute	

Year	Assyria	Israel	Judah	Edom, Moab, Ammon	Arameans	Phoenicia	Egypt
836	Assyria conducts no further campaigns to the Levant, political riots had obviously ended, campaign against Tabal in Anatolia followed by tribute payment	War between Israel and Aram, during several years Hazael, despite Assyrian intervention, brings large parts of Israel under his control, as far south as Gath			War between Israel and Aram, during several years Hazael, despite Assyrian intervention, brings large parts of Israel under his control, as far south as Gat		
835	Campaign against Anatolia, Tabal renews tribute, campaign against the Zagros region						
834							
833	Campaign against Anatolia (Que), conquest of Muru belonging to Bit-Agusi						
832	Continuation of the campaign against Anatolia (Que), conquest of Tarsus						
831	Campaign against Que in Anatolia						
830	Campaign against countries to the north (Urartu)						
829	Campaign against northern Syria						
828	Campaign against countries to the north						
827	Campaign against the Zagros region, tribute received						
826	Campaign against countries in the north and the Zagros region (Musasir, Urartu)						
825							
824	Death of Shalmaneser III						
823	Reign of Šamši-Adad V						
822							
821							
820							
819							
818		War between Israel and Aram, death of Jehu, Jehoahaz becomes king in Israel	Joash undertakes restoration works at the temple in Jerusalem				

Table 8. Combines dates and events as discussed in this article.

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Assyrian Synchronisms, the 'Tyrian Annals' and Carthage: Repercussions for Ancient Greek Chronography and Mediterranean Archaeology

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The study of the Tyrian Annals and related sources shows significant surprises, including Troy's Fall, which, according to Phoenician chronographers, was dated to 10th century BCE, i.e. more than two centuries later than other classical writers would argue. This shows that our modern tension within Mediterranean and Near Eastern archaeology – i.e. between a 'high' and a 'low' chronology – already existed during antiquity. If accepted at face value, traditional chronographical work would have clear consequences for the dating of relevant archaeological research. The Annals also set the date of the foundation of a Phoenician city called 'Qart-ḥadašt' in the late 9th century BCE, evidently on the island of Cyprus as determined by evidence found in the island. The colony subsequently ceased to exist, making the Hellenistic readers of the Annals to assume that 'Qart-ḥadašt' can only be the well-known Carthage in North Africa. This solves the problem of the early date of the foundation of Carthage which disagrees with the chronology of local archaeology.

Introduction

The value of the 'Tyrian Annals', fragments of which are preserved primarily in Josephus, has often been doubted. However, an examination of literary evidence from Thales of Miletus (6th century BCE) to Timaeus of Tauromenium (4th/3rd century BCE) has shown that the Greek and Hellenised worlds were well aware of Phoenician ancient records. One Hieronymus the Egyptian/Phoenician, in the 3rd century BCE, followed by Menander of Ephesus/Pergamum, in the 2nd century BCE, translated officially the 'Tyrian Annals' into Greek. The core of the original archive was a Phoenician king-list (stating names, ages and reign lengths), covering the period from the mid-10th to the late 6th century BCE, and annotated with brief historical notices relating the major acts performed by the kings – not unlike Babylonian Chronicle 1 extending from 747 to 668 BCE[1] and the Assyrian Eponym List from 911/10 to 648 BCE.[2] The Tyrian list would have been composed working from local archives in the early Persian period. Surviving fragments concern three 'floating' segments which can be pegged in time and augmented by Assyrian and Babylonian synchronisms.[3] The first begins with Eirōmos I (Hiram I) in 955/4 BCE and stops at the end of Pygmalion (Pumiyaton)'s reign in 768/7 BCE. The second covers only the reign of Eloulaios (Elul), at the end of which (seemingly at 697 BCE) Sennacherib conquered Tyre, having already (701 BCE) installed as king over Phoenicia one Tubail II. The gap from 768/7 to 697 BCE can be bridged by three kings found in the Assyrian records: Tubail I, Hiram II and Mettena. The third segment extends from the reign of Ithobalos II (Ithobaal II) in 590/89 BCE to the end of that of Eirōmos III (Hiram III) in 533/2 BCE. Concerning the second gap from 697 BCE to 590/89 BCE, one king is known from the Assyrian records: Baal under Esarhaddon (in *c.* 677 and 671 BCE) and Assurbanipal (in 662 BCE). Also, another Hiram, father of Ithobalos II, may well have reigned some years before and up to 590/89 BCE, as revealed by a Phoenician inscription published more recently.[4]

Chronological Surprises and the Tyrian Annals

Detailed study of the Tyrian Annals has revealed several surprises, perhaps the most significant being the date of the Fall of Troy, which, from the Phoenician chronographic point of view, was firmly placed in the 10th century BCE.[5] Many ancient Greek chronographers found this difficult to swallow in their political desire

[1] Grayson (1975, 69–87).

[2] Millard (1994).

[3] See Kokkinos (2013).

[4] Lemaire (2004); cf. Kokkinos (2013, 45, n. 72), where this inscription is reinterpreted.

[5] E.g. Kokkinos (2009b, 41–45; 2013, 43–45).

to claim a higher antiquity for their own cultural past. This 'low' dating could not have been invented by the translators in the Hellenistic period (against the contemporary tide of inventing 'high' dates), because it was found to have been known to the earliest Greek chronographer (or 'proto-chronographer') of the late 6th century BCE – that is to say Hecataeus of Miletus. It is now understood that the modern tension of archaeological work between a 'high' and a 'low' chronology, as previously demonstrated in *Centuries of Darkness*,[6] was already present in antiquity. Thus there are serious consequences for the dating of relevant archaeological material when guided by traditional chronographical work, for the latter had come to be split in ancient thought between a high 'heroic' setting and a low 'historical' one. Another significant surprise was the realisation, hitherto neglected, that Timaeus of Tauromenium (working in Athens between 314 and 264 BCE) had paid privately to have the Tyrian Annals translated, long before the 'official' translation by the subsequent Hellenised chronographers. This finally explained his extraordinary knowledge for dating (or massively down-dating) the foundation of famous 'Carthage'. Until then the event was placed in the 'heroic' age, to as early as 1215 BCE (*FGrH* 556 F 47), but Timaeus now argued for its immediate transfer to the 'historical' age of the late 9th century BCE. Timaeus' bold move changed the entire perspective of ancient Greek chronography, radically shifting the focus from a vague heroic past into a decidedly realistic historical context. His redating of Carthage took the foundation of Rome along with it, creating a major problem in the way the origins of this city had previously been perceived. However, although consistent with the lowered dating of events based on the 'new' Phoenician evidence, Timaeus at the same time seems to have compromised by retaining, apparently paradoxically, the date of the Fall of Troy in the 12th century BCE (or this is as far as we can be informed about his lost work).

Modern Scholarship and the Inflated Chronology of an 'Heroic Past'

Arnaldo Momigliano was puzzled by how Timaeus could have reconciled a foundation of Rome in 813 BCE, with his assertion (if so) that Troy fell in 1193 BCE.[7] The real question is, Why is Timaeus' date for Troy not in the 10th century BCE anyway, following the Tyrian Annals? No provable answer is possible, but it is conceivable that he was simply forced to operate with two chronologies simultaneously. He will not have been alone in doing so, given what we know of Herodotus in the 5th century BCE (reporting on Hecataeus of Miletus), and what we can gather from Josephus in the 1st century CE and Porphyry of Tyre in the 3rd century CE, who also based themselves on the Tyrian Annals.[8] Eratosthenes of Cyrene (working in Alexandria in *c.* 220 BCE), the famous chronographer and successor of Timaeus, must have paid no attention to the created discrepancy between Carthage/Rome and Troy, as he dated the Fall of Troy to 1183 BCE, only ten years later than Timaeus, and continued to regard Romulus as the grandson of Aeneas.

The compromised, traditional ('heroic') part of Timaeus' chronology must have later guided Manetho of Sebennytus (*c.* 260 BCE), whose Fall of Troy was placed 670 years (the sum total of the lengths of the Egyptian 20th–26th Dynasties) before the conquest of Egypt by Cambyses in 525/4 BCE (27th Dynasty), that is to say exactly in 1195/4 or 1194/3 BCE. This date, found 'miraculously' to be linked to the end of the 19th Dynasty, led modern Egyptologists, who essentially utilised Manetho,[9] to build their archaeological scheme around the inflated ancient Greek chronographic past. And then, by a scandalous, circular argument, initiated by Flinders Petrie based on the Greek legendary dating, Egyptian absolute chronology was offered as a valuable gift back to Greece – misleading the modern debate over the Greek archaeological 'Dark Age', and setting the standard date for the Late Bronze to Iron Age transition in the Eastern Mediterranean.[10]

The Founding and Whereabouts of Ancient 'Carthage'

Yet the revised ('historical') part of Timaeus' chronology, regarding the foundation of Carthage and Rome, remained generally untapped. Whether Timaeus' estimate for the event is to be taken as '813 BCE' or '823 BCE', it is not far from the real date of 808/7 BCE (the highest possible year for the beginning of the mission) that can now be calculated from King Pygmalion's year 7. During this year the Tyrian Annals mentioned Pygmalion's sister fleeing home on her way to founding Carthage. In the words of Josephus (*Ap.* 1.125):

> ἐν δὲ τῷ ἑπ'αὐτοῦ ἑβδόμῳ ἔτι ἡ ἀδελφὴ αὐτοῦ φυγούσα ἐν τῇ Λιβύῃ πόλιν ᾠκοδόμησεν Καρχηδόνα (It was in the seventh year of his reign that his sister, having taken flight to Libya, built the city of Carthage.)

The sister's original Phoenician name, Ἐλίσσα/Elissa (Elisha/אלישע; Heb., 'El [God] is salvation'), is preserved for us in the fragments of Timaeus (*FGrH* 566, F 82), later to be called Dido (Δειδώ), presumably locally and in

[6] See James *et al.* (1991).

[7] Momigliano (1977, 55).

[8] See also Kokkinos (2009a, 10–13; 2015).

[9] Cf. James and Morkot (2013).

[10] See Kokkinos (2009b, 49–51).

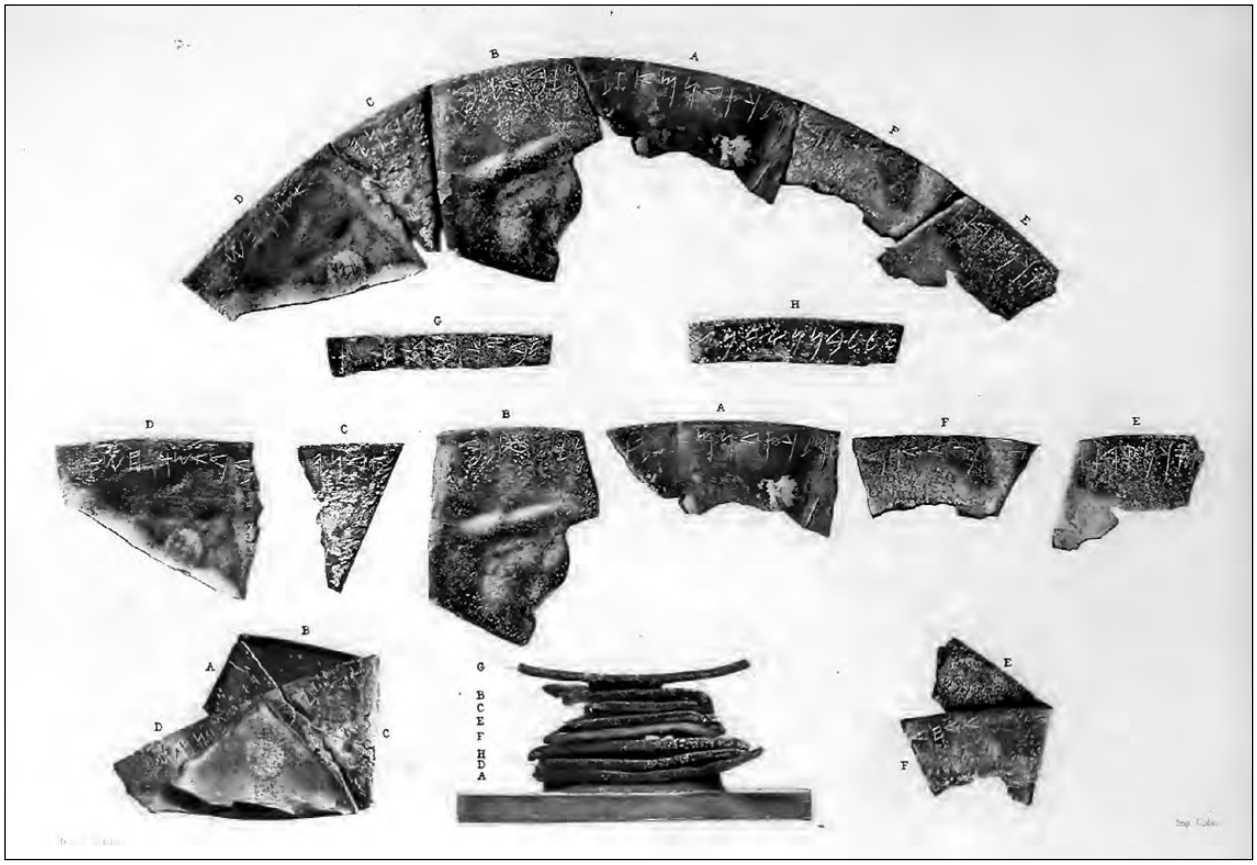

Figure 1. The written fragments of the two bronze bowls from the Bibliothèque Nationale attesting a Cypriot Carthage and a Phoenician governor, allegedly found on Mt. Sinoa (after CIS I 5, Table IV).

Punic, and subsequently to be remembered in Greek as Theiosso (Θειοσώ).[11] It is not important here to consider whether Elissa's name was connected to 'Alashiya' (found in Egyptian and Hittite sources and believed by most scholars to refer to the island of Cyprus),[12] or/and to 'Elishah'/אלישה/Ἐλίσα (found in Gen. 10:4 as one of the sons of Javan and grandson of Noah). Josephus (*Ant*. 1.127) spells the latter name as Alisas/Ἀλίσας (or Halisas/Ἁλισᾶς), which he understands in geographical terms to be a place inhabited by 'Aeolians'. But he does not specify whether he means the area of Aeolis on the western coast of Asia Minor (with Lesbos being its largest island), or the so-called 'Aeolian' islands northeast of Sicily (with Lipari being the largest). In any case Josephus separates this place from Chethima/Χέθιμα, the homeland of the Chethim/Χεθίμ (Kittim), according to him Cyprus. Ezekiel (27:6–7) is equally ambiguous when he refers both to 'the isles of Elishah' and to 'the isles of the Kittites' (Kittim/כתים), seemingly also two different places supplying different products to Tyre, with the latter commonly thought to be Cyprus. It is more important here to consider how the Phoenician name of 'Carthage' would have appeared in the Tyrian Annals before translation, and whether its identification with the one in Libya was merely Josephus' assumption.

'Carthage' on Cyprus

It has always been understood that the Greek name Καρχηδών (Carthage) is a transliteration of the Phoenician 'Qart-ḥadašt' (קרתחדשת), meaning New Town. As early as the beginning of the 2nd century BCE, this was clear to Cato the Elder (*apud* Solinus, *Collectanea Rerum Memorabilium*, 27.11), whether based on Timaeus or from his direct contact, that the Phoenician name 'Carthada' meant 'New City' (... *Carthadam dixit, quod Phoenicum ore exprimit civitatem novam*). As it happens, apart from the city in North Africa, we are aware of another Qart-ḥadašt/Carthage on Cyprus (*CIS* I 5; *KAI* 31; *TSSI* III 17), palpably an early colony under Hiram II (*c.* 741/40–735/4), documented on fragments of two bronze bowls in the Bibliothèque Nationale, and

[11] See Kokkinos (2013, 33 and n. 40). The name 'Elissa' survived also in Latin in Cato the Elder (*apud* Solinus, *Collectanea Rerum Memorabilium*, 27.10), and later (in a curious account of her story) in Pompeius Trogus of Gallia Narbonensis (*apud* Justinus' epitome of *Historiae Philippicae*, 18.4–6).

[12] For older literature, see Scheepers (1991, 80–83) and for recent discussions, see Gilbert (2017).

mentioned later as *Ḳar-ti-ḥa-da-as-ti* in the Assyrian records (*KB* 2, 240; *ANET*[3] 291, 294) under Esarhaddon (680–669 BCE) and Assurbanipal (668–631 BCE).[13] The written fragments (see *Figure 1*), attesting a Phoenician local governor, were brought to Paris from Limassol in the late 19th century, allegedly found on Mt. Sinoa (*c.* 20km northeast) – a story that has been doubted by some scholars in trying to find an 'appropriate' provenience further afield.[14]

But a vociferous opinion agreed that the location of Qart-hadast had to have been in the wider vicinity of Limassol, which includes nearby Amathus (*c.* 10km south of Mt. Sinoa).[15] What is clear, partly in contrast to this opinion, is that an identification cannot be made with a Late Roman/Early Byzantine city of the 5th century CE called Neapolis (New Town), for this was a 'new' foundation evidently in honour of Emperor Theodosius II (402–450 CE), known as Theodosiana/Theodosias/Neapolis,[16] with no previous history – even if it could be proven that it was situated within later Nemesos/Lemesos/Limassol. This leaves Amathus as the preferable identification of Cypriot Qart-hadast. Traditionally an autochthonous Eteocypriot foundation (Pseudo-Skylax, *Per.* 103) from the time of Homeric 'Kinyras' of Cyprus (*Iliad* 11.24–25),[17] Amathus would have endured Greek pressure, as it subsequently did strongly Phoenician.[18] This is consistent with the pottery finds and other archaeological evidence,[19] including non-Greek Cypriot syllabic inscriptions.[20] Kinyras is said to have originally founded Paphos (Pseudo-Apollodorus, *Bibl.* 3.14.3), before he was expelled by the followers of Agamemnon (Theopompos of Chios, *Philippica* 12 – *apud* Photius, *Bibl.* 176 = *FGrH* 115, F 103), when Paphos was re-founded by Agapenor (Aristotelian *Peplos* = *Anthol. Lyrica* [ed. Hiller], 368, no. 30).[21]

But if again the respective inscribed fragments of the bronze bowls had been transferred to Limassol from further afield, other cities with Phoenician influences not listed by Esarhaddon, such as Lapithos/Lapēthos (*c.* 60km north of Mt. Sinoa), could also be considered (see below). One way or another, Josephus (or his main source Menander) was not to know of the existence of Cypriot Qart-hadast, as its Phoenician name would have been dropped by his time. So it would not be unlikely if Josephus (or Menander) automatically assumed that the Tyrian Annals referred to the famous one in Libya.[22] It would be understandable for the earlier Carthage on Cyprus, to have become the 'mother city' of the later Carthage in North Africa. Of course this creates a problem for the dating of the latter's foundation, since the '808/7 BC' date, mentioned above, would instead concern the Cypriote Qart-hadast. Elissa in her flight from Tyre must have stopped at Cyprus anyway. This is also recorded at least by Pompeius Trogus in *c.* 15 BCE (*apud* Justinus' epitome of *Historiae Philippicae*, 18.5), whatever the worth of his elaborate account concerning North Africa (even if ultimately drawn from Timaeus). Questions are then raised. Did Elissa ever leave the island after the foundation of Qart-hadast? Would she personally have reached North Africa, as related by the later tradition, or would the affiliated priest and his family mentioned by Trogus have been sent forward on her behalf? If so, how many years later could that mission have taken place? But more generally, how does an early colony fit the literary, documentary, and archaeological evidence that can be gathered regarding Phoenician colonies on Cyprus, as much as regarding Carthage in North Africa? While Phoenician material of different types and periods (the result both of trade and settlement) is to be found in different areas of the island, we can only assume three early foundations or

[13] An early suggestion by E. Schrader (originally proposed by F. C. Movers) that 'Ḳar-ti-ḥa-da-as-ti' should be identified with Citium was firmly dismissed by Cooke (1903, 53). Nevertheless, it has been supported since by important scholars, including W. Landau, E. Gjerstad, W. F. Albright, V. Karageorgis, S. Moscati, as well as by the recent excavator of Citium, if with some reservation (Yon 1997, 11–12) – see now the review by A. Cannavò (2015). The tempting point for this identification is that Esarhaddon's inscription listing the kings of ten cities of Cyprus, while referring to Qart-hadast, omits major Citium. But whatever the reason (geopolitical or otherwise) for the omission, the list is manifestly incomplete. It also does not mention other cities, known to be early foundations, such as Amathus, Lapithos and Carpasia. Further, the Citium ostracon (*CIS* I 86; *KAI* 37; *TSSI* III 33) shows that at least in the 5th/4th century BCE, Citium was certainly not called Qart-hadast. Cannavò (2015, 149–150) thinks that the name change lasted only for a century, yet the same can be said by those supporting other sites for the identification – above all Amathus.

[14] See Gjerstad (1979, 235–236).

[15] Supporting Limassol or Amathus against the identification with Citium, see mainly Hill (1940, 108, n. 1); Peckham (1968, 321–322); Katzenstein (1997, 85); Lipiński (2004, 48–50).

[16] We hear of a Soter, Bishop of 'Theodosiana' at the Council of Chalcedon in 451 CE, of a John, Bishop of 'Theodosias' sometime later, and then of famous Leontios, Bishop of 'Neapolis' in the 7th century CE. The link is given by Theodore, Bishop of Paphos in his hagiographic *Life of Spyridon* (23) of the 7th century CE, mentioning 'John...who became Bishop of Theodosias, or the so-called New Town of the eparchy of Cypriots' ('Ιωάννου...γενομένου επισκόπου Θεοδοσιάδος ἤτοι Νέας Πόλεως τῆς Κυπρίων ἐπαρχίας). For full documentation, see Hill (1939, 375–379; cf. 1940, 263, n. 1).

[17] Cf. Brown (1965, 205–206); Pardee (1995).

[18] Gjerstad (1979, 243); Karageorghis (1988, 165).

[19] Bikai (1987).

[20] Aupert (1997).

[21] See Gjerstad (1944, 112); Petit (1999).

[22] True, Josephus' understanding of the Tyrian Annals may be correct in the case of 'Auza', founded 'in Libya', under 'Ithobalos' (*Ant.* 8.324), but here he would be wrong in assuming that the king responsible for this colony was Ithobalos I (878/7–847/6 BCE), 'the contemporary of Ahab' (873–852 BCE: Galil 1996, 147), rather than Ithobalos II (590/89–572/1 BCE).

re-foundations (based on literary and documentary sources). In chronological order these are: Citium, Carpasia and Qart-hadast/Carthage.[23] We may perhaps also assume two somewhat later ones: Lapithos (based on the presence of local Phoenician coins) and Urania/Aphrodisium (based on the cult of the sky-goddess).

In the Tyrian Annals, early in the reign (the year has not survived) of Hiram I (955/4–922/1 BCE), the king is said to have gone against 'Citium' (evidently in Cyprus being the only one known) to claim unpaid taxes. This means that the original subjugation might possibly have occurred earlier, late in the reign of his father, Abibaal. The name of the city, or rather the plural accusative of its ethnic form as Κιτιαίοις, is a modern emendation in the text of Josephus (MSS: Ἠυκαίοις/Ἰυκέοις/Ἠϋκέοις/Ἠύκαις/Τιτυοῖς/Τιτυαίοις), but it seems an inevitable one which has come to be widely accepted.[24] It replaced an older and unworkable emendation of Ἰτυκαίοις (in reference to Ἰτύκη/Utica).[25] In the words of Josephus (*Ant*. 8.146):

> τοῖς τε <Κιτιαίοις> ἐπεστρατεύσατο μὴ ἀποδιδοῦσι τοὺς φόρους καὶ ὑποτάξας πάλιν αὑτῷ ἀνέστρεψεν. (And he undertook a campaign against the people of <Citium>, who were refusing to pay their taxes, and having made them subject again to him, he returned home.)[26]

The restoration is conceivably supported by what we find later again in the Tyrian Annals. During the reign (the year has not survived) of Elul (733/2–698/7 BCE), it is chronicled that he 'sailed' to 'Citium' for exactly the same reason. In the words of Josephus (*Ant*. 9.284):

> οὗτος [βασιλεύς Ἐλουλαῖος] ἀποστάντων <Κιτιαίων> (MSS: Κιτταίων/Cetuteis) ἀναπλεύσας προσηγάγετο αὐτοὺς πάλιν. (He [king Eloulaios], upon a revolt of the Citians, having sailed [to Citium] he reduced them to submission again.)

The Judaean historian (*Ant*. 1.128) elsewhere refers explicitly to Κίτιον (Citium), according to him a Hellenised form deriving from the name of Χέθιμος (Chetimos), as discussed earlier.

At face value, the archaeological finds should not present a problem for Citium to be the earliest Phoenician colony west of Tyre. Archaeology has shown that Citium was occupied from the Late Bronze Age (from 13th century BCE as dated conventionally) in both main districts excavated (Kathari and Bamboula).[27] However, Kathari, according to the stratigraphy established by Vassos Karageorghis,[28] survived only until the earliest part of the Iron Age (up to about 1000 BCE),[29] followed by some 200 years of abandonment. This is odd. Bamboula is believed to have shown some traces of settlement along the ramparts next to the port, dating to the 10th century BCE,[30] but not much substance can be laid upon this evidence, especially since there exists a serious 'occupational problem' during this period across the island. An embarrassing archaeological gap thus remains, and all attempts to fill it up with a few equivocal burials, by stretching the available stratigraphy and its material, or even by revising the whole pottery chronology as set by Einar Gjerstad, amount frankly to mental gymnastics.[31] Under the circumstances, it appears as if Hiram I is not accommodated in Citium, and the 'Phoenician' city is thought to begin only from the 9th century, continuing through to its Assyrian subjugation (as evidenced by the famous stele of Sargon II, 722–705 BCE),[32] and to its later history. This may be acceptable to those supporting conventional absolute chronology for the Late Bronze Age (ultimately via Egypt), but not to those

[23] Cf. Jones (1971, 365).

[24] Stern (1974, 120, 122), based on Albright, prefers the reading as Κιτίοις. Lipiński (2004, 42, n. 23) disagrees altogether, proposing a reference to Acco/Akko. But this city was known in Greek as Ἄκη (e.g. Strabo, 16.2.25/758), and the plural accusative of its ethnic form would be Ἀκηαίοις or Ἀκηοῖς, an unconvincing emendation. Josephus (*Ant*. 8.37) also identifies Ἄκη 'on the sea' with the Solomonic district of governor Banakates (τὴν δε περὶ Ἀκήν παραλίαν εἶχε Βανακάτης), which seems to indicate Ba'ana the governor of Asher under Solomon (1 Kgs 4:16). Hiram I is not in place here at this time (cf. Katzenstein 1997, 104–108).

[25] For assessing the date of Utica, see Kokkinos (2013, 30, n. 27; 52). Bikai's attempt (1992) to revive the old emendation of Ἰτυκαίοις, remaining hopeful for a discovery of Phoenician LB/IA archaeological material in North Africa – something not hoped for even by 'high' chronologist Albright (1941, 21) – runs against recent understanding of the mechanics and purpose of ancient chronography. Also, Katzenstein (1997, 85) added that it would have been very dangerous at the beginning of his reign for Hiram to have personally gone as far away as Utica (bypassing Cyprus) and be absent a long time from his throne. Further, it sits uncomfortably with her [Bikai's] own 'low-dated' (correctly) Cypro-Phoenician ceramic horizon, since the earliest local pottery in Carthage is associated with material akin to Tyre III–II (see James, Kokkinos and Thorpe 1998, 30).

[26] Cf. *Ap*. 1.119: ὁπότε <Κιτιαίοις> ἐπεστρατεύσατο μὴ ἀποδιδοῦσι τοὺς φόρους, οὓς καὶ ὑποτάξας ἑαυτῷ πάλιν ἀνέστρεψεν. (At which time he undertook a campaign against the people of <Citium> who were refusing to pay their taxes, and having made them subject to him again, he returned home.)

[27] The name may be engraved on a bronze arrowhead of the 11th century BCE (Yon 2004, 36, n. 16), but the palaeographic dating of such items (cf. Naveh 1982, 37–40) is long in debate (see James, Kokkinos and Thorpe 1998, 29–30).

[28] Karageorghis and Demas (1985, 21–23, 141–163).

[29] Cypro-Geometric I conventionally starts at 1050 BCE (see James *et al.* 1991, 152–153, 158, Table 7:1; Iacovou 2008, 626, Table 1).

[30] Yon and Caubet (1985, 27–33); Yon (1997, 9).

[31] Cannavò (2015, 143–144) briefly reviews various theories.

[32] See Radner (2010).

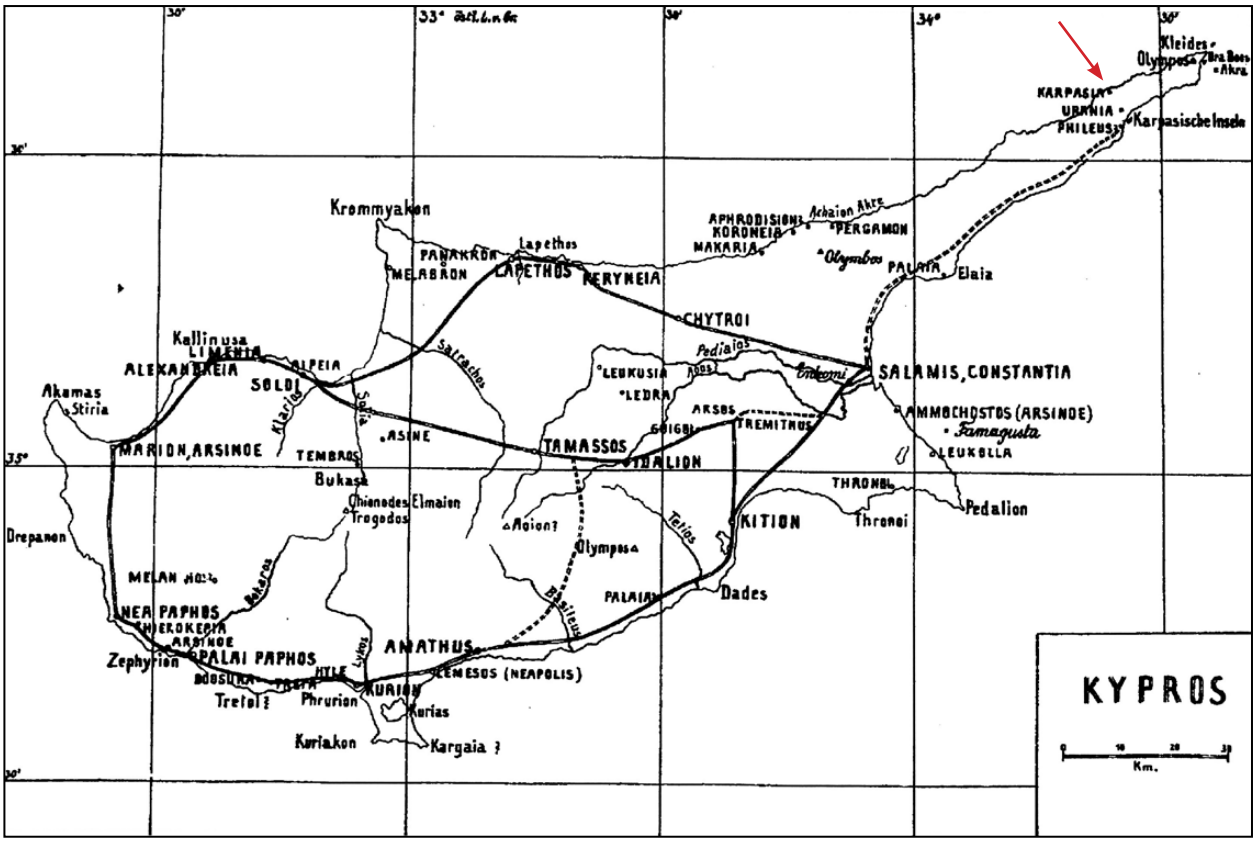

Figure 2. Map of Cyprus showing the location of Carpasia, see upper right (after Oberhummer 1924, 97–98).

who have proposed its radical downward revision, ending the period in the 10th century when Hiram I was reigning.

The second colony we hear about from the literary sources is Carpasia, which Strabo (14.6.3) located on the northern coast of the long, finger-like eastern peninsula of Cyprus (see *Figure 2*).[33]

Herodotus' older contemporary Hellanicus of Mytilene, in his lost *Kypriaka,* referred to Carpasia as having been founded by 'Pygmalion' (*FGrH* 4, F 57 = *apud* Stephanus, *s.v.* Καρπασία). As we know from the Tyrian Annals, linked to Assyrian chronology, Pygmalion reigned 814/3–768/7 BCE. The fragments we have do not mention the foundation of Carpasia, although they do refer to Pygmalion's sister's mission, as we saw. The question is, How could Hellanicus in the 5th century BCE have known about a Phoenician colony on Cyprus established by a king of Tyre at the end of the 9th or early 8th century BCE? Could Hellanicus have written his *Kypriaka* based solely on Greek traditions emanating from an island with long-shared past between Greeks and Phoenicians (*Hdt* 7.90)? Or could he have also known of Phoenician traditions, deriving from the Tyrian Annals before their translation in the Hellenistic period? Pseudo-Scylax (103) in the 4th century BCE clearly confirms the Phoenician origins of Carpasia, and Demetrius of Salamis later assumes the spelling of the name to have been 'Carbasia', associating it with the direction of the wind known in Phoenician as the *Karban (FGrH* 756, F 1). The 'Phoenician wind', *Karbas,* was already known to Aristotle (*De ventis* 937b 2–5; cf. Theophrastus, *On Winds* F 5, 62.5, ed. F. Wimmer).

After Citium, Carpasia and Qart-hadast, the fourth colony that surfaces is Lapithos. Pseudo-Scylax (103) calls it Λάπηθος Φοινίκων (Lapithos belonging to Phoenicians). The late Hellenistic writer Alexander of Ephesus (*apud* Stephanus, *Ethnica*, *s.v.* Λάπηθος) presents in mythical terms the Phoenician origin of Citium by saying that Βήλου δ'αὖ Κίτιόν τε καὶ ἱμερόεσσα Λάπηθος ('To Belos belong Citium and the charming Lapithos'). This corresponds at least with local numismatic evidence. Phoenician coins were being struck at Lapithos from the 5th century BCE, in the name of a king DMON[KS] ([דמון[כש]), understood to reflect the Greek name Demonikos known from an inscription of the 4th century BCE.[34] The fifth

[33] Not considered by Gjersad (1979); see map in Oberhummer (1924, 97–98).

[34] See Robinson and Stanley (1948, 60–65; 'Appendix: Kings of Lapethos'). The old numismatic corpora are out of date – see *BMC*, *Cyprus*, 29–31, and conveniently *HN*, 739.

colony that can be assumed to have existed is Urania/Aphrodisium, based on the cult (Astarte/Aphrodite Urania/'sky-goddess'/'heavenly one') introduced into Cyprus by the Phoenicians. In narrating how the Scythians marching by the city of Ascalon in 'Syrian Palestine', around 620 BCE, plundered the temple of Aphrodite Urania, Herodotus (1.105.3) says:

> ἔστι δὲ τοῦτο τὸ ἱρόν, ὡς ἐγὼ πυνθανόμενος εὑρίσκω, πάντων ἀρχαιότατον ἱρῶν ὅσα ταύτης τῆς θεοῦ: καὶ γὰρ τὸ ἐν Κύπρῳ ἱρὸν ἐνθεῦτεν ἐγένετο, ὡς αὐτοὶ Κύπριοι λέγουσι, καὶ τὸ ἐν Κυθήροισι Φοίνικές εἰσὶ οἱ ἱδρυσάμενοι ἐκ ταύτης τῆς Συρίης ἐόντες. (This temple, as I learned from inquiries, was the oldest of all those belonging to this goddess; for the shrine in Cyprus was founded from it, according to the Cyprians themselves, and the one on Cythera was founded by Phoenicians who came from this area of Syria.)

Urania Aphrodisium, which is referred to by Diodorus of Sicily (20.47) in the 1st century BCE, as much as by Strabo (14.6.3), among the early writers of the imperial period, continued to exist apparently to the end of the Roman Empire (Ptolemy, *Geogr*. 5.13.4; Nonnus, *Dionys*. 13.452).

Problems with Carthage in North Africa

Moving now west, and unlike the historical subjugation or re-foundation of Citium as a Phoenician colony on Cyprus in the early 10th century BCE, which, in the proposed downward revision of its chronology, does not present any conflict with the excavated archaeology of the place, however, the historical foundation of Qart-hadast/Carthage in North Africa in the late 9th century BCE does. The date of 808/7 BCE is too high when compared to the earliest finds excavated in Carthage. These finds belong to the oldest level (Tanit I) of the *tophet* (or precinct for sacrifices), situated in Salammbô south of the Byrsa hill (Carthage), followed by those in Junon, the most ancient cemetery, north of Byrsa. They consist of Euboic-Cycladic and Corinthian pottery, dated conventionally from the last quarter of the 8th and into the 7th century BCE.[35] Discussion has focused on a small vaulted chamber (initially called a 'chapel', but no more than an elaborate *favissa*, or a sacred refuse pit), seemingly cut into the bedrock under Tanit I and wishfully thought to predate it. This produced in its bottom, among other ceramic pieces, an askos in the shape of a bird, three oenochoe and three cotyles with geometric decoration, none really predating the finds of the oldest level. The same was the case with a foundation deposit, at the base of one of the enclosing

Figure 3. Pottery from the deposit in the Tophet at Carthage (after Aubet 1993, 195, Fig. 38).

walls of this vaulted chamber, producing a bowl-lamp in the Phoenician tradition with a single spout, and an egg-shaped amphora with twisted handles in the geometric decoration (see *Figure 3*).[36]

Despite original claims for extreme antiquity, these materials cannot defend a date for the foundation of Carthage in the late 9th century, let alone three centuries earlier! Besides, there is no evidence, historical or archaeological, that can support the presence of Phoenicians in North Africa and the West Mediterranean in the Late Bronze Age.[37] The limits in Carthage are set by the earliest local pottery, which is associated with material akin to Tyre III–II, by plentiful Greek pottery, and by a large number of Egyptian amulets and scarabs.[38]

[35] See conveniently, Aubet (1993, 190–196); Lancel (1995, 25–32).

[36] Lancel (1995, 241–245).

[37] See conveniently James *et al.* (1991, 365, n. 12); cf. Krahmalkov (1981, 177–191; on the Pygmalion text from Carthage); Aubet (1993, 179–181; on the Nora stele).

[38] James, Kokkinos and Thorpe (1998, 30); cf. Bikai (1978, 54–55).

In a way little has changed since the conclusion of William Culican some fifty-five years ago:

> Even if we grant that the pottery of the earliest deposit at the Precinct of Tanit at Carthage dates to the second half of the eighth century, the earliest tombs known from the Hill of Juno and Douimes, hitherto the earliest cemeteries at Carthage, and on Malta, cannot be placed before 710 on the system of dating adopted for Protocorinthian pottery, and are more likely to be placed in the first quarter of the seventh century.[39]

Attempts have been made to raise the beginning of the Proto-Corinthian pottery, but this could not exceed the 720 BCE mark.[40] More recently, and in order to 'resolve' the problem, attempts have concentrated on radiocarbon determinations from associated material. But nothing positive has come out of that. An honest, if diplomatic, statement by Roald Docter *et al.* regarding the ^{14}C dates from Carthage, implies that what we ought to be doing was to look into the limitations and deviations of the method, rather than encourage Greek archaeologists to consider (hardly any ever will) changing their pottery chronology:

> The inconclusiveness is, of course, frustrating, but we are not equipped to judge any possible technical limitations or inconsistencies (or rather consistent deviations) inherent in the radiocarbon method involved. In an earlier joint study on the contents of six urns from the Carthaginian tophet in Leiden and Amsterdam, the radiocarbon determinations proved to be very successful, falling well within the proposed date ranges based on typological comparisons of the individual tophet urns. What we are able to judge are the archaeological data. Here, we see no reason, for the time being, to drastically alter the paradigms of archaeological dating. This does not mean that we oppose an upgrading of archaeological dates for some classes (as for example the skyphoi with one bird and metopes that are attested in Carthage and are now seen to start already by the end of Middle Geometric...and, hence, for the foundation of Carthage. What we do not wish to do is have such a re-dating based solely upon the present Carthaginian stratigraphy. In presenting the archaeological material from the contexts from which the early radiocarbon dates came, we hope to have contributed the pillars on which a future bridge may be constructed.[41]

Similar is the pattern with the Western Mediterranean and the claims for higher ^{14}C dates, for example from a mixed deposit at Huelva in Spain, in an effort to bring Phoenicians here in the first half of the 9th century or even earlier![42] But nothing provides confidence for the method itself and its manipulative statistics in this period. Subsequent thoughts, assessing the entire period from the Late Bronze to the 8th century BCE, have preferred to interpret early finds (in terms of conventional chronology) as being connected to the indigenous people rather than to the Phoenicians, for example in the region of Valencia:

> Cabezo Redondo, in Villena, must have been a crucial settlement for the political economy in the area between the 14th and 11th centuries. It is a key site for analysing the interplay of interactions between different Mediterranean areas, and between the sea and inland territories. Not far from the settlement, a hoard of gold and silver objects was discovered in the 1960s. The hoard was found isolated in a ravine, but it is thought to be related to the settlement of Cabezo Redondo. The hoard is made up of vessels – mainly gold bowls and silver and gold bottles – and personal items like bracelets.... Overall, they weigh around 9kg. The detailed study of the technology and typology of the objects has revealed different influences in their composition. These scholars highlight the oriental influence in the technological choices in the production of some objects, namely the use of rivets and wheel-based tools for the production of these bracelets. However, the decorative pattern of the bowls and the typology of the bracelets and bottles, remain local, which betray an interesting mixture in the transmission of know-how, while retaining local choices. There are a couple of small iron objects included in the hoard that have raised a lively debate about its chronology since the time of the discovery, because to some scholars, the presence of iron objects in the deposit must have been related to the Phoenician trade – thus implying a 'low' chronology, ca. the 8th century. This interpretation was based on a fundamentally flawed assumption: that the Phoenicians were considered the first people to have sailed long distances around the Mediterranean, that they were the only people responsible for the dissemination of iron metallurgy or novelties, and that local groups were not capable of

[39] Culican (1970, 32).

[40] See James *et al.* (1991, 101, Table 5:2), where relative chronology between Attic and Corinthian pottery during the Late Geometric and Archaic periods.

[41] Docter *et al.* (2008, 417).

[42] Nijboer and van der Plicht (2006).

> conducting exchanges. These assumptions have long been challenged on the basis of a reconsideration of the role of the local groups in the exchanges – as being capable of cabotage seafaring – and the recognition of the existence of more contexts with iron dated before the 8th century. It is clear by now that the first iron objects were already in circulation around the Mediterranean between the 12th and 11th centuries: bracelets, small iron objects and little knives have been recorded in Cyprus, Greece and the Levantine coasts in the 12th century, in Sardinia between the 13th and 12th centuries, and on Menorca, in the Balearics, around 1000 BC. And they also reached the shores beyond the Straits before the 11th century, as revealed by the finds in Portuguese settlements like Monte do Frade, Moreirinha, Monte do Trigo and Quinta do Marcelo. This picture in which the circulation of few iron items were restricted to elite contexts is admittedly in clear contrast with the evidence available for the 8th century, when iron metallurgy – and not exchanged objects – is attested for the first time in Iberia. The evidence comes from several Phoenician settlements: Fonteta, Cerro del Villar, Toscanos, or Morro de Mezquitilla, and indigenous settlements like Castellar de Librilla with connections to the new networks....[43]

Conclusions and the Pitfalls of ^{14}C Dating

In conclusion, the first Phoenician colony west of Tyre was that of Citium in Cyprus, early in the reign of Hiram I (955/4–922/1 BCE), or possibly late in the reign of his father Abibaal. The second Phoenician colony on the same island was that of Qart-hadast, most probably located at Amathus. It was founded in 808/7 BCE, in year 7 of Pygmalion, by his sister Elissa, and attested under Hiram II, in *c.* 741/40–735/4 BCE. Centuries later, in reading the Tyrian Annals, which mentioned Qart-hadast, and not knowing of its existence in Cyprus, since its Phoenician name had been dropped, Josephus (or his main source Menander) had to assume automatically that the reference was to the famous colony of his time in Libya. This solves the problem of the foundation of Carthage, which must be dated some considerable time after that of its mother city in Cyprus. We cannot know precisely when, but if there is any kernel of truth in the story of Trogus, it would have been either within the lifetime of Elissa, perhaps two to three decades after 808/7 BCE, or probably later within the lifetime of the affiliated priest's family. It could well have been a later generation of that family that moved to North Africa for the first time, bringing the date of the foundation very close to the earliest archaeology in Carthage, dated to the last quarter of the 8th century BCE.

It is worth noting that the craze for higher ^{14}C dates in the period concerned, has recently extended surprisingly to Greece – that is to the island of Andros – where the raising of the chronology of the Greek Geometric pottery has been attempted![44] This follows the problematic Gordion claims,[45] as well as the Nineveh ^{14}C fiasco, where the historically established destruction of the Assyrian capital by the Babylonians and Medes in 612 BCE, was found to be radiocarbon dated to *c.* 795 BCE, about 180 years earlier![46] The serious clash with the Assyrian chronology had been predicted long time ago by the present writer in a team's paper:

> The only alternative envisaged is raising dates for the later Iron Age, a possibility for Greek Geometric chronology tentatively suggested by Renfrew in the Foreword to our book. Cross has already suggested raising the dates for the Protogeometric in Greece on the basis of the Egyptian dating for the Levantine alphabet.... Desborough considered the effects which an acceptance of Levantine dates would have on the chronology of Greek pottery, but rejected them as preposterous.... Indeed they are; raising the chronology would create a new Dark Age during the Archaic period, contradicting every available historical source, beginning with Thucydides. In the long run the unchallengeable dates of Assyrian history back to the late tenth century BC would have to be rejected![47]

With naïve confidence the team analysing the radiocarbon results from Andros, declared:

> Bayesian modelling suggests that the Late Geometric I period at Zagora started no later than 935–850 BC. Even if the Bayesian modelling were discounted, Late Geometric I ceramics from two secure deposits at Zagora were found in context with animal bones whose non-modelled dates at 95.4% confidence date no later than the third quarter of the ninth century BC.[48]

To raise the beginning of Late Geometric from the current 760 BCE mark to somewhere between 935–850 BCE, while keeping its end at 700 BCE (a date that actually needs to be lowered well into the 7th century),[49] is to extend this brief and poorly-represented pottery period

[43] Sánchez (2015, 281).

[44] Alagich *et al.* (2024).

[45] Manning and Kromer (2011).

[46] See conveniently Porter (2015, 229–231), and Zerbst and van der Veen (in these Proceedings, pp. 373–399).

[47] James *et al.* (1992, 129).

[48] Alagich *et al.* (2024, 13).

[49] James *et al.* (1991, 111); cf. Morris (1998, 362).

from 60 years to up to 175 years! This not only would make Nicolas Coldstream turn in his grave, but it would make nonsense of any study of Attic workshops.[50] Not to mention the bones of animals from secure deposits, living and feeding near the sea of an island absorbing old carbon, there is ignorance here of pottery chronology and historical interconnections. This is science fiction at its best, based on inventive statistics, and as Mark Twain once wrote: 'There are three kinds of lies: lies, damned lies and statistics.'[51]

Abbreviations

ANET	Pritchard (ed.) 1969
BMC	Hill 1904
CIS	Corpus Inscriptionum Semiticarum
FGrH	Jacoby (ed.) 1923–1958
Hdt	Herodotus
HN	Head 1963
KAI	Donner and Röllig (eds) 1962–1964.
KB	Schrader 1890
TSSI	Gibson 1982

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[50] See for example the old study of Davison (1961, 125–126), who allowed no more than 100 to 150 years for the development of the entire Geometric style (Early, Middle and Late) – thought to extend over 200 years (900–700 BCE).

[51] Attributed by Mark Twain, apparently wrongly, to Benjamin Disraeli – see Twain (1907, 471).

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‘Late Assyrian-Style’ Seals, Bullae and Pottery as Chronological Markers for Dating Iron Age IIB*

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In this article ‘Late Assyrian-style’ pottery and seal evidence will be presented which appears to support the idea that the conventional dates for Iron Age IIB in Israel and Judah (c. 800–732/700 BC) are too high by several decades. Many scholars date the end of Iron Age IIA to c. 800 BC and the end of Iron IIB to c. 732/722 BC (in Israel) and to 701 BC (in Judah and the Negev) respectively. This was the time when Tiglath-pileser III and Shalmaneser V (c. 734–722 BC) campaigned in the region (and annexed the Northern Kingdom of Israel) and later Sennacherib, when he invaded the Southern Kingdom of Judah. Yet the ‘Late Assyrian-style’ pottery found in the relevant archaeological strata calls for lower dates, as the vessels are almost exclusively local imitations made by local pottery workshops. While similarly shaped genuine ‘Assyrian Palace Ware’ prototypes were introduced in the Assyrian heartland during the Sargonid era (c. 721–612 BC), imitations should not be expected to have been produced much before the seventh century BC. The demand for ‘Assyrian-style’ imitations certainly would have increased in the remoter peripheries of the empire only when the region flourished during the Pax Assyriaca*. Solar imagery, as found on stamp seal impressions on clay in Iron Age IIA–IIB layers in Jerusalem appears to corroborate this very same observation.*

Introduction

This article will deal with so-called ‘chronological markers’ which could help to fine-tune Iron Age IIB chronology, mainly so in Cisjordan. The markers concern ‘Late Assyrian-style’ pottery and small items (mainly seals and bullae) from the Neo-Assyrian period. These objects would have reached this remote area on the southwestern periphery of the empire only when the Neo-Assyrians invaded the region and when it began to flourish during the heyday of the so-called *Pax Assyriaca* period (i.e. during the late eighth and seventh centuries). It would also have been from this point onwards that local potters would have begun to imitate ‘Late-Assyrian’ pottery, including so-called ‘Assyrian Palace Ware’ styles. While many such items are found in late Iron Age strata of the seventh and sixth centuries BC, some were retrieved from earlier Iron Age IIB levels, which have traditionally been dated to the eighth century BC. Some of these strata even predate the period of the Assyrian hegemony within the traditional framework. The present author therefore will seek to find answers to this thorny issue by asking if perhaps the conventional dates of Iron Age IIB must be revised in accordance with this evidence. In the following we shall study a ‘Late Assyrian style’ stamp seal from Megiddo (from Stratum VA–IVB), three ‘Late Assyrian-style’ stamp-seal-impressed clay bullae from the ‘City of David’ in Jerusalem (contemporary with Yigal Shiloh’s Stratum 13) and imitation ‘Late Assyrian-style’ pottery from Megiddo (Stratum IVA) and Samaria (apparently from Building Phase ‘V’), and from Tel Beersheba (Strata III–II), Tel Arad (Strata X–VIII) and Tel Aroer (Strata IV–IIb) in the ‘Beersheba-Arad-Valley’ located on the southern border of the Kingdom of Judah.

1. Stratified ‘Late Assyrian-Style’ Sigillographic Evidence as Markers

1.1 The stratified lion seal of ‘Shema, the servant of Jeroboam’ (šmᶜ ᶜbd yrbᶜm)

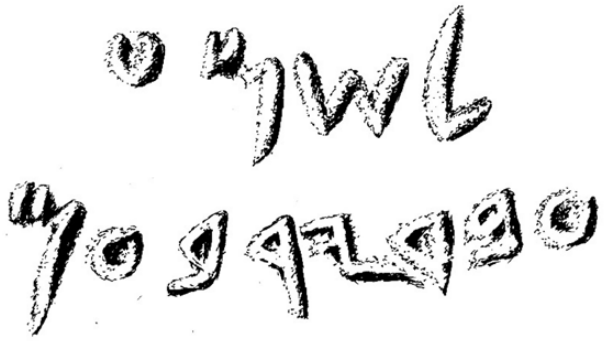
A figured and inscribed jasper scaraboid stamp seal without perforation (*WSS* 2) was excavated during 1904 in Megiddo by German architect-archaeologist Gottlieb Schumacher. It is the largest stamp seal by far ever found in the Levant, measuring 37 x 27 x 17.55mm (*Figure 1*). Its finely modeled details portraying a roaring lion display high-quality workmanship. Although the seal was sent to Constantinople/Istanbul soon after its discovery, in then Ottoman-period Palestine

* This summary article is based mainly on the author’s postdoctoral research (van der Veen 2020; but see also van der Veen 2014).

(its current whereabouts may have been solved [1]), gypsum casts were produced, whose informative value clearly superseded the photographs made of the seal by the original team (Schumacher 1908, 99). Based on these casts and on descriptions found in the excavation reports (Schumacher 1908; Watzinger 1929), a reliable reconstruction of its iconographic and palaeographic facets could be made by the present author. As has been discussed at length elsewhere (van der Veen 2020, 26–63; 2021, 207–223), the Shema seal clearly holds the potential of being an important chronological marker for the end of Iron Age IIA, as its archaeological circumstances have been clearly reported by the original excavator. While earlier archaeologists dated the end of Iron Age IIA to the last quarter of the tenth century BC (allowing Shema to have served king Jeroboam I; on this see Yeivin 1960 and Ahlström 1993), the style of the seal and its inscription rather suggest a date not before the early eighth century BC. Moreover, virtually identical and similar seals were found in eighth and seventh century strata in Assyria and its peripheries, suggesting that the king involved could only have been Jeroboam II (790/89–750/49 BC, after Galil 1996) and that the destruction layer from which the seal was retrieved (i.e. Stratum VA–IVB) terminated sometime during the eighth century BC.

Figure 1. A reconstruction of the mirror image of the Shema lion seal as based on the photographs of the gypsum impressions and on the descriptions by Gottlieb Schumacher (line-drawing by the author).

The epigraphic evidence: the inscription is divided into two registers, one above (reading *lšmᶜ*, i.e. 'Belonging to Shema') and one below the lion (reading *ᶜbd yrbᶜm*, i.e. 'servant [in the sense of courtier] of Jeroboam', see *Figures 2a–b*). By closely scrutinising photographs made of the gypsum impressions (which proved to be more reliable than the bronze cast, which is mostly shown, i.e. IAA 1947-5396[2]), four letters qualify for palaeographic dating (i.e. *mem*, *ᶜayin*, *dalet* and *yod*). The two *mems* in the inscription (one each in the upper and lower registers) slightly differ. While the zigzag head of *mem* in the upper register represents the more archaic form (as is for instance found on the Mesha Stele), the head of *mem* in the lower register (with two small vertical strokes placed to the left of the vertical stem) is a slightly more developed form. Both forms however are attested during the eighth century, as can for instance be seen on a jar handle from Tel Dan Strata 3 or 2 (*WSS* 692 A–C) and on the so-called Shemaryo seal, allegedly found at Samaria (*WSS* 377).



*Figures 2a–b. top: the inscription in the upper register of the Shema seal (*lšmᶜ*); bottom: the inscription in the lower register of the Shema seal (*ᶜbd yrbᶜm*; line-drawings by the author).*

ᶜAyin, which is found three times on the seal (once in the upper and twice in the lower registers) appears twice to be somewhat oval-shaped, and if correctly interpreted, this form finds good parallels on eighth century seals. For instance, it can be seen on the Judahite seal of ᶜAbdiyo, a courtier of king Uzziah (*WSS* 4), as well as on a Phoenician seal found at Khorsabad (*WSS* 743). The triangular shape of *dalet* (found only once in the lower register) appears to contain a short leg. This form is the successor of the completely triangular 'legless' *dalet* as we still find it on the Gezer Calender and on the Mesha Stele (Renz 1995: III, Tables 2–3). Short legged *dalets* are attested in several late ninth and eighth century inscriptions, including the Aramaic Tel Dan Stele, the Ammonite Amman Citadel inscription, as well as in

[1] According to Ariel Berman, who met G. Schumacher's daughter Nellie Marcinkowski (1896–1991) in 1966, the seal, which had first been dispatched to the archaeological museum in Istanbul, was subsequently buried with other burial goods in the tomb of Sultan Abdul Hamid II, when he died in 1909. See Deutsch *et al.* (2024, 136–149).

[2] The bronze cast has proved to be a copy of a copy. Its details do not always match those of the old gypsum impressions, i.e. those in Schumacher (1908, 99) and Watzinger (1929, 65, Fig. 62) nor of the copy made for Dr Baroody, shown in the Lebanese newspaper al-Mashrih of May 15, 1904, which was also published by Cook (1904, 299, Fig. 3).

*Figures 3a–c. Left and centre: plasticine impressions of the Rapati seal from Khorsabad (*WSS *843) and the unexcavated ᶜAshna'el lion seal (*WSS *1169); right: the anepigraphic lion seal from Tell en-Nasbeh (left and centre: courtesy of Prof. B. Sass; right: reproduction with kind permission of the Israel Antiquities Authority, photo by Clara Amit).*

several inscriptions from Kuntillet ʿAdjrud (Renz 1995, III, Tables 2–3). This type is also found on Judahite and Aramaean seals (e.g. *WSS* 3, 752–753). Palaeographically, the letter *yod* (found only once in the lower register) is likely the most characteristic letter on the seal, whose wavy tail bends downwards, a feature which was already noted by Emil Kautzsch (1904, 3). This type is found for instance at late ninth to early eighth century Kuntillet ʿAdjrud (Ahituv *et al.* 2012, 90, 98, 100–102), as well as on the eighth century ostraca from Samaria (Renz 1995: III, Tables 6 and 9). It may also be seen on the Shemaryo seal said to be from the same site (*WSS* 377).

The glyphic details: the central scene is filled with an imposing roaring lion (see *Figure 1*). The fierce feline features of its agape jaw with sharp teeth, the muscular bare shoulder, the paws and the upcurved tail, clearly remind us of similar lion representations in Neo-Assyrian hunting scenes among the palace wall reliefs of Aššurnaṣirpal II (883–859 BC) at Calah and of Assurbanipal (668–627 BC) at Nineveh (van der Veen 2020, 37–40, with Figs. 15a–d). Similar lions can also be seen on other Neo-Assyrian objects, such as on seal impressions (e.g. Herbordt 1992, 34–36), glass and alabaster bowls (e.g. Muscarella 1965, 41–46; Curtis and Reade 1995, nos. 115–117), bronze plates (e.g. Frankfort 1956, 172–173) and bronze weights (e.g. Albenda 1974, 3, 15, Fig. 6). The same style was employed by later Neo-Babylonian and Achaemenid artisans, as can for instance be seen on stamped bricks from Babylon (Sass and Marzahn 2010). It has however been suggested that the representation of the fierce lion on the Shema seal is merely 'Assyrianising' in style rather than truly Assyrian, as it was only an interpretation of that style by local artisans. Indeed, many such depictions of lions are found on inscribed Aramaean seals, albeit mostly of unknown provenience. Yet, one such lion seal was excavated in Khorsabad (*WSS* 843), Sargon II's ancient capital Dur-Sharrukin (founded in 713/12 BC). The name of its seal owner is given as 'Rapati.'[3]

The vertical stance of the letters indeed suggests that the engraver used an Aramaic or Aramaising script (Bordreuil 1986, no. 89; Lemaire 1979, 67–69; see *Figure 3a*). The inscription is situated between the legs of the feline creature. Regardless of whether the Shema seal was made by an Aramaean, or an Israelite artisan employing an Aramaean style, other lion stamp seals – albeit anepigraphic – have been uncovered at Israelite and Judahite sites, including Megiddo, Tell en-Nasbeh (see *Figure 3c*) and Jerusalem, all dated to the eighth to sixth centuries BC, when 'Assyrianising' styles were common place in the Southern Levant.

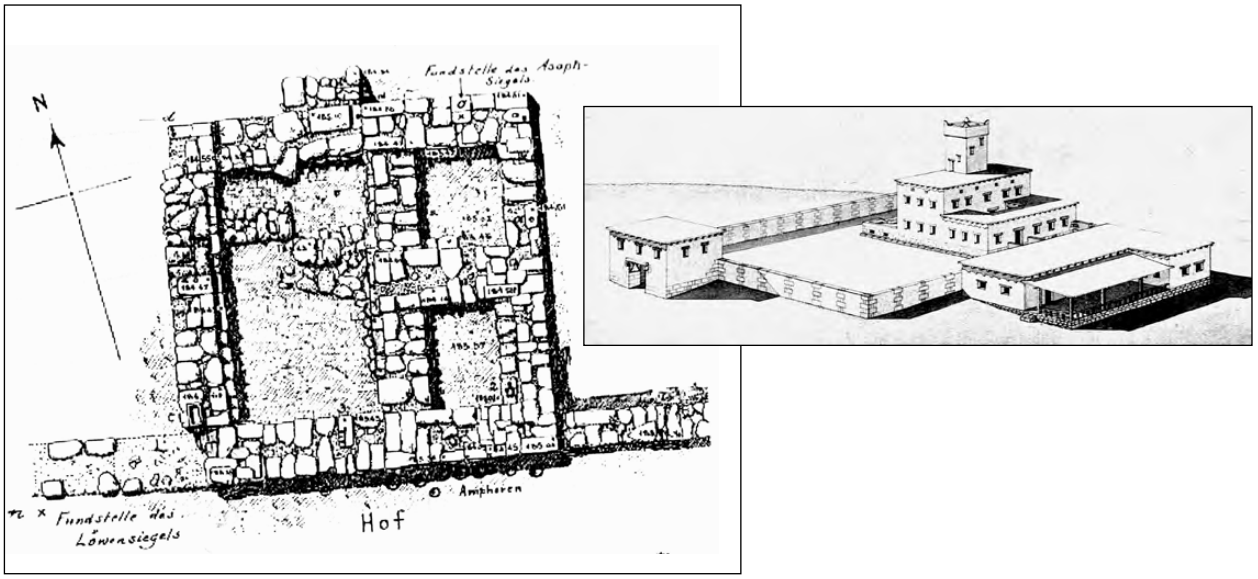
Two more incised secondary ('filler') motifs were originally identified on the Shema seal by the excavators and are described by Schumacher (1908, 100) and his epigrapher Emil Kautzsch (1904, 3). In scholarly works, these incisions have largely escaped notice, as was also noted by Lawrence Mykytiuk (2004, 133) and Brent Strawn (2005, 102). They were apparently so lightly scratched or etched into the surface of the seal's base (suggesting perhaps that the seal was never completed) that they cannot be recognised on the gypsum impressions, let alone on the bronze casts subsequently made of these impressions. Even so, Schumacher clearly describes them:

> Near the jaw of the lion a palmaceous tree and to the right of its tail a sign like the hieroglyph *ᶜankh* ☥ was scratched or etched into the surface with very fine lines, but these seem never to have been finalised. (Schumacher 1908, 100)[4]

These filler motifs are visible in the photograph on the inner front cover of Schumacher's 1908 report. It seems, however, that they were added by an artist based on Schumacher's observations. A reconstruction, highlighting them is reproduced above (see *Figure 1*). Strikingly, Egyptianised symbols, as well as floral motifs, are regularly found as associated icons alongside 'Assyrianising' lions on epigraphic and anepigraphic seals from the eighth to sixth centuries BC. For

[3] This name is possibly related to the toponym *ru-ub-tu* (Bordreuil 1986, 78).

[4] Translation from the German by the present author.



Figures 4a–b. Left: the original plan by G. Schumacher with findspot 'x', where the Shema seal was found (after Schumacher 1908, Pl. XXIX-A); right: a reconstruction of Gatehouse '1567', enclosure wall '1610' and southern Palace '1723' (after Lamon and Shipton 1939, Fig. 29, courtesy of S. Allison of the Oriental Institute in Chicago, IL).

instance, the Rapati seal from Khorsabad also portrays an Egyptian symbol, this time a winged scarab beetle placed over the back of the lion (*Figure 3a*). The unexcavated ᶜAshna'el seal (*WSS* 1169; see *Figure 3b*), albeit known since the mid-nineteenth century (de Vogüé 1868, no. 8), has a winged scarab beetle in the register below the lion. *ᶜankh*-signs are found on several other unprovenanced inscribed seals (for instance: *WSS* 391[5], 1129[6]; Deutsch and Lemaire 2000, 135[7]), while it can also be seen behind the hind legs of the lion shown on the seventh century BC provenanced anepigraphic seal excavated at Tell en-Nasbeh/Mizpah (McCown 1947, 149–150, 296, Pl. 55: 74; see *Figure 3c*).[8] As on the Shema seal, a floral motif is depicted to the left of the creature. This seal too dates to the Neo-Assyrian period. A more schematic lion with a zigzag (floral?) motif behind its legs, was found in the so-called 'Babylonian grave' at Megiddo, dated to the seventh or sixth century BC (Deutsch 2000, 426–427). Lions are also depicted on jar handles excavated in Hazor (Stratum VA) and Ramat Rahel (Stratum VA).[9] Hence, the mixing of Egyptianising and Assyrianising symbols is a typical feature of Levantine seal production, especially so during the Late Assyrian period of the eighth and seventh centuries BC (for a brief discussion of this, see van der Veen 2020, 41–43).

The stratigraphic evidence: the circumstances concerning the discovery of the Shema seal as described by the excavator have led the present author to suggest that Iron Age IIA did not terminate, at least not at Megiddo (and presumably not in its environs) much before the first half of the 8th century BC. While some scholars (including Ussishkin 1994, 421) have repeatedly argued that the find-context of the seal is ambiguous, this is not supported by the information the present author was able to retrieve from both Schumacher's and Watzinger's reports. Schumacher assigned the Shema seal to his Stratum 'V' (Schumacher 1908, 99), which classical archaeologist Carl Watzinger assigned to the ninth and eighth centuries BC.[10] The fact that the seal was found in the debris of this stratum, and because the seal was apparently never completed (see above), it may be surmised that it belonged to the final stages of that stratum.

The seal was found on March 22 of 1904 in the southern remains of what would later become known as the four-

[5] As on the Shema seal, the *ᶜankh* is placed behind the lion, while in front of the creature the head of a gazelle is portrayed.

[6] Here two *ᶜankhs* flank the winged scarab beetle placed over the back of the lion.

[7] Here an *ᶜankh* is placed in front of the lion.

[8] Found in Square AJ22 in Silo 170. Based on a similar schematic lion on a seal from the 'Babylonian grave' in Megiddo (van der Veen 2020, 44, Fig. 19a; Deutsch 2000, 426–427), it was likely found in Stratum 3 at Tell en-Nasbeh (also pers. comm. with J. Zorn, 2015).

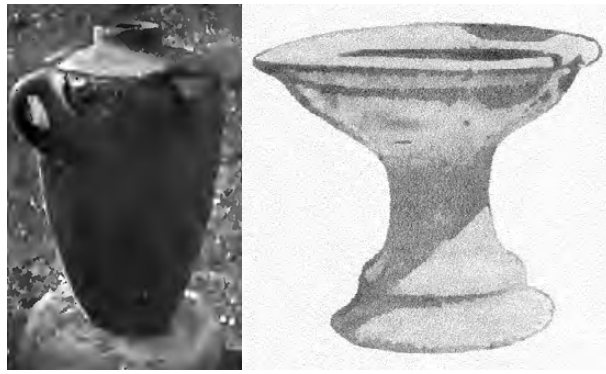
[9] Van der Veen (2020, 44). For a seventh century date for Hazor Stratum VA, see James (2008, 137–183).

[10] His conclusions were based mainly on the close resemblance between the Stratum 'V' structures at Megiddo and the earliest building activities at Samaria, uncovered by George Reisner between 1908–1910 (Reisner *et al.* 1924). His 'Low Chronology'-dates would be corroborated later by Kathleen Kenyon's pottery analyses at Samaria, who excavated there during the 1930's (van der Veen 2023, 177–178; Niemann and Lehmann 2006, 692–694; Chapman 2015, 141–142).

chambered gatehouse '1567' of the Oriental Institute of Chicago, when their team discovered the hitherto unexcavated western chambers of that same building (see *Figures 4a–b*).[11] Gatehouse '1567' allowed access to the royal courtyard '1693' of the palace structure '1723.' According to Schumacher's description, his team had discovered the seal 'by the wall' (i.e. 'hart neben der Mauer'[12]), which, based on the Chicago excavations, was the ashlar pier that had linked the southwestern corner of gatehouse '1567' with enclosure wall '1610' of the palace courtyard (Lamon and Shipton 1939, 12-17; van der Veen 2021, 209, n. 7). Using modern estimations, the seal was found approximately 1.5m above the courtyard's floor and not actually on that floor.

This has led to some confusion among scholars, as if perhaps the seal had been found 'out of context' (Watzinger 1929, 64; Ussishkin 1994, 415–417, Figs. 24–1, 2, 3; van der Veen 2020, 51, n. 138). And despite a seemingly inconsistent statement made by Schumacher about the height of the wall[13], Watzinger's descriptions of the debris of the gatehouse, found on the same level with the seal, render it probable that the seal had in fact derived from the *upper* room of the two-story gatehouse (Watzinger 1929, 59). Here Schumacher encountered a thick layer of 'brick powder and brick pieces, mixed with burned pieces of wood', boulders and straw, material that had evidently derived from the roof and floor of the upper room of that same building.[14] While Daniel Frese considers 'the existence of at least one upper floor in a gatehouse' to be now 'almost universally accepted', he discusses parallels for layers of debris such as at gatehouse '1567' at other Iron Age II gatehouses, including at Hazor, Gezer, Lachish, Khirbet Mudayna and Beth Saida (Frese 2012, 103–104; 2015, 75–92). Especially at Beth-Saida 'chunks of the second-story floor' were found at 'about 1m above the floor.' This material was found mixed with 'broken pottery jars from the upper story' (Frese 2012, 103–104). The situation corresponds precisely with the find-context of the Shema seal as described by Schumacher and his team.

Datable Iron Age IIA and IIA–B ceramics were also found on the same level with the seal as listed by Carl Watzinger (for two specimens, see *Figures* 5a–b). These include for instance a 'cylindrical two-handled storage jar with bulbous knob' (with parallels at late Iron Age I–Iron Age IIA Megiddo and at contemporary sites including Tel Qasile [type SJ 3] and Tel Dor [horizons Ir 1a and b]), a 'handleless holemouth jar with virtually straight sides and a round base' (attested at Iron Age IIA–B Tell Jemmeh Phase KB1, Tell Farᶜah North Str. VIIb, Tel Rehov Str. IV and Hazor Str. VII), as well as an 'incense burner with an everted rim and a carinated base' (attested throughout the Iron Age IIA, for instance at Megiddo Str. VB, Tel Rehov Str. V and IV, Tel Amal III).[15]



Figures 5a–b. Whole forms found in association with the Shema seal in Stratum VA–IVB. Left: 'cylindrical two handled storage jar with bulbous knob'; right: 'incense burner with an everted rim and a carinated base' (after Schumacher 1908, 101).

From the precise description of the destruction debris and the pottery found in the same level with the Shema seal, it appears sufficiently clear therefore that the seal indeed had belonged to Stratum VA–IVB (as Schumacher believed) or to the very beginning of Str. IVA (as suggested by William F. Albright and Yigael Yadin: van der Veen 2021, 212, n. 12). As the seal had been carved for an official of king Jeroboam II, whose reign ended in *c.* 750/749 BC, Iron Age IIA at Megiddo can hardly have ended much before the middle of the eighth century BC. Watzinger even suggested that the

[11] Schumacher himself had originally believed that the remains had belonged to what he called the 'Palastwohnung'.

[12] The German expression indicates that the seal was found *by* the wall and not *c.* 1m away from the wall, as Ussishkin has argued (1994, 421).

[13] Ussishkin questions Schumacher's claim that the wall had reached a height of *c.* 2m in this area and that its upper courses were reached *c.* 60cm below the surface (Ussishkin 1994, 421, Fig. 24–1). Having carefully restudied the excavations reports, photos and plans myself, it may now however be concluded that Schumacher never claimed to have found an intact wall at this height, but simply that 'upper stone courses of this wall' ('die oberen Steine dieser Mauer', Schumacher 1908, 99) and 'layers of stone above the walls of the palace' ('Steinschichten über den Mauern des Palastes', Schumacher 1908, 96) had been uncovered in the excavation (van der Veen 2020, 52–56; 2021, 211). While loose boulders can be seen protruding from the debris at about this height in Schumacher's figure 139 (1908, 95), 'the eastern pier of the southwestern chamber did reach this height' (van der Veen 2021, 211).

[14] Watzinger describes the situation as follows: 'eine Schuttmasse von Ziegelmehl und Ziegelstücken, die mit verbrannten Holzstücken untermischt war und die nach oben in eine Schicht von verbranntem Holze überging' (1929, 59).

[15] For a detailed discussion of the pottery found with the seal and the parallels attested at contemporary sites, see van der Veen (2020, 56–59).

'palace-building' (i.e. gatehouse '1567' of the Chicago team) could have been destroyed during the Neo-Assyrian conquest in *c.* 730 BC (Watzinger 1929, 67).

1.2 Assyrian-style bullae from the Rock-Cut Pool in Jerusalem (Silwan Village)

During the excavations by Ronny Reich and Eli Shukron at the 'City of David' (Silwan village), in the so-called 'Rock-Cut Pool' in Area C, many anepigraphic seals and bullae were uncovered, along with vast amounts of pottery sherds, fish bones and other small finds (Reich 2011, 206–219; de Groot and Fadida 2011, 159). The seals and bullae were subsequently published by Othmar Keel and his colleagues (Keel 2017, 340–409, nos. 138–187). According to the excavators, the 'Gihon Rock-Cut Pool' was 'filled in' with material from the nearby slopes of the 'City of David' to block the water reservoir, when a new tunneling system was being constructed, presumably sometime during the reign of king Hezekiah (726–698/7 BC, Galil 1996, 95, 106). Consequently, the old access to 'Tunnel 4' and the associated older tunneling system, located at the bottom of the Pool, was 'blocked' (Reich and Shukron 2011, 147–157). While previously scholars had assigned the older water system to the time of Hezekiah, the pottery retrieved from the fill (contemporary with Stratum 13[16]), however, suggests a somewhat earlier date, if we accept the traditional chronology. Accordingly, the excavators assigned the filling in of the Pool to no later than the early eighth century BC. Yet, Lily Singer-Avitz has questioned their early date, suggesting that the latest pottery in the fill belongs to Iron Age IIB.[17] Be this as it may, some pottery types, characteristic of the so-called 'Lachish Level III horizon' (i.e. contemporary with Stratum 12 at the 'City of David') are missing in the fill, including sherds from closed cooking pots, molded rim cooking pots and royal storage jars (of the so-called 'Olga Tufnell type-484' jars), as Alon de Groot and Atalya Fadida have indicated.[18] Moreover, as the pottery from underneath the newly constructed house and from the southern and eastern edges 'above the Pool' clearly belongs to Iron Age IIB (while the extramural quarter in this area was abandoned no later than early Iron Age IIC[19]), it seems reasonable to conclude that the filling in of the Pool occurred sometime during the initial phase of Iron Age IIB. This would have been *c.* 740/730 BC according to the low chronology envisaged by Ze'ev Herzog and Lily Singer-Avitz (2004, 230, 236; 2015, 214, n. 8).

But bullae with typical Late Assyrian-style solar representations also found in the Pool's fill, suggest a date some decades later, when Assyria exerted a direct control over the region. This would have likely been either during the reign of Sargon II (who marched against Ashdod and Gath in 711 BC) or perhaps more likely so, during the reign of Sennacherib, who invaded the Judaean Shephelah and laid siege to Jerusalem in 701 BC and subsequently imposed heavy tribute on Hezekiah, while the latter's son Manasseh (698–642 BC) remained a loyal vassal to Assyria during most of the years of his long reign.[20] Plausibly, the Pool could well have been blocked during the reign of Hezekiah prior to the Assyrian siege, when according to biblical tradition Hezekiah blocked the Gihon spring to prevent the Assyrians access to the city's water system. For we read in 2 Chronicles 32: 2–4, 30:

> And when Hezeki'ah saw that Sennacherib had come and intended to fight against Jerusalem, he planned with his officers and his mighty men to stop the water of the springs that were outside the city; and they helped him. A great many people were gathered, and they stopped all the springs and the brook that flowed through the land, saying, 'Why should the kings of Assyria come and find much water?' ... This same Hezeki'ah closed the upper outlet of the waters of Gihon and directed them down to the west side of the city of David.

Besides seal material from the Middle and Late Bronze Ages, many seals and bullae from the 'Rock-Cut Pool' show typical Iron Age II motifs. They include Egyptianising pseudo-cartouches, uraei, vultures and

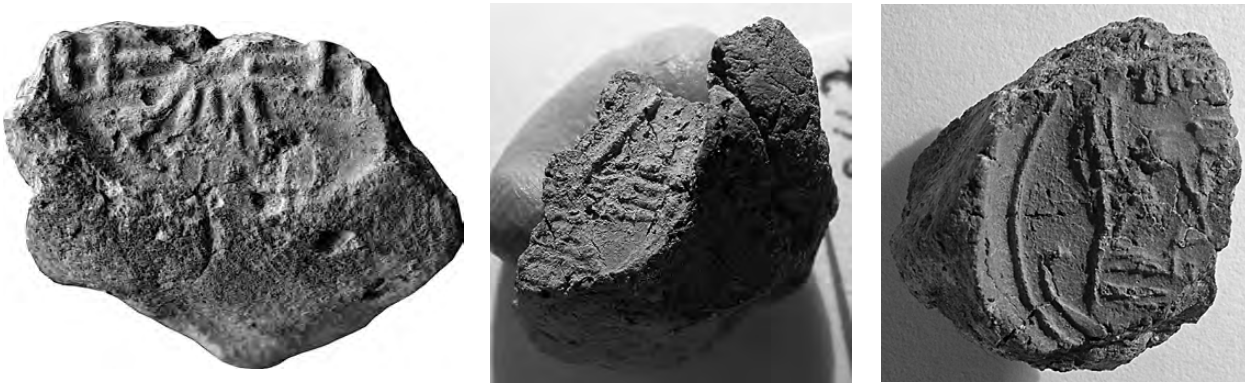
[16] As the excavators argue, the latest pottery in the fill covering the entrance to 'Tunnel 4' at the bottom of the Pool, belongs to late Iron IIA (contemporary with Stratum 13 in Area E1 of the former Yigal Shiloh excavations), while the pottery below the newly built house atop the fill dates to Iron Age IIB (Reich 2011, 217). Concerning the water system to the south of Area C, see Reich and Shukron (2021). According to Alon de Groot and Hannah Bernick-Greenberg, who published the pottery of Stratum 13 in Area E1, this material indeed predates Stratum 12 (i.e. the main Iron Age IIB stratum at the site). See de Groot and Bernick-Greenberg (2012, 199, 210–215, 244–247 with Pls. 5.21–22). For further comments, see also van der Veen (2020, 19, n. 16).

[17] Singer-Avitz questions the excavator's all too rigid approach, as 'a fill is not a stratigraphical context' and as 'the debris for the fill ... was brought hither from the surrounding area of the City of David ... most probably from more than one stratum' (Singer-Avitz 2012, 12). Having looked through the fill material herself (pers. comm. 2015), she feels that some sherds qualify as Iron Age IIB, such as those belonging to 'flat open bowls, folded rim bowls and kraters ...' (2012, 13).

[18] De Groot and Fadida (2011, 161).

[19] Van der Veen (2020, 27, 169). While Reich argues (2011, 159) that the house above the 'Rock-Cut Pool' and the associated 'extra mural quarter' in this area, was mainly occupied during Iron Age IIB, de Groot and Fadida argue that also some Iron Age IIC pottery was found, evidently 'postdating' the quarter's occupation. Be this as it may, the present author prefers to date its abandonment sometime early during Iron Age IIC.

[20] For an excellent summary, see Bagg (2011, 241, 244–269). On Sennacherib's campaign against Judah, see also Bagg (in this volume, pp. 311–321).



Figures 6a–c. Left: photo of bulla '1' with a sun disc with rectangular barred wings; centre: photo of bulla '2' with a schematic sun disc; right: bulla '3' with a sun disc and a complex constellation (photos: courtesy of Prof. Dr O. Keel).

regional Levantine elements, such as boats, fish, Proto-Aeolic capitals and the 'Master of Animals' theme. We also find simple geometric designs (Keel 2017, 340–409, nos. 138–187). More relevant to our discussion here, are 'Late Assyrian-style' motifs, mainly 'winged solar discs', apparently representing the Assyrian sun god Shamash, who played a most prominent role (also in combination with the horse as his symbol), especially during the Sargonid period, i.e. after 720 BC (van der Veen 2020, 155–162). Three bullae are of particular interest:

Bulla No. 1 – Figure 6a (Keel 2017, 406–407, no. 283; van der Veen 2020, 22–23): this bulla measures 16.9 x 15.6 x 6.5mm and portrays a winged sun disc. Although the upper edge of the seal impression is missing, the sole element depicted is a schematic solar icon with two vertical wings divided by horizontal and vertical bars. Due to the fragmentary state of the bulla, it is difficult to say if the solar disc is represented by an actual disc in the centre, or if the space in the centre was left empty. Below the wings, the sun's tail contains three vertical strokes flanked by others ones bending upwards at the end of the beams, representing 'bird claws' or 'streamers'. This is a well attested feature in Neo-Assyrian and Neo-Babylonian art (see Collon 2000, 81, 82).[21] Although due to the missing upper edge of the bulla, we cannot now know if the solar emblem also contained upper rays and/or an appendage with terminating loops. Similar Late Assyrian solar disc representations are well-attested during the eighth–seventh century BC in the Assyrian Central Polity and bordering regions.[22]

Bulla No. 2 – Figure 6b (Keel 2017, 406–407, no. 285; van der Veen 2020, 23–24): this fragment measures 21.8 x 15.3 x 6mm and the seal impression portrays a very schematic type of winged sun disc, and although it is only partly preserved, its form is well attested on Late Assyrian and Neo-Babylonian seals. The wings and tail consist of mere horizontal lines. A diagonal 'antenna'-like stroke extends from the centre upwards and is likely part of the upper rays or part of another astral image. Similar seal imagery (partly from Ashur, Borsippa and Babylon) was published by Jakob-Rost (1997, 95, Figs. 401–408).[23] Moreover, impressions on datable cuneiform tablets suggest a date well within the seventh century BC for this particular type. Good parallels exist for the years 687, 669 and 661 BC and from the post-canonical period at Nimrud and Nineveh (Herbordt 1992, Table 4: Fig. 13, Table 10: Fig. 3, 9 and 10).[24]

Bulla No. 3 – Figure 6c (Keel 2017, 408–409, no. 286; van der Veen 2020, 24–25): this bulla measures 14.6 x 13.4 x 6.6mm. Again, the bulla is incomplete as the right, top and bottom edges have crumbled. Its depiction contains a complex constellation, whose elements appear to be a mixture of Late Assyrian and Syro-Phoenician and/or Egyptianising elements. As was argued above, the combination of various symbols of diverse origins was characteristic of the Late Assyrian period.

The constellation portrays a winged sun disc (either attached to or hoovering) over a dais, as the central theme. The sun disc is represented by a small dot, while its wings are crudely demarked. While the left wing contains three horizontal bars, the right one has only two.[25] The solar 'tail' is relatively long, while

[21] See for instance: Delaporte (1923, 170, Pl. 90, Figs. 4 and 12, Pl. 91, Fig. 10); Moortgat (1988, Table 88, Fig. 750). This feature is also seen on a seal from Tel Gezer (IAA J.875), Ornan (2013, 17, Fig. 21).

[22] For instance, Collon (2000, 81–82), Jakob-Rost (1997, 66–67, Fig. 190 [from Merkes, Babylon], 72–73, Fig. 231). For further examples, see van der Veen (2020, 22–21 with footnotes).

[23] Similar schematic representations can be found at Tel Goren (Ornan 2005, 217, Fig. 12; in the same article see also Fig. 14a).

[24] See also Delaporte (1923, Table 91:10), Jakob-Rost (1997, 94–95, Fig. 406).

[25] For a somewhat similar solar disc (but generally with

Figures 7a–b. Left: photo of the seal with an 'Assyrian-style' winged sun disc above a solar boat from Amrit (after K. Galling 1941, 145); right: photo of the bulla found outside the 'Rock-Cut Pool' in Loc. 2328 (courtesy of Prof. Dr O. Keel).

a thin line demarks what seems to be a pole attached to the dais. The figure to the left of the winged sun is more difficult to interpret, especially as the right side of the bulla is missing.[26] Othmar Keel has suggested that this figure may represent a backrest of a throne or chair (Keel 2017, 408). Alternatively, it could be an Egyptianising 'maᶜat-feather' or a floral motif (van der Veen 2020, 24–25). The whole composition (including the dais) sits on what appears to be an Egyptian 'solar bark' (Keel 2017, 408; Schroer 2018, 454–455, Fig. 1427). Above the constellation, four vertical strokes are depicted at the upper edge of the bulla, which resemble the tail of another winged emblem, but as available space for it is very limited, it is very difficult to see what it represented.

While solar boats with enthroned pharaonic figures are well-attested on Egyptian scarabs of the early first millennium BC (Keel 1994, 53–134, esp. Figs. 14–16; Schroer 2018, 454–455, Fig. 1426), the combination of an 'Egyptian' solar boat with Assyrian-style motifs is characteristic of 'Aramaising' seals of the latter part of the Late Assyrian period (Keel and Uehlinger 1998, 308, Figs. 306a–c; van der Veen 2020, 26). Additionally, a seal with a solar boat and a nonanthropomorphic Assyrianising winged sun hoovering over it, was found at north Phoenician Amrit and dates to the late eighth–seventh century BC (Bordreuil 1986, 25, no. 9; see *Figure 7a*).

Finally, above the 'Rock-Cut Pool', on the southern edge (Locus 2328), east of the Spring Tower, Reich and Shukron uncovered yet another typical Late Assyrian-style bulla with a solar icon (Reich 2011, 217; Keel 2017, 406–407, no. 284; van der Veen 2020, 26–27, Fig. 4). It was found with other (partly epigraphic) seals and bullae and Iron Age IIB pottery. It can therefore be squarely dated to the time when the extramural quarter in this area was inhabited, likely during the first half of the seventh century BC. The bulla depicts a winged sun disc with barred flaring wings, a triangular tail and a looped element above the solar disc (see *Figure 7b*).[27] It closely resembles the solar icon depicted on an official jar impression (with the inscription *lnrt*) from Tel Goren Stratum V of the later 7th century BC.[28]

In closing, the following can be said: Based on the iconography of the 'Late Assyrian-style' bullae from the 'City of David' (with datable parallels from Sargonid Assyria) and the biblical tradition, that Hezekiah 'closed the upper outlet of the waters of Gihon' (apparently by 'blocking' the old tunnelling system nearby), it appears unlikely that the 'Rock-Cut Pool' was filled in much before 700 BC. If justified, this then suggests that the dates of either the end of Iron Age IIA or those of the early years of Iron Age IIB must be lowered to the late eighth century BC (if the 'blocking' occurred prior to the Assyrian siege of Jerusalem) or slightly later (if the 'blocking' is related to building measures taken for the construction of the extramural quarter), perhaps during the early reign of Manasseh (van der Veen 2020, 176–178). A similar late date for the end of Iron Age IIA has been proposed by James *et al.* (1991, 178).

broader tails), with and without streamers, see Collon (2000, 82, 143, 172, 189, etc., e.g. Pls. XI: 143, XXII: 271). For a rather similar sun disc on a stamp from Nineveh, likely dated to 620 or 619 BC (reign of Sin-šar-iškun), see Mitchell and Searight (2008, 89, no. 181).

[26] It is therefore impossible to say if an identically shaped motif was found in parallel to the right of the solar icon.

[27] Similar loops are for instance found on the solar emblem depicted on the Kilamuwa orthostat (late ninth century BC) and on the orthostat and royal bullae of Bar-Rakib (*c.* 730 BC) of Samal/Zincirli (Parayre 1993, 46, Fig. 32; 51, Fig. a; for a more detailed discussion, see also Parayre 1990, 269–314).

[28] See van der Veen (2014, 153–161); van der Veen and Bron (2014, 215–217); Stern (ed.) (2007, 161–162).

2. Stratified 'Late Assyrian-Style' Pottery in the Southern Levant and the Dating of the 'Lachish Level III- Horizon'

2.1 'APW' in Assyria and its derivations in the Southern Levant

'Archaeological horizons' are frequently dated by 'destruction levels' (if such evidence exists). Consequently, ancient historians seek to associate these levels with attested historical events. But rarely so are the associations beyond any reasonable doubt (see Chapman in this volume, pp. 347–354 and Bagg, pp. 311–321). Often sufficient stratified inscribed evidence or imported luxury items (including imported pottery styles and their local derivations) are lacking, which could have supported a more precise dating.[29] In this section we will concentrate on the chronological significance of so-called locally produced 'Late Assyrian pottery styles' (including 'Late Assyrian-Edomite-styles', especially as found in the Negev desert and in central Edom) with a specific focus on finds retrieved from Iron Age IIB strata in Cisjordan. It will be necessary, however, to delineate genuine 'Late Assyrian pottery styles' from Assyria, from which these local 'imitations' (made by local Levantine potters) were derived, to better appreciate their similarities.

While locally produced 'Late Assyrian-style' derivations continue to linger on during the Neo-Babylonian and Persian periods (i.e. well after the fall of the Assyrian empire), it is their *early* occurrence in Iron Age IIB strata in Cisjordan (*c.* 830–730/700 BC) that primarily concerns us here. For it is this early occurrence of styles imitating so-called 'Assyrian Palace Ware' (henceforth: 'APW') that stands in apparent chronological contradiction with what we know from the Assyrian homeland. The first appearance of genuine APW in Assyria is believed to have occurred during the second half of the eighth century or likely so during the Sargonid period, i.e. from *c.* 721–612 BC, postdating the end of Iron Age IIB in Israel and partly so in Judah. Some of the most characteristic styles are dimpled goblets and carinated bowls, types that have surfaced at Iron Age IIB Megiddo, Samaria and in the Beersheba-Arad-Valley. While the end of Iron Age IIB in Israel/Judah and the Sargonid period in Assyria proper are not necessarily separated by many years, the main occurrence of APW styles in Assyria was in fact the seventh century BC when these styles appear to have especially flourished. It seems therefore strange for APW-imitations in Israel and Judah to occur a) partly before the Sargonid period, and b) before even the Assyrians had invaded and annexed these regions.

This apparent anachronism has led to some controversy in the field (for a lengthy discussion of the problem, see van der Veen 2020, 69–124). While some scholars have suggested lower dates for Iron Age IIB strata in Israel and Judah (James 2008; Knauf 2002, 181–195; Fantalkin and Tal 2015), others have preferred to question their 'Late Assyrian' derivation in order to simply avoid the matter altogether (Na'aman and Thareany-Sussely 2006, 61–82; Smith and Levy 2008, 41–91). The latter suggestion, however, has not met with much acceptance, as these wares (despite some local modifications in style) still reflect their 'Late Assyrian' prototypes (Finkelstein and Singer-Avitz 2009, 207–218; Bienkowski 2011, 77–78).

So how would we explain the occurrence of 'Late Assyrian style' derivations in Iron Age IIB strata before the Sargonid period? And would it not be more logical to assume that these derivations only started when the local populations of the region were more fully assimilated into Neo-Assyrian culture during the seventh century BC? For it was at this time that the demand for such styles steadily increased. As a matter of fact, 'Late Assyrian-style' pottery and other luxury items in Transjordan (for instance at Amman and at Edomite sites including Busayra, Tawilan and Umm al-Biyara) become popular during the seventh century. This has been confirmed by epigraphic finds (including seals and bullae of kings and courtiers) found in association with these 'Late Assyrian styles'. These finds name kings Amminadab I of Ammon and Qosgabr of Edom, who are attested in the annals of Esarhaddon (681–667 BC) and Assurbanipal (667–627? BC). The present author has discussed these finds at length in a monograph (van der Veen 2014, 163–187, 213–250).

2.1.1 Genuine 'Assyrian-style' pottery from Sargonid Assyria

Before we look more closely at locally manufactured 'Late Assyrian-style' vessels from Iron Age IIB strata in Israel and Judah, it is necessary to delineate the features of genuine 'Late Assyrian' styles, including typical APW forms, and determine why experts have dated these types mainly to the Sargonid period rather than to the ninth–earlier eighth century. In recent years, scholars have sought to establish a more viable chronological framework (Hausleiter 2010; Anastasio 2010). Additionally, Alice Hunt (2015), has sought to delineate more precisely the characteristics of typical APW-style pottery in the Assyrian heartland, beyond what earlier studies had already treated (Rawson 1954, 168–172; Oates 1959, 130–146).

Genuine APW styles contain 'eggshell' thin walls, exquisite forms and decoration and were fired at very high temperatures in an 'oxidising kiln' (Rawson 1954,

[29] Mostly so, scholars seek to obtain independent confirmation from radiometric dating. But how certain can we be, that 14C indeed yields independent satisfactory dates? On this issue, see Zerbst and van der Veen in this volume, pp. 373–399. See also Zerbst and van der Veen (2015).

168–172, esp. 170). According to Hunt it is especially the combination of form, thin walls and pasty clays that typifies APW (Hunt 2015, 4). She argues that the 'calcareous ceramic' of its fabric tends to turn olive green when it is vitrified at high temperatures up to some 1100 degrees Celsius (Hunt 2015, 74). She identifies the following subgroups (also *Figures 8a–d*):

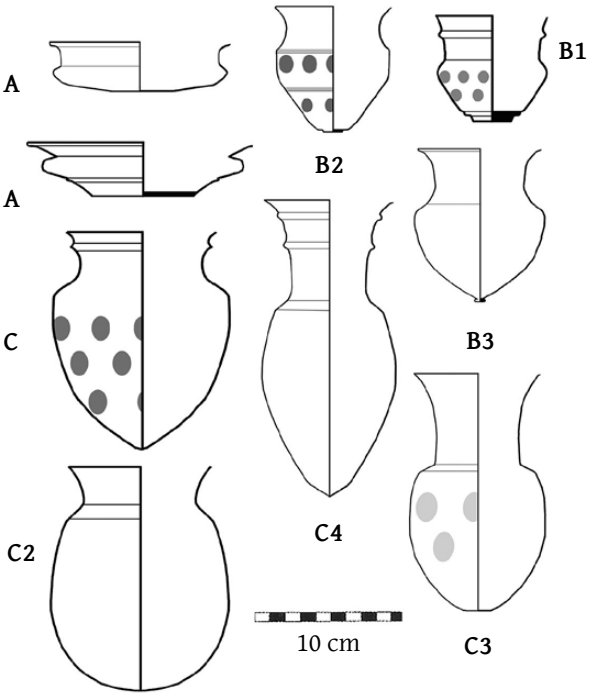


Figure 8a. APW subgroups 'Forms A–C' according to the study of A. Hunt (after Hunt 2015, 57, Fig. 3.20; reproduction courtesy of Dr Hunt and the E. J. Brill publishers in Leiden).

Form A: this subgroup mainly consists of horizontally shaped, low biconical carinated, noncarinated and/or S-shaped bowls with flat and disc-shaped bases (Hunt 2015, 48–50). Their walls measure *c.* 0.3cm. On average they are between 2.5–4.0cm tall and 9.0–14.0cm wide. Some 32% of all APW styles belong to this group.

Form B: this subgroup consists of biconical vertically shaped 'jars' with 'incurving necks, out-curving horizontal everted rims, and thinned and rounded lips' (Hunt 2015, 49–50). Their bases can be flat, knobbed, or ring-shaped. Due to their small size, the term 'cups' or 'goblets' would have been better chosen. Some 83% of this group contains two or three rows of impressed dimples. Their walls are very thin and measure on average between 0.15–0.3cm. Their sizes range between 7.6–14.8cm in height. This is by far the largest subgroup within the APW repository (i.e. 74% or more).

Form C: this subgroup closely resembles Form B, but its vessels are larger, i.e. between 10.0–20.5cm in height.

Some 65% of all forms in subgroups B and C are decorated with dimples. The vessels belonging to these subgroups are so exquisite, that there can be little doubt that APW was the table ware par excellence used by the Neo-Assyrian elite (van der Veen 2020, 76–80).

Figures 8b–d. Left: Hunt Form B2 'APW' dimpled goblet from Nimrud, c. eighth–seventh century BC (AN 54.117.35, Metropolitan Museum of Arts, New York, Public Domain); centre: Hunt Form C3 'APW' dimpled beaker from Nimrud, seventh century BC (AN 1951.20, courtesy of the Ashmolean Museum in Oxford, photo by the author); right: Hunt Form A 'APW' carinated bowl from Nimrud, c. eighth–seventh century (MMA 52.23.16, The Metropolitan Museum of Art, New York, Public Domain).

2.1.2 When did APW start to be produced?

APW and related forms in the Central Polity of Assyria have been mainly retrieved from seventh century contexts, for instance at Nimrud, Fort Shalmaneser, Ashur, Khorsabad and Nineveh (Hausleiter 2010, 146, 149–152). While these styles continued in use after the fall of the empire, at sites where Assyrian officials remained in office,[30] when did the Assyrians first produce APW and related styles? Although some scholars have argued in favour of dates well before the Sargonid period (Ohtsu 1991, 134–136; Mallowan 1966, 38–50), the evidence for this is very slim. During their excavations there, British archaeologists uncovered great quantities of APW-styles at Nimrud, for instance at the Governor's Palace and Northwestern Palace. The construction of the relevant buildings is believed to be supported by cuneiform tablets from the reigns of Aššurnaṣirpal (883–859 BC), Shalmaneser III (858–824 BC) and Adad-nirari III (810–783 BC). But the stratigraphic relationship between the pottery and the original structures is debatable, as several of these buildings remained in use until the fall of the empire (Oates and Oates 2001, 251). This has been confirmed by other cuneiform tablets in these structures from the reigns of Sennacherib down to Sin-šar-iškun (622–612 BC; Ohtsu 1991, 136; Lehmann 1996, 422; Hausleiter 2010, 158–161). While overall it seems likely that the pottery in these buildings belonged mainly to the final phase of occupation, the seventh century date for the apogee of APW is also supported by inscribed evidence at Assur, Nimrud and Khorsabad (Hausleiter 2010, 13, 146):

> Most of the material examined here can be dated to the Late Neo-Assyrian period (nA II) and thus covers the period from the 'eighth–seventh century BC' (a maximum of 89%). Due to the available information on the stratigraphy, it was possible only in a few cases to arrive at dates in the eighth century BC (nA IIa, 1% only), while the chronological containment to the seventh century BC or the second half of the seventh century BC is provided by stratigraphic clues and associated datable epigraphic finds. (translated from the German by the author)

Although some earlier forms have surfaced at more recent excavations at Ashur (Qalᶜat Sherqat), only very few 'early forms' could be identified with certainty, as most early 'Late Assyrian styles' still maintain conservative Middle Assyrian forms. Moreover, some of these continued in use – virtually unmodified – throughout the Late Assyrian period (Anastasio 2011, 348). These include shallow and deep heavy bowls, footed and unfooted cups, storage jars, straight-sided and S-shaped juglets, double saucer lamps and goblets and beakers *without* dimples (van der Veen 2020, 82). While Hausleiter assigns some 17% of the pottery forms from 58 sites in the Upper-Tigris, Upper- and Central-Euphrates and Habur regions to the early phase of the Late Assyrian period, only 5% of the relevant pottery made as early as the ninth century is found in the Assyrian Central Polity (Hausleiter 2010, 497–502). More relevant to our discussion here, is the question of which types are clearly missing in the period before the eighth century? One form is clearly the 'dimpled cup or beaker' (Hausleiter 2010, 303, 308, Table 1 and 9; Anastasio 2007, Fig. 16, no. 16.5, Fig. 23:14 and 15; Bartl 2011, 2, 7, Fig. 4), for Hausleiter argues that dimpled beakers only 'begin in the heartland at the end of the eighth century BC' (2010, 308). Also according to Hunt dimpled beakers (belonging to her 'Form B cups' and 'Form C jars') belong to the latter half of the Neo-Assyrian empire. Moreover, in addition to their dimples, it is their thinness and their pasty clays that stand out as characteristic for the later period (Hunt 2015, 4). And it is precisely these features that also local Levantine potters sought to imitate, even if they never managed to completely emulate the egg-shell thin fabric of genuine APW. Also, at Tell Jemmeh (in the southern coastal plain of Israel/Palestine), where the local potters sought to imitate 'stylistic and formal similarities to vessels in the greater Neo-Assyrian ceramic assemblage, particularly tableware' (Hunt 2015, 156), dimpled and undimpled goblets proved to be *c.* 0.1cm thicker than in Assyria (Hunt 2015, 153). Other typical late types include various smaller sized thin-walled carinated bowls with everted rims (Hausleiter 2010, 503, Fig. 69). These styles too were imitated. But as excavations nearer the Central Polity show, these types do not occur there before the late eighth century BC. For even local potters in the Habur region, such as at Tell Barri, did not begin imitating them before that date (Jamiesen 1999, esp. 304, Fig. 6; 2009, 27, Fig. 10; 2012). The situation proves to be similar in the western parts of the Assyrian hinterland, as well as in the province of Rassapa, whose sites were founded during the eighth century (Hausleiter 2010, 13; Radner 2007). Apart from the APW innovations, most funnel-necked bottles and Assyrian-style storage jars also originated during this period (Hausleiter 2010, 503, Fig. 68, Tables 88–89). That these styles only reached the remoter parts of the empire (including Israel and Judah), when these regions finally fell to Assyria (i.e. after *c.* 730/701 BC), seems a logical conclusion.

2.2 'APW-styles' outside the Assyrian Central Polity

Very little true Assyrian APW has been found outside the Central Polity (van der Veen 2020, 83–87). Hunt found virtually identical types at nearer Dur Katlimmu

[30] E.g. at Dur-Katlimmu/Tal Shekh Hamad and Khirbet Qasriya; Hausleiter (2010, 13–16); Anastasio (2010, 61); Kreppner (2008, 147–165).

(Tal Shekh Hamad) – where most of the pottery derived from post-Assyrian squatters – and at Tell Guzana (Tell Halaf). At sites much further afield like in the southern Levant (Lehmann 1996, 422–423, Table 54), local potters also sought to imitate carinated globular and open bowls, dimpled and undimpled beakers, but they arrived at poorer results. Often these styles also contain coarser fabrics, which – based on Neutron Activation-Analyses – are clearly local productions made of local clays (Engstrom 2004, esp. 71–79; Anastasio 2010, 25; Stern 2015, 534). Genuine Assyrian wares are hardly ever found in these regions, except for glazed ware (for instance at Lachish and Khirbat al Mudayna: Magrill 1989–1990, 41–45, Daviau and Klassen 2014).

2.2.1 'Assyrian-style' derivations from Iron Age IIB Israel and Judah

Now let us see which styles have been found in the Northern Kingdom of Israel and which in the Southern Kingdom and bordering regions, by presenting representative *Iron Age IIB* locally produced 'APW-styles'. This list is by no means exhaustive.

2.2.1.1 An 'APW-style' tall, dimpled beaker at 'mid-eighth century BC' Megiddo? (see *Figure 9*)

According to the excavators (Finkelstein *et al.* 2000, 313) three sherds of a burnished 'Late Assyrian-style' locally produced dimpled beaker[31] (reg. no. 96/H/31/VS6) were found 'in a safe stratigraphic context of Level H-3' (i.e. in unit 94/H/8). This type can be classified as a 'Form B3' cup in Hunt's system. Its prototype in Assyria clearly belongs to the Sargonid period (for parallels see: Hausleiter 2010, Table 87: types BD 2.2 and 2.3 [from Nimrud], and BD 3.1 and 3.2 [from Ashur]). The stratum in which the beaker was found, is considered by the excavators to be part of Stratum IVA (Finkelstein and Ussishkin 2000, 597). Finkelstein's team dates this stratum to *c.* 800–732 BC (Finkelstein *et al.* 2000, 322; Finkelstein and Ussishkin 2000, 597–598). But even this *terminus ad quem* appears somewhat too high as the excavators admit. While a date *c.* 720–715 BC for the beaker cannot be ruled out, it still seems unusually early for this locally produced specimen. Even Finkelstein *et al.* admit that we have a chronological conflict:

> The most significant vessel for dating the last days of the assemblage of Level H-3 (Stratum IVA) is the locally made Assyrian bottle [sic. dimpled beaker] ... if Assyrian forms were imitated only after the annexation of the Galilee and the northern valleys by Tiglath-Pileser III, the destruction of Megiddo IV must be placed after that event. If the latter opinion is preferred, the site would have ... been destroyed somewhat later, perhaps when the province was reorganized under Sargon II ... (Finkelstein *et al.* 2000, 322)

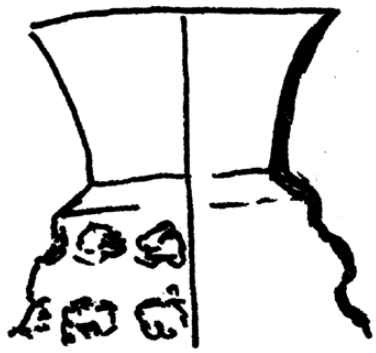


Figure 9. Dimpled goblet from Megiddo Stratum H-3 (IVA; line-drawing by the author after Finkelstein et al*. 2000, 306, Fig. 11.46.7).*

While it is generally assumed that Megiddo Stratum IVA was destroyed by the Assyrians, the Assyrian records do not actually tell us this. What they do say is that *Magiddu* became an Assyrian province in 732 (Bagg 2011, 226, 232; Holladay 2009, 87) and that an Assyrian official resided there in 679 BC (Millard 1998, 61). Baruch Halpern, former co-director of the Megiddo excavations, has postulated that Megiddo Stratum IVA could well have overlapped with Stratum III of the seventh century and that the shift between the two strata would have been smooth rather than violent (Halpern 2000, 565–566). Halpern's lower dates surely do more justice to the appearance of the dimpled beaker at Megiddo. In the light of the findspot of the Shema seal discussed above, one could also suggest that Megiddo Stratum IVA in its entirety originated after the takeover by the Assyrians (James 2008, 166–172). Alternatively, Stratum IV began before the annexation and continued uninterruptedly into the seventh century BC.[32]

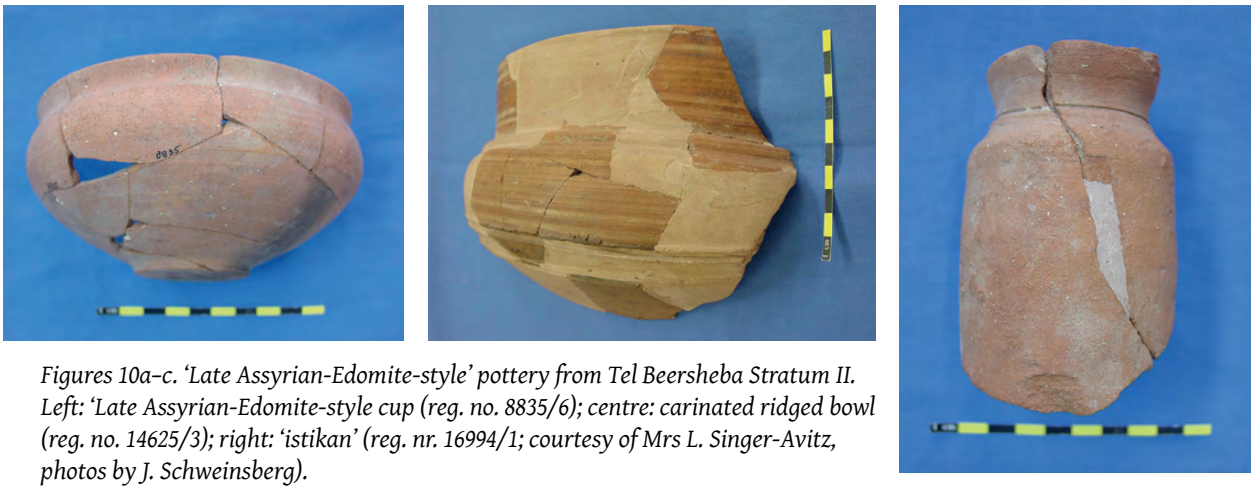
2.2.1.2 'APW-style' derivations in eighth century 'pre-conquest' Samaria?

A similar situation is found at Samaria, but as the excavations took place during the first half of the 20th century, the stratigraphic context is insecure (van der Veen 2020, 93). When Kathleen Kenyon discussed the stratigraphy of 'Assyrian-style bowls'[33] from Pottery

[31] According to Y. Goren this beaker had been locally manufactured (Finkelstein *et al.* 2000, 313).

[32] I.e. depending on whether the destruction of Stratum VA–IVB was caused by an earthquake referred to in Amos 1:1, i.e. in *c.* 759 BC (Galil 1996) or by the Assyrians in *c.* 732 BC (see van der Veen 2020, 61–63).

[33] The most intact 'Assyrian-style' vessel is a thin-walled



Figures 10a–c. 'Late Assyrian-Edomite-style' pottery from Tel Beersheba Stratum II. Left: 'Late Assyrian-Edomite-style cup (reg. no. 8835/6); centre: carinated ridged bowl (reg. no. 14625/3); right: 'istikan' (reg. nr. 16994/1; courtesy of Mrs L. Singer-Avitz, photos by J. Schweinsberg).

Periods (PPs) V–VI at Samaria (by comparing them with similar vessels from PPs VI and VII and from Megiddo Stratum III), she associated them with 'Pit *i*', belonging to Building Phase V (Tappy 2001, 311–315, Figs. 10: 8–10; 344, 349–350). Building Phase V is dated by her to before the Assyrian takeover in 722/721 BC. Despite persisting stratigraphical uncertainties, Ron Tappy still accepted her attribution of Building Period V to before the fall of the city but suggests that the pit and the associated house would have suffered damage after 722/721 BC. Alternatively, of course, one may assign the entire Building Phase V to after 722/721.

2.2.1.3 'APW-style' derivations from 'eighth century BC' levels in the Beersheba-Arad Valley (Judah-Negev border)

Tel Beersheba: some 'Late Assyrian- and Edomite-Assyrian-style' vessels were retrieved from Levels III and II (Singer-Avitz 2016, 657; see *Figures 10a–c*).[34] The excavators date these layers to the eighth century (i.e. Level III = 'early Iron Age IIB' [800–720/15 BC] and Level II [720/715–701 BC]: Herzog 2016, 29, Table 2.2; Singer-Avitz 2016, 656–658, pers. comm. 2024[35]) by assuming that the destruction of Stratum II was caused by Sennacherib's incursions into the region (van der Veen 2020, 96). The 'Assyrian-style vessels' include 'bowls, jugs, and bottles' (Singer-Avitz 2016, 659; van der Veen 2020, 96–98, with Figs. 46a–b, 47a–b and Figs. 48–b). These can be classified into the following types: B-6, a bowl-type 'with pronounced carination at mid-wall, splayed rim and a low base' (found in both Levels III and II[36]); B-23: a 'closed globular bowl' with a ring-base and a ridged neck (found only in Level II)[37]; JD-12: Assyrian-style 'istikans' or drinking cups (found in Level II)[38], and JD-11: bottles with a globular body and round and flat bases (found again in both Levels III and II).[39]

Tel Arad: although most vessels belong to the late Iron Age IIC Strata VII and VI at the site, some 'Late

carinated bowl with one band and incised lines. Tappy notes 'the general motif of horizontal lines, ribs, or bands proves quite common on many classes of Assyrian-style pottery from the seventh century BCE' (Tappy 2001, 312, with Fig. 10:8 on p. 311). Sherds of several more relevant vessels were found.

[34] For the chronological ascription of Strata III–VII to the second half of the eighth century, see also Singer-Avitz's earlier publications (Singer-Avitz and Eshet 1999; Singer-Avitz 2014, 269–277).

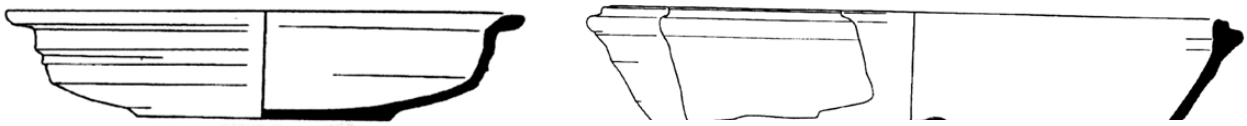
[35] Strikingly, Ze'ev Herzog suggests that Stratum IV at Tel Beersheba (= late Iron Age IIA) was destroyed by earthquake, which caused the 'collapse of a large part of the rock roof of the underground reservoir of the water system' (Herzog 2016, 24). While this earthquake is assigned by him to *c.* 800 BC, it may relate to the earthquake that happened at the time of kings Jeroboam II and Uzziah, referred to in Amos 1:1, that occurred in *c.* 759 (Galil chronology). Similarly, the present author has suggested that evidence of earthquake at the end of Megiddo Stratum VA–IVB (late Iron Age IIA) could belong to that same event and would therefore be in harmony with the lower date suggested here for the destruction of Gatehouse '1567' and the deposition of the Shema seal (van der Veen 2020: 61–63).

[36] See Singer-Avitz (2016, 586, Pl. 12.1, Fig. 12.65: no. 3) with parallels in Singer-Avitz (2007, 183–185). For similar bowls in Assyria, see Hausleiter (2010, Table 59, SF 20.8 from eighth–seventh century Ashur), Anastasio (2010, 160–161, Pl. 42:14) from Khirbet Qasriya dating to the post-Assyrian settlement.

[37] Singer-Avitz (2016, 592, Pl. 12.2, Fig. 189: no. 4). It finds parallels in Assyria for instance at eighth–seventh century Sherqat (Anastasio 2010, 116–117, Pls. 20: 4 and 53: 11). This type is also found at Tel Batash (Mazar and Panitz-Cohen 2001, Fig. no. KR 11c).

[38] Singer-Avitz (2016, 637, 978, with Pl. 12.17, Figs. 12.161: 14 and Fig. 12.223: no. 32). This style is rarely found in the southern Levant (e.g. in Megiddo Stratum III and at Tell Jemmeh: Singer-Avitz 2016, 637). It is however common in the Central Polity of Assyria and stretches from the ninth to sixth centuries BC. For several parallels, see van der Veen (2020, 97).

[39] Singer-Avitz (2016, 637, Pl. 12.17, Fig. 12.77:21). For similar vessels from Assyria, see Hausleiter (2010, Table 92, FL 1.1, 1.3–5 from Nimrud and Ashur, dating the seventh and sixth centuries BC). Similarly, see Anastasio (2010, 142–143, Pl. 33,7) from Tell Ahmar, also from the seventh century.



Figures 11a–b. 'Late Assyrian' and 'Late Assyrian-Edomite-style' pottery types from Tel Arad: left: bowl type B13; right: bowl type B12 (after Singer-Avitz 2002, 134, courtesy of Mrs L. Singer-Avitz).

Assyrian- and Edomite-Assyrian style' vessels were found in the Iron Age IIB Strata X–VIII (i.e. between *c.* 750–701 BC; Herzog and Singer-Avitz 2015: 213–214, 223–224), assuming that Stratum VIII was destroyed by Sennacherib in 701 BC (see *Figures 11a–b*). These vessels include ribbed 'Assyrian-style bowls' (of 'Tel Arad Type B13'), the earliest of which appears in Stratum IX (Singer-Avitz 2002, 130)[40], and a 'Late Assyrian-Edomite-style' bowl ('Tel Arad Type B12'), which was likewise found in Stratum IX. Parallels from Assyria and Edomite sites in the highlands (including Tawilan Type D, TS 518) span the period from the late seventh century BC onwards (van der Veen 2020, 95).[41] Other 'Late Assyrian-Edomite-style' bowl types from Tel Arad include Types B29 (found in Strata IX–VIII) and B30 (the earliest of which was found in Stratum X). Some of these are decorated with black and red lines (Singer-Avitz 2002, 132, 135). Worth citing here also, are small finds including an Assyrian-style bronze lion weight (found in Stratum IX) and Assyrian-style fibulae of the 'triangular bow-type' (occurring as early as Stratum X).

Tel Aroer: while most 'Late Assyrian-Edomite-style' vessels were found in the Iron Age IIC Stratum IIb (Thareani 2011-I, 122, BL 8–10), the earliest specimens occur in Strata IV and III (spanning the period between the mid-eighth century and 701 BC). While Thareani rightly questions that some are 'Late Assyrian-Edomite' in style (Na'aman and Thareani-Susseley 2006; Thareani 2011-I, 124–126[42]), others are indisputably 'Late Assyrian' and are described as such by her: i.e. the 'Late Assyrian-Edomite-style' globular closed bowl type BL 22 (Thareani 2011-I, 125–126), with a straight rim (from Stratum IV)[43], the 'Late Assyrian-Edomite-style' deep globular ridged bowl BL 23 (Thareani 2011-I, 126), with black lines (from Stratum III)[44], and carinated 'Late Assyrian-Edomite- style' bowl BL 28 (Thareani 2011-I, 127; 2011-II: Pl. 162:4), which has an everted rim and is placed on a small disc base (Stratum IV–III).[45] For the latter, see *Figure 12*.

Figure 12. 'Late Assyrian' bowl from Tel Aroer (by courtesy of the Nelson Glueck School of Biblical Archaeology, Dr D. Ilan).

2.3 Sennacherib's campaign in 701 BC: a faulty linchpin?

What do we make of the 'pre-701 BC' strata in the Beersheba-Arad-Valley in terms of the 'Late Assyrian-Edomite-style' vessels at Tel Beersheba, Tel Arad and Tel Aroer?

As the local Iron Age IIB pottery at these sites finds close parallels at Lachish Level III (believed by most scholars to have been destroyed by Sennacherib in 701 BC), these strata are believed to be squarely contemporaneous with Lachish Level III. But this assumption has put an incredible strain on the proposed time frame. While the 'Late Assyrian-style' pottery in these strata rightly suggests that it cannot predate the earliest Assyrian impact on the region, the assumption that Sennacherib

[40] Similar (albeit not identical) bowls are known from the Assyrian Central Polity but are dated there to the late and post-Assyrian periods (Hausleiter 2010, 334, 337; van der Veen 2020, 94, n. 269).

[41] Singer-Avitz also cites a parallel from Tel Batash Stratum II, from the late seventh century BC (2002, 130).

[42] This is partly justified as some styles are rather remote and lack sufficient diagnostic traits (cf. Tel Aroer BL15, Thareani 2011-I, 124; van der Veen 2020, 98, n. 280). Some specimens are dated by Thareani to her post-701 BC Stratum IIb, but the existence of this stratum is debatable (Singer-Avitz 2022, 79).

[43] Parallels from Assyria may be found in Hausleiter (2010, Table 115: TE 7.1, from Tell Rimah; Table 116: TM 1.1, from Khirbet Qasriya; TM 2.1, from Nimrud).

[44] Local parallels are found in Tel Beersheba Stratum II and late 7th–6th century BC Tawilan (Thareani 2011-I, 126).

[45] Parallels are from Tel Sera and Tell Jemmeh in Israel/Palestine and from seventh century Nimrud and Nineveh (Thareani 2011-I, 127).

destroyed these sites in 701 BC, literally squeezes the duration of these successive strata into a very short period. As for Tel Arad Stratum X Singer-Avitz concludes:

> The inevitable conclusion is that Stratum X (or, at least its final phase, represented by the finds we present) **must have existed when Judah was already one of the vassal states of the Assyrian empire**, incorporated into its economic system and exposed to certain Assyrian/Edomite influences. Hence the end of Stratum X cannot be earlier than the reign of Tiglat Pileser III and a date ca. 725 BCE seems possible. (Singer-Avitz 2002, 162; emphasis added by the present author)

The problem with this view is that we have no evidence whatsoever that Sennacherib's army ever invaded southern Judah, much beyond Lachish or Debir (see Bagg in this volume, pp. 311–321; also Bagg 2011, 244–251). As a matter of fact, the assumption that Sennacherib would have invaded the area, is difficult to sustain, as also Axel Knauf has argued:

> According to this reconstruction of the 701 campaign, Sennacherib had his hands too full to even think of a tiny outpost in the Negev. No Assyrian soldier, in all probability, ventured south of Debir (Ḫirbet Rabūd). (Knauf 2002, 190)

Similarly, Assyriologist Andreas Fuchs has argued that Sennacherib's campaign against Judah (one of the remotest parts of the Assyrian empire) ate up enormous sums of the state's revenues.[46] An additional siege against Jerusalem and a most time-consuming guerilla war in the Judaean hills (let alone against towns and villages as far south as the Negev) would have brought Assyria financially to the limits of the impossible. And why would the Assyrians have done so? Quite to the contrary, they would have cared to maintain the infrastructure of these trading towns and border fortresses to secure the trading route between Gaza, the Arabah and the Red Sea. Goods traveling between Arabia and Gaza were an important source of income while also this route was a crucial army track, as also later Esarhaddon would find out, when he invaded Egypt with the help of Arabian camel raiders.

Knauf dates the demise of Tel Beersheba Stratum II to *c.* 675 and of Tel Arad VIII to *c.* 650 BC (2002, 181–195). For similar reasons also Itzhaq Beit-Arieh and Liora Freud have dated Stratum VII at Tell Ira to the first half of the seventh century (Beit-Arieh 1999, 174–176; Freud 1999, 195), suggesting that other culprits could have been responsible for its demise. And indeed, several potential candidates seem possible: local Edomite or Arab raiders, Kushites from Egypt, or even Assyrians as late as Esarhaddon or Assurbanipal (van der Veen 2020, 108).

2.4 A Faulty presumption based on deviant 14C dates:

Before we close, one final point needs to be addressed. Despite our earlier claim that in Transjordan seventh century dates for this pottery are based on epigraphic finds, recent 14C dates now seem to support earlier dates, in line with the 'pre-701 BC' dates in the Beersheba-Arad-Valley.[47] But these conclusions may be more apparent than real. Neil Smith *et al.* conducted surveys and sounding work at various smaller sites in the Edomite heartland, including at Khirbat al-Malayqtah, Khirbat al-Kur and Tawilan (Smith, Najjar and Levy 2014, 247–295). While date ranges at the two former sites are too broad to be useful (i.e. between *c.* 800–550 BC[48]), samples taken at Tawilan suggest higher dates than were previously envisaged. For one particular sample taken in Area J yielded a date between 890–785 BC with a probability factor of 95.4 % (Smith, Najjar and Levy 2014, 284–285). Without a doubt, this date clashes with former claims by archaeologists that the pottery at the site is largely 'homogenous throughout' (Bienkowski 1995, 48) and that Tawilan was basically a one-period site of the seventh–fifth centuries BC. For despite the 14C dates, the archaeological evidence at Tawilan still supports lower dates (van der Veen 2020, 115; Zerbst and van der Veen in this volume, pp. 373–399). Moreover, the discovery of an Achaemenid cuneiform tablet dated to the first year of a king called Darius (either Darius I or II) still confirms a *terminus ad quem* at Tawilan, sometime during the late sixth or fifth century BC. Likewise, anepigraphic seals at Tawilan also suggest a seventh to fifth century date (van der Veen 2020, 115).

Conclusions

What then shall we say? Whether or not one accepts that Lachish Level III itself was destroyed by Sennacherib in 701 BC (or some decades later, as Peter James suggests:

[46] Terra X-television documentary 'Sturm auf Jerusalem' (ZDF 2011) for which the present author worked as an academic consultant (pers. comm. with film director Renate Beyer).

[47] See van der Veen (2020, 112–115); see Tebes (in this volume, pp. 323–346) and Zerbst and van der Veen (pp. 373–399).

[48] As this author has argued, small finds at these sites suggest lower rather than higher dates (van der Veen 2020, 114). For instance, a disc shaped limestone palette with zigzag patterning and a limestone palette, whose shape is reminiscent of palettes in the form of a northern Phoenician goddess with a head and feathered headdress (Smith *et al.* 2014: 274–275, Figs. 4.24: A-I, and 3.25: B and G) point to a date well into the seventh or sixth century.

pers. comm.[49]; also see James on pp. 359–365), there can be little doubt that the citizens at contemporary Judaean sites that were not destroyed by the Assyrians in 701 BC, did not stop producing 'Lachish III' pottery, but rather would have continued to use and produce it (van der Veen 2020, chapters 2–3; Fantalkin and Tal 2015, 194). Moreover, if we accepted the conventional view that Lachish Level III rather than IV (as per James) was destroyed by Sennacherib (a conclusion that cannot as yet be proven one way or another), its floruit would belong to the early years of Iron IIB (rather than to its final years as in the conventional scheme), while other Iron Age IIB sites, that survived the 701-disaster, would have continued perhaps throughout Manasseh's reign (698–642 BC). For the Late Assyrian-style imitations and other small finds found in these strata indeed suggest that the beginning of Iron Age IIB cannot be dated much before the final years of Hezekiah's reign (*c.* 710–700 BC) when the Assyrians tightened their grip on the region. For instance, the 'Late Assyrian-style' bullae in the 'Rock-Cut Pool' debris in the 'City of David' were likely deposited there on the very eve before Jerusalem was besieged by the Assyrians in 701 BC while the latest pottery in the fill is either Iron Age IIA (late) or early Iron Age IIB, as pottery specialists admit.

The present author therefore believes that the finds presented in this article generally demand for lower dates, not only for the end of Iron Age IIB (down to *c.* 650 BC) but also for the end of Iron Age IIA, which as the Shema seal at Megiddo and the bullae in Jerusalem suggest, could be lowered to *c.* 730/722 BC in Israel and to *c.* 710/701 BC in Judah.

Abbreviations

WSS Avigad and Sass 1997

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[49] See especially James (2007, 214–216). James suggests that Sennacherib destroyed Lachish Level IV (Iron Age IIA) rather than Level III. The latter would then have been destroyed sometime during the first half of the seventh century, likely between *c.* 675–650 BC, whoever may have been responsible for its demise. For arguments in favour of both views, see van der Veen (2020, 171–176).

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In dubio pro reo: The Case of Sennacherib and the Beersheba Valley

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During his third military campaign in 701 BC, the Neo-Assyrian king Sennacherib devastated Judah and forced its rebel king Hezekiah to surrender. The campaign's itinerary is difficult to reconstruct with certainty, as the Neo-Assyrian royal inscriptions yield very little information about the conquered and destroyed settlements. While the biblical sources focus mainly on Jerusalem's deliverance from a physical conquest, the reliefs, which decorated the walls of Sennacherib's palace in Nineveh, only portray a detailed depiction of the conquest of Lachish. On the other hand, many sites in the Shephelah and the Beersheba-Arad Valley reveal destruction levels. The question must therefore be asked: which sites were destroyed by Sennacherib, and which were not? Did his military operations include the Beersheba-Arad Valley? If the destructions were not caused by Sennacherib, who then was responsible and when? By considering all the available sources, the role of Judah in the Neo-Assyrian Empire and the criteria for the composition of the Neo-Assyrian royal inscriptions will be scrutinised in this article.

Introduction

During his third military campaign in 701 BC, the Assyrian king Sennacherib devastated Judah and forced its rebel king Hezekiah to surrender. The campaign's itinerary is difficult to reconstruct with certainty, as the Assyrian royal inscriptions give very little information about the conquered and destroyed settlements. However, this traumatic event left its imprint on the archaeological remains in the form of destruction levels, thus providing one of two chronological anchors for the stratigraphy of Judaean sites in Iron Age II, the other one being the devastating military action in 587/86 BC by the Babylonian king Nebuchadrezzar II. In this context, the key archaeological site is Tel Lachish, a well excavated mound, whose destruction levels in strata III and II were dated to 701 BC and 587/86 BC, respectively.[1] Consequently, strata with pottery assemblages like those from Lachish III – especially with *lmlk* impressions – in other Judaean sites with destruction levels were attributed by most scholars to Sennacherib's campaign in 701 BC. Considering the evidence available at Lachish, the fine knowledge of the pottery in the region, and the great number of well-excavated sites, this is a logical conclusion, and indeed, it functions in most cases. However, lumping the Judaean sites all together may lead to wrong conclusions with regard to the settlements in the Beersheba Valley, as this region had very particular characteristics in the period under consideration.

In the case of a murder, in order to find the culprit, one needs, on the one hand, the body of the murdered and the weapon and, on the other hand, someone with a motive and the opportunity to commit the crime. In our case, the destruction levels in Judah are the *corpus delicti*, and undoubtedly Sennacherib had the opportunity in 701 BC. But, what about the Beersheba Valley? In this presentation, I will contest the *communis opinio* and try to exculpate Sennacherib from the charge of having also destroyed cities in the Beersheba Valley. I will focus my plea on who could have had a motive and the opportunity to do it. Considering all available sources, the Assyrian policy in the Southern Levant, the role of the Beersheba Valley at that time, and the information – or more precisely the lack of information – in the Assyrian royal inscriptions, I will try to shed light on these problematic questions. One should not expect an airtight solution, because the available sources only allow proposing probable or less probable scenarios, but I hope to show that it is worth asking the question, Did the Assyrian army destroy settlements in the Beersheba Valley in 701 BC?

The Written and Archaeological Evidence

Sennacherib's third campaign in 701 BC was neither a 'campaign against Judah' as it is usually referred to, nor

[1] Finkelstein (1994, 170); Thareani-Sussely (2007, 70).

an extraordinary event, but a routine military action to punish rebel vassals.[2] The campaign was carried out in three phases directed against the Phoenician city of Sidon, the Philistine cities of Ashkelon and Ekron and Judah, respectively. After dethroning Lulli from Sidon and Sidqa from Ashkelon and replacing them by loyal rulers (Tuba'alu and Sharru-lu-dari, respectively), the Assyrian army confronted a coalition of Philistine and Egyptian forces at the plain of Eltekeh. Thereafter, Sennacherib called Hezekiah of Judah to account for imprisoning Padi, the king of Ekron and a loyal Assyrian vassal, who was dethroned, put in chains, and sent to Judah by the Ekronites. Sennacherib's third campaign is referred to in many of his royal inscriptions,[3] but even the most detailed accounts from 700[4] and 691/689 BC[5] give very little information about the affected settlements in Judah. Sennacherib boasts to have conquered forty-six Judaean fortified cities, fortresses and the small settlements in their environs.[6] The only Judaean city mentioned in this context is Ursalimmu/Jerusalem.[7] The conquest of Tamnâ/Timnah, at the border between Judah and Philistia, is mentioned in a previous passage together with Altaku/Eltekeh.[8] A further important city, Lakisu/Lachish, the Judaean military stronghold, is attested once in a caption to the 'Lachish Reliefs' from Sennacherib's 'Palace Without Rival' at Nineveh (Room XXXVI, slabs 12–13),[9] where the conquest of this city is depicted in detail.[10] The three cities mentioned, namely Jerusalem, Lachish and Timnah, are only attested in Sennacherib's inscriptions. In fact, only one further Judaean city is mentioned in the Neo-Assyrian corpus, and that is Azaka/Azekah, which is attested once in a fragmentary inscription attributed to Sargon II or Sennacherib.[11]

The Biblical accounts about Sennacherib's military actions in Judah are not very helpful,[12] as, besides Jerusalem and Lachish, the only other site referred to as attacked by the Assyrians is Libnah in the Shephelah.[13] A passage from the book of Micah (1:8–15) may be interpreted as a lament over Sennacherib's devastating campaign, but, although most sites seem to have been located in the Shephelah,[14] besides Lachish only Mareshah (Tel Maresha) and Adullam (Khirbet esh-Sheikh Madhkûr) have been identified with certainty.[15] Thus, there is no positive evidence, neither in the Neo-Assyrian nor in the Biblical sources, that the Assyrian army operated in the Beersheba Valley in 701 BC.

The real dimension of the devastation caused by the Assyrian army in 701 BC in Judah becomes clear when we analyse the archaeological evidence. As mentioned above, the key site for the chronology of Judaean pottery during Iron Age IIB and IIC is Lachish. On the basis of a comparison with the pottery assemblage of Lachish III, the end of which can be attributed with great certainty to Sennacherib,[16] great number of sites with destruction levels could be identified (*Figure 1)*:[17]

[2] There is an enormous, disproportionate amount of secondary literature about this campaign. In this context I only refer to the most recent studies, where previous literature may also be found: Evans (2009); Ussishkin (2009); Kalimi and Richardson (2014); Dubovský (2016); Matty (2016); Borowski (2018); Kahn (2020).

[3] RINAP 3/1, no. 4, 32–58; no. 6, 1'–14'; no. 15, iii, 1–iv, 14'; no. 16, ii, 76–iv, 37; no. 17, ii, 58–iii, 81; no. 18, ii, 1''–iii, 31; no. 19, i', 1'–14'; no. 21, i', 1'–7'; no. 22, ii, 37–iii, 49; no. 23, ii, 35–iii, 42; no. 26. I, 9'–13'; no. 34, 13b–15; no. 42, 7b–11a; no. 44, 17–22a; no. 45, 1'–6'; no. 46, 18–32; no. 66 (caption); no. 67 (caption); no. 140, Obv. 15'–Rev. 21; no. 142, Obv. 1'–Rev. 8; no. 143, I, 1'; no. 165, iii, 1–46.

[4] RINAP 3/1, no. 4, 32–58 (Rassam Cylinder).

[5] RINAP 3/1, no. 22, ii, 37–ii, 49 (Chicago Prism).

[6] RINAP 3/1, no. 4, 49–50; no. 22, ii, 19–23.

[7] RINAP 3/1, no. 4, 48 and 52; no. 22, iii, 15 and 28; RGTC 7/1, 269–270.

[8] RINAP 3/1, no. 4, 46; no. 22, iii, 6–14; RGTC 7/1, 248 (Tamnâ).

[9] RINAP 3/2, no. 66; RGTC 7/1, 153.

[10] Nineveh, Southwest Palace, Room XXXVII, Pls. 5–8 (conquest) and 9–16 (booty): Barnett *et al.* (1998, Pls. 322–352). Studies concerning the Lachish reliefs: Ussishkin (1980, 1982, 1990, 2003); Battini (2019).

[11] RINAP 3/2, no. 1015, 5'; RGTC 7/1, 37.

[12] 2 Kings 18:13–19:36; Isa. 36–37; 2 Chron. 32:1–23.

[13] Isa. 37:8; 2 Chron. 19:8. Different sites have been proposed as plausible candidates for Libnah in the Shephelah, e.g. Tell Bornaṭ, Tell el-Judede, Khirbet Tell el-Beḍa and Khirbet Zeta el-Charab, see ABD 4, 322–323 and Mulzer (2010).

[14] For the geography and history of the Shephelah, see Finkelstein (1981) and Rainey (1983).

[15] See Rainey and Notley (2006, 243); Faust (2008, 181–182).

[16] Most scholars accept the destruction of Lachish III in 701, and there are no serious reasons to doubt it. The dating based on the archaeological work on the site is supported by the written and iconographical sources and fits the data; see discussion in van der Veen (2020, 172–176). The excavations by David Ussishkin confirmed 'beyond any doubt' that Stratum III was destroyed by Sennacherib (Finkelstein 1994, 171). The objections to this dating are based on assumptions and are merely speculative. A later date – presumably 648 – was proposed based on 2 Chron. 33:11–13, where an assumed military campaign by Assurbanipal and Manasseh's captivity in Babylon are mentioned (van der Veen 2020, 171–172, with literature and discussion). There is no evidence for such a military action in Assurbanipal's inscriptions. James (2007, 214–215) considers that 'the logic for dating of Lachish III's destruction to 701 is seriously flawed' because Lachish III was destroyed by fire and the burning is attested neither in Sennacherib's inscriptions nor in the reliefs. This is in fact no convincing argument as firstly, the conquest of Lachish is not mentioned at all in the Assyrian written sources, so that the necessary information is not available. Secondly, the reliefs focus on the siege and assault of the city, on what happened before the Assyrians got in and burnt it. James (2007, 216) also refers to the problematic relation between the two stages of construction of the siege ramp; for a possible explanation see Ussishkin (1990, 64–69). Even if there are still open questions concerning this matter, most archaeological evidence points to Sennacherib's ramp fitting its depiction in the reliefs.

[17] For an overview of the most important sites see Faust (2008, 169; 2012, 149–166; resettlement after 701) and Knauf (2002, 184–185).

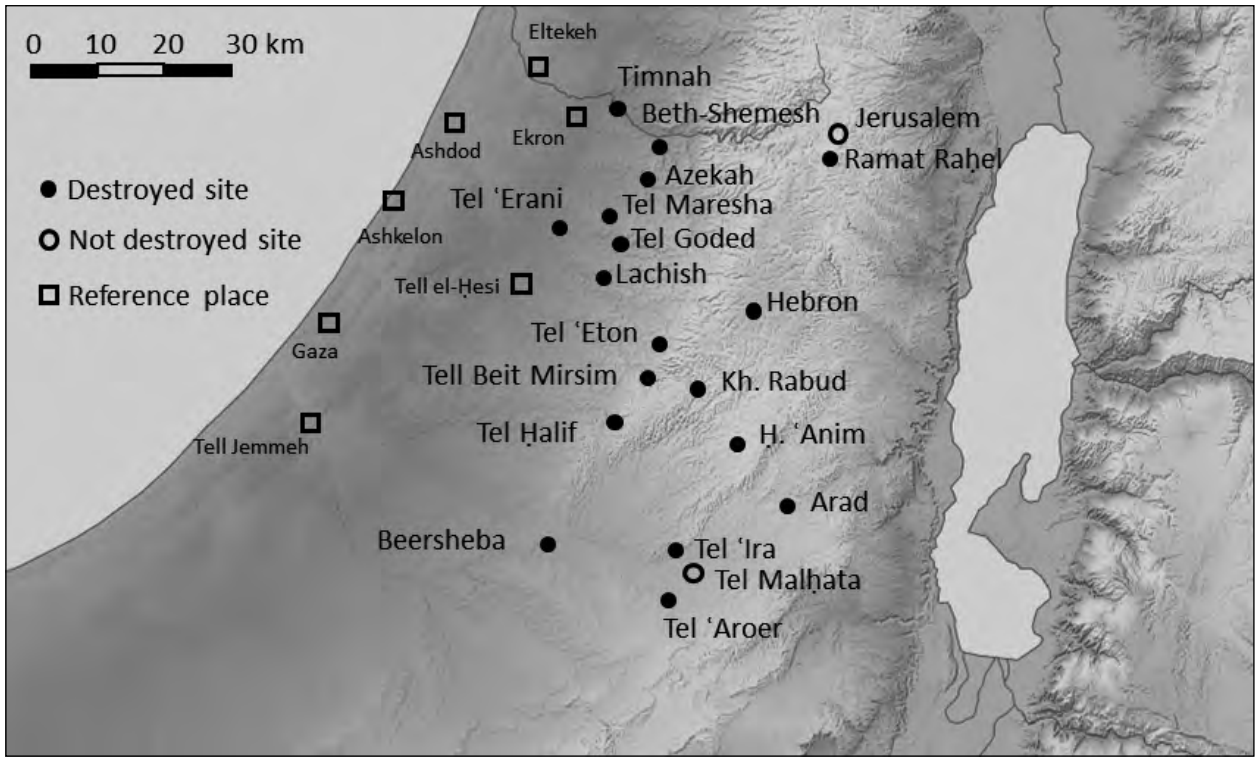

Figure 1: Places in Judah around 701 BC (credits: map by A. M. Bagg after map from Wikipedia Commons).

1) From north to south in the Shephelah: Timnah (Tel Batash),[18] Beth-Shemesh,[19] Azekah/Tell Zakariye),[20] Tel Erani,[21] Tel Goded,[22] Tel Maresha,[23] Tel Lachish,[24] Tel Eton,[25] Tell Beit Mirsim,[26] and Tell Halif.[27]

2) In the Judaean Highlands: Ramat Rahel,[28] Hebron,[29] and probably Khirbet Rabud[30] and Horvat Anim.[31]

3) In the Beersheba Valley: Tel Beersheba,[32] Tel Arad,[33] Tel Ira,[34] and Tel Aroer.[35]

Moreover, among the 276 sites where Lachish III pottery types were found, only 38 settlements dating from the late 7th to the early 6th century have been recorded, meaning a reduction of 70 percent of the total built-up area (from about 250ha to 80ha). Furthermore, about 85 percent of the sites in the Shephelah from the Iron Age IIB were not reoccupied in the Iron Age IIC.[36] Israel Finkelstein put it in a nutshell: 'the settlement system of the 8th century was virtually annihilated by Sennacherib'.[37] Beyond that, Sennacherib reduced the territory of Judah giving cities conquered in the

[18] Stratum III, 'partly destroyed during Sennacherib's campaign in 701 BCE', Mazar and Kelm (1993, 155) and further Mazar and Panitz-Cohen (2001).

[19] Stratum IIc, Bunimovitz and Lederman (2001, 144); Finkelstein and Na'aman (2004, 67–69); also Faust (2008, 173).

[20] Lipschits *et al.* (2015).

[21] Stratum VI, Brandl (1997, 257).

[22] Stratum IIB, Gibson (1994, 201); also Faust (2008, 173).

[23] Avi-Yonah and Kloner (1993, 950); Kloner and Eshel (1999, 150), quoted by Faust (2008, 173).

[24] Stratum III, Ussishkin (1977, 1982); the dating of the end of Lachish III is widely accepted but see above n. 16.

[25] Stratum I, Faust (2008, 173); Katz and Faust (2012).

[26] Stratum A, Finkelstein and Na'aman (2004, 61–64) contra Blakely and Hardin (2002, 14–24); Faust (2008, 172).

[27] Stratum IIA, Finkelstein and Na'aman (2004, 64) contra Blakely and Hardin (2002, 24); Faust (2008, 172).

[28] Aharoni (1993); Barkay (2006); Faust (2008, 178).

[29] Eisenberg and Nagorski (2002, 92*), quoted by Faust (2008, 178).

[30] Kochavi (1997); Faust (2008, 178).

[31] Faust (2008, 183).

[32] Stratum II, Aharoni (1973, 5–6), also Aharoni and Aharoni (1976, especially Table on p. 73), and also the following scholars: Herzog *et al.* (1984, 19); Singer-Avitz (1999, 11 and 58); Blakely and Hardin (2002, 26); Thareani-Sussely (2007, 70); Faust (2008, 169; 2012, 150); Hardin (2014, 749); Höhn (2016, 65, with reservation); Borowski (2018, 35–36), among others.

[33] Stratum VIII, Herzog (2002, 98); Thareani-Sussely (2007, 72); Faust (2008, 169); Höhn (2016, 94, with reservation); Frevel (2018, 297).

[34] Stratum VII, Beit-Arieh (1999, 176); Freud (1999, 226–227); Thareani-Sussely (2007, 71–72); Frevel (2018, 298).

[35] Stratum III: Thareani (2011, 306).

[36] Figures quoted by Finkelstein (1994, 173) according to the data presented by Dagan (1992; not available to me).

37] Finkelstein (1994, 173).

Western Shephelah to Mitinti of Ashdod, Padi of Ekron and Silli-Bel of Gaza. Judah was not only thoroughly devastated but it also lost its granary.[38] In complete contrast to the biased Biblical account, the campaign was successful and its goal was more than achieved.[39]

Before directing our attention to the Beersheba Valley and leaving this part of Judah aside, it is worth asking if Assyrian kings other than Sennacherib may have been responsible for the above-mentioned destruction of sites in the Shephelah and the Judaean Highlands. Blakely and Hardin (2002) proposed that some sites were destroyed first by Tiglath-pileser III and then by Sennacherib. They argued that the destruction levels at Tell Beit Mirsim (Stratum A), Tel ʿEton (Stratum II), Tel Halif (Stratum VIB) and Tel Beersheba (Stratum II), commonly attributed to Sennacherib's campaign, are rather to be interpreted as the result of Tiglath-pileser III's military actions in 734/33 BC, whereas Sennacherib would have been presumed responsible for later destruction levels. Finkelstein and Na'aman (2004) refuted this proposal and clearly showed that it is based on a wrong interpretation of the archaeological evidence. Moreover, there is no evidence for military actions against Judah in Tiglath-pileser III's inscriptions.

In the introduction to an inscription commemorating the construction of a palace in Kalhu (Calah), Sennacherib's father, Sargon II, calls himself the one 'who subjugated the land of Judah, whose location is far away'.[40] This expression may not necessarily refer to a military campaign with extensive destruction of settlements, but rather to the payment of tribute or to an increased payment.[41] However, the possibility of a campaign against Judah cannot be ruled out completely, and has in fact already been proposed by some scholars.[42] As the Juniper Palace Inscription dates probably from late 717 BC or early 716 BC,[43] the most likely date for a putative campaign against Judah is 720/19 BC, Sargon II's second regnal year, when he was in the region and came as far as Raphia. The corresponding passage of Sargon II's annals does not mention Judah at all, but it is badly broken.[44] Two further written sources have been adduced in relation to an assumed campaign against Judah in 720 BC: A difficult passage in the book of Isaiah[45] and the so-called Azekah Inscription, but the evidence is again inconclusive. Isa. 10:28–32 describes a route taken by an anonymous enemy approaching Jerusalem from the north and mentions several Judaean sites, which cannot be identified in all cases. Relating this episode to a possible action in 720 is only one of many possibilities and highly speculative.[46] The other alleged evidence, the Azekah Inscription, is a fragmentary text, which describes the conquest of the city of Azaqa/Azekah, as well as of a Philistine city whose name is not preserved.[47] It is a matter of debate, whether the inscription should be attributed to Sennacherib or to Sargon II.[48] By its content, the most probable candidate is Sennacherib;[49] by language and style, Sargon II,[50] as the text shows parallels to his 'Letter to the god Ashur'.[51] If the text describes a military action by Sargon II, it would explain his epithet as 'subduer of Judah' in the Juniper Palace Inscription. However, the text may be a report to the god Ashur about Sennacherib's campaign in 701. Sargon II's letter to Ashur was written in 714 BC or shortly thereafter, only 14 years before Sennacherib's campaign, so that it is plausible to assume, that the same scribes [or at least scribes from the same scribal school] were at work, explaining the similarities in language and style. Summing up, there is no certain evidence that Sargon II campaigned against Judah.

Assuming that, if not all, most of the Judaean sites were destroyed by Sennacherib in 701, there is a distinct concentration of destruction levels in the Shephelah, while very few are attested in the Judaean Highlands (Ramat Raḥel, Hebron, Khirbet Rabud and Ḥorvat Anim). There cannot be any doubt about the great devastating effect of Sennacherib's campaign, and it makes sense that the Assyrian king focused on the Shephelah, the granary of Judah and its most densely settled area.[52] Furthermore, the geography of this region presented no obstacles to the Assyrian army. It was hard, but routine work at the end of a long campaign. But, what about the Beersheba Valley? Of the four fortified sites (Tel Beersheba, Tel Aroer, Tel Ira and Tel Malhata) and one fort (Tel Arad) occupied in the late 8th century, four sites, namely Tel Beersheba (Stratum II), Tel Ira (Stratum VII), Tel Arad (Stratum VIII) and Tel Aroer (Stratum III) clearly show a relevant destruction level.[53]

[38] Finkelstein (1994, 177).

[39] Fales (2014, 248).

[40] *Mušakniš* ^{kur}*Iaudu ša ašaršu rūqu*, RINAP 2, No. 73, 8.

[41] Tadmor (1958, 39, end of n. 146), followed by Sweeney (1994, 467–468) and Younger (1996, 108–109).

[42] Younger (1996; 2003, 237–240).

[43] Frame (2021, 320–321) following to Na'aman (1994).

[44] RINAP 2, No. 1, 23b–57 (ll. 27–52 are missing).

[45] Isa. 10:28–32.

[46] Sweeny (1994) comments on the different options and pleads for Sargon II's campaign in 720.

[47] RINAP 3/2, no. 1015.

[48] For a summary of the different positions see Frame (2021, 350–351).

[49] Na'aman (1974).

[50] Fuchs (1994, 314–315) and Frahm (1997, 229–232; with discussion of the different dating proposals), see also Frahm (2018, 67–68).

[51] RINAP 2, no. 65.

[52] The four geographical regions in Judah were the Judaean Highlands, the Shephelah, the Judaean Desert and the Beersheba Valley, Finkelstein (1994, 172).

[53] For a good summary of the archaeological finds in the Beersheba Valley see Thareani-Sussely (2007).

On the basis of assumed similarities with the ceramic assemblage of Lachish III, the excavator of Tel Beersheba dated the end of Stratum II, which was destroyed by conflagration, to 701.[54] This dating is widely accepted among scholars,[55] and serves as the starting point of L. Singer-Avitz's seminal article about the pottery of the site.[56] Stratum VII from Tel Ira, which also ended in destruction,[57] was dated by the excavators to the end of the 8th or the beginning of the 7th century.[58] The ceramic assemblage of this stratum shows similarities with that of Beersheba II and Lachish III, and its destruction was also dated to 701.[59] According to its excavator, Stratum VIII in Tel Arad was destroyed in a conflagration, and again the pottery assemblage parallels that of Beersheba II[60] and Lachish III.[61] Therefore, the end of this stratum was also related to Sennacherib's military actions in 701.[62] The destruction of eighth-century settlements in the Beersheba Valley was not complete and, with the exception of Tel Beersheba, all sites were reconstructed not long after their destruction.[63]

The dating of the destruction of Beersheba II, Tel Ira VII and Tel Arad VIII to 701, as well as the parallels with the pottery assemblages of Lachish III, are widely accepted. However, there were some critical voices.[64] Already in 1976, K. Kenyon pointed out some differences between the ceramic types in Beersheba II and Lachish III, suggesting that the pottery of Stratum II, and consequently its destruction, fits better the period between Sennacherib's and Nebuchadrezzar II's campaigns. In a more detailed article, E. A. Knauf arrived at a similar conclusion, suggesting that the ceramic typology of Beersheba II 'enhances the impression ... of a destruction after 701, but not too long after 701'.[65] The main arguments are, firstly, that storage jars typical of Lachish II can be traced back to types found at Beersheba II, but are missing at Lachish III.[66] Secondly, while sites with destruction levels and *lmlk*-impressions are well attested in the Shephelah and the Judaean Highlands,[67] very few *lmlk*-seal impressions were found in the Beersheba Valley.[68] *Lmlk*-stamped storage jars are indeed a key indicator, as they were centrally distributed to Judaean sites by Hezekiah as part of the provisions taken before the Assyrian onslaught of 701.

Whether these objections are relevant enough to contest the dating of Beersheba II and its destruction, is a question for specialists in the Iron Age pottery of the Southern Levant. However, the discussion concerning the pottery assemblages shows two important points: firstly, not all sites with a ceramic typology similar to that of Lachish III have necessarily been destroyed by Sennacherib, and secondly, that Lachish III was destroyed by Sennacherib does not mean that the corresponding pottery types disappeared after 701.[69] In other words, even sites with a ceramic assemblage similar to that of Lachish III may have been destroyed sometime before or after 701. Even for the *lmlk* system, it has been proposed that it was already initiated in the last quarter of the 8th century (early *lmlk* types) and continued during the first quarter (late *lmlk* types) and the middle (the concentric circle incisions) of the 7th century.[70]

Looking for the Culprit

Even if the differences in the pottery assemblages may point to an earlier or later destruction of the sites in the Beersheba Valley, they alone are not a compulsory argument for an alternative dating. But there is a substantial argument that has been overlooked in the discussion. There is a wide consensus about the fact that the Beersheba Valley played an important role in the Arabian international trade from the middle of the 8th century on, connecting the routes coming from the Arabian Peninsula with the Philistine ports in the Mediterranean.[71] The trade caravans travelled through the territories of Edom, Judah and Philistia, all regions under Assyrian control, and it is widely accepted that Assyria profited from this trade.[72] Furthermore, this flourishing trade was seen as the principal motivation for the Assyrian interest in the region.[73] In fact, all participants, namely the Arabian traders, the Edomites, the cities in the Beersheba Valley, Judah, the

[54] Aharoni (1973, 5–6), also Aharoni and Aharoni (1976, especially Table on p. 73).

[55] Herzog *et al.* (1984, 19); Thareani-Sussely (2007, 70).

[56] Singer-Avitz (1999, 11).

[57] Finkelstein and Beit-Arieh (1999, 76).

[58] Beit-Arieh (1999, 176); Freud (1999, 226–227).

[59] Thareani-Sussely (2007, 71–72).

[60] For reasons of simplicity – and following Knauf (2002, 181, n. 3) – I will refer to Tel Beersheba/Tell es-Seba mostly as 'Beersheba'. The ancient name of this site is still unknown.

[61] Herzog (2002, 98).

[62] Herzog (2002, 98); see further Thareani-Sussely (2007, 72).

[63] Thareani-Sussely (2007, 73 and 75).

[64] See discussion in van der Veen (2020, 171–178).

[65] Knauf (2002, 191); see also Zwickel (2021, 245–246).

[66] Knauf (2002, 183–184).

[67] Knauf (2002, 184–186) with figures and literature.

[68] Only sixteen according to Thareani-Sussely (2007, 72–73 and n. 9 on p. 75): Tel Arad (9), Tel Aroer (5), Beersheba (1) and Tel Ira (1).

[69] Thareani-Sussely (2007, 70); Fantalkin and Tal (2015, 194).

[70] Lipschits (2012, 11).

[71] On the trade through the Beersheba Valley, see Höhn (2016, 102–139, esp. 124–126).

[72] Singer-Avitz (1999, 55); Knauf (2002, 181 and 192); Thareani-Sussely (2007, 75); Bagg (2010, 208–209; 2018, 256).

[73] Finkelstein (1992, 159); Singer-Avitz (1999, 57).

Philistines and the Assyrians, profited from the trade with aromatic products (and also probably copper from the Araba), and all of them must have been interested in a fluid development, avoiding and preventing any possible interruption or disturbance. The sites in the Beersheba Valley served as way stations along the trade route and supplied the necessary services.[74] The vassal kingdom of Judah protected the caravans passing through its territory by establishing a fortified settlement system, and local written sources attest to Judaean involvement.[75] Therefore, why should Sennacherib, after having devastated Judah, have been interested in destroying sites in the Beersheba Valley? Why should Sennacherib or any other Assyrian king kill the goose that laid the golden eggs? There is simply no motive for doing so, and, if we consider that an assumed incursion into the Beersheba Valley would have taken place at the end of a long campaign including dozens of sieges and destroyed settlements – when the army was tired, the region supposed to feed it completely devastated,[76] and the goal reached – we have a case against the assumed dating to 701.

The next question is, if not Sennacherib, who else? Let us consider who had the opportunity and the motive. As already said, it is not plausible that an Assyrian king would deliberately attack and destroy the Judaean fortified settlements in the Beersheba Valley. However, it is useful to know who among them had, if not the motive, at least the opportunity. Besides Sennacherib, the only Assyrian king who may have campaigned in Judah is Sargon II in 720, as discussed above. Other kings were not far from the Beersheba Valley, when they took the coastal route: Tiglath-pileser III reached Raphia and, further south, the Brook of Egypt in the Sinai in 734 during his campaign against Philistia;[77] Esarhaddon plundered in 679 the city of Arza[78] in the district of the Brook of Egypt,[79] threw its king Asuhili into fetters and brought him to Nineveh;[80] he was also in the region on his way to Egypt in 674, 671 and 669 BC; Assurbanipal followed the same route to Egypt in 667 and 664 BC, and campaigned in Transjordan pursuing the Arabs in 652 BC.

Leaving aside the Assyrians, there are at least four other potential candidates who had the opportunity to attack Beersheba and the other sites in the valley. Edom, the neighbour in Transjordan, had the opportunity to launch an attack into the Beersheba Valley during the 7th century, but lacked a motive for the same reason as did Assyria: there was no wish to interfere with the profitable Arabian trade. Furthermore, Edom was a loyal Assyrian vassal, who, as far as the sources tell us, never acted against Assyria, and to attack Judah, another Assyrian vassal, would have called forth a severe retaliation, for which there is no trace in the written sources. Moreover, the attested Edomite pottery in Judaean sites in the Negev or at the cultic site of Horvat Qitmit, probably a cult place for Edomites dwelling in the Negev, rather points to friendly interactions between Edom and Judah during most of the 7th century.[81]

The second potential aggressor in the region is Egypt, but there exists no concrete evidence for an onslaught directed against the Beersheba Valley. The Assyrian policy of buffer states (Judah, Edom) and military outposts (e.g. Tell Jemmeh) on the south-western border of the empire, was efficient, and up to the battle of Eltekeh there was no direct confrontation.[82] However, between the end of Sennacherib's reign and before the Assyrian conquest of Egypt – which began in 679, was effective in 671, and lasted up to the middle of the 7th century[83] –there is a narrow time frame (683–679 BC) for Egyptian activities in the southern Levant, as proposed by D. Kahn.[84] But it must be stressed that an Egyptian military campaign in the Beersheba Valley remains hypothetical. The Egyptians had a motive but a narrow time frame for an opportunity. Further military actions are imaginable in the period between the decline of the Assyrian power about the middle of the reign of Assurbanipal up to the Babylonian conquest, but the late 7th or early 6th century seems to be too late to fit the actual chronology of the pottery in the Southern Levant. The same applies to the Babylonians who had a

[74] Singer-Avitz (1999, 60).

[75] Thareani-Sussely (2007, 75). A sample of sources from Tel Arad can be found in Weippert (2010, 352–364); for a comprehensive publication of the texts from Tel Arad see Aharoni and Naveh (1981).

[76] Knauf (2002, 189).

[77] RINAP 1, Tigl III, no. 42, 8'–15'; no. 48, 14'–19'.

[78] RGTC 7/1, 29–30.

[79] RGTC 7/1, 5–6 (Al-Nahal-Musur) and 291 (Nahal Musur).

[80] RINAP 4, no. 1, iii, 39–42.

[81] Finkelstein (1992, 158–159). The Hebrew Bible does not record any struggles between Judah and Edom for 150 years between the reign of Ahaz and the destruction of Jerusalem by the Babylonians. The negative image of Edom in the prophetic books refers to a later period, as does the famous Ostracon no. 24 from Arad (late 7th–early 6th century), Singer-Avitz (1999, 9).

[82] Both sides, the Assyrians and the Egyptians, claimed a victory at Eltekeh, Kahn (2004, 109; 2014). The Assyrian victory at Eltekeh was also questioned by Knauf (2002, 189–190). However, as Sennacherib continued his operations after that battle and as there is no evidence that Assyrian fighting power was diminished, the result of the open-field battle must have been at least a draw. Moreover, while the scarce information about the battle in the Assyrian inscriptions could be interpreted as a reference to a defeat, the battle seems to have been depicted on slabs 27–23 that decorated the throne-room of Sennacherib's Southwest palace in Nineveh (Vidal 2012), and it seems implausible that the Assyrians would have presented a defeat in such a representative place.

[83] 'It is impossible to know when the Assyrians finally left Egypt and under what circumstances', Kahn (2005, 266).

[84] Kahn (2004).

motive and the opportunity during Nebuchadrezzar II's campaigns between 604 and 587/86 BC, but also in this case the date is not compatible with the chronology of the sites.[85]

The fourth and most plausible candidates responsible for the destruction levels in the Beersheba Valley are Arabs not directly involved in the international trade, as has already been suggested by E. A. Knauf.[86] They had both a motive and the opportunity. A great number of Arab tribes are attested in the Assyrian sources, which I have dealt with elsewhere.[87] In fact, we know from Assurbanipal's records that some Arab tribes under the command of the Qedarite leader Uaite plundered territories under Assyrian control in the West, leading to the only military operation in Transjordan as far as Edom in 652 BC.[88] Moreover, the same inscription explicitly mentions that another Qedarite leader named Ammu-ladin attacked Assyrian vassals in the Southern Levant[89]. Thus, we have concrete evidence of Arab tribes making trouble in the region.[90] Such tribal leaders were not afraid to interfere with the Assyrian interests in the Southern Levant, and the road stations in the Beersheba Valley could have been an attractive prey. An Arab onslaught in the first half of the 7th century would be compatible with the archaeological evidence and the corresponding chronology. That Esarhaddon's punishment of Asuhili (probably an Arab name[91]), the ruler of the city of Arza in 679 BC, was triggered by his involvement in the destruction of sites in the Beersheba Valley, as has been proposed, is a possibility, but there exists no conclusive evidence.[92]

Even if an Arab tribe seems to be the most plausible culprit, it must be said that also in this case there are open questions. Were Beersheba II, Ira VII, Arad VIII and Aroer III destroyed in one coup by the same tribe or on various occasions by different tribes? Was it possible for a nomadic tribe to destroy a small but fortified settlement? Would it not have been easier to attack the caravans? Another important point is the expected Assyrian retaliation. Was Esarhaddon's plunder of Arza in 679 BC the Assyrian reaction? Did the destructions in the Beersheba Valley happen at a time when Assyria was no longer able to react, namely in the last 20 years of Assurbanipal (third quarter of the 7th century) or in the last 15 years of the empire (last quarter of the 7th century)?

Did Sennacherib destroy settlements in the Beersheba Valley? I admit that I cannot present a smoking gun for a bone-crushing 'no', and that the proposed alternative scenario is only a proposal. However, I hope to have shown that there are enough arguments to free Sennacherib from the blame of having destroyed Beersheba II, 'Ira VII and Arad VIII. *In dubio pro reo.*

Abbreviations

ABD	The Anchor Bible Dictionary
PNA	The Prosopography of the Neo-Assyrian Empire
PNA 1/1	Radner 1998
RGTC	Répertoire Géographique des Textes Cunéiformes
RGTC 7/1	Bagg 2007
RINAP	The Royal Inscriptions of the Neo-Assyrian Period
RINAP 1	Tadmor and Yamada 2011
RINAP 2	Frame 2021
RINAP 3/1	Grayson and Novotny 2012
RINAP 3/2	Grayson and Novotny 2014
RINAP 4	Leichty 2011
RINAP 5/1	Novotny and Jeffers 2018
SAA	State Archives of Assyria (Helsinki)
SAA 1	Parpola 1987
SAA 18	Reynolds 2003
SAA 19	Luukko 2012
SAAB	*State Archives of Assyria Bulletin*
SAAS	State Archives of Assyrian Studies
SAAS 8	Fuchs 1998

[85] Beersheba II (= Stratum Lachish III) was destroyed and not rebuilt, but Aroer III, Ira VII and Arad VIII (all = Lachish III) were rebuilt after the destruction, as the following layers Aroer II, Ira VI and Arad VII (all = Lachish II) attest.

[86] Knauf (2002, 191).

[87] Bagg (2010, 2018).

[88] RINAP 5/1, no. 11, vii, 82–viii, 14. The mention of Sa'arri (viii, 112) – commonly identified with Biblical Seir (RGTC7/1, 205) – among the places of the Arabian rebellion may be an additional argument for the proposed destruction of sites in the Beersheba-Valley by the Arabs. According to Tebes (2021, 73) Sa'arri may denote the Negev.

[89] RINAP 5/1, no. 11, viii, 15–29.

[90] A letter from the governor of Ṣimirra, Qurdi-Ashur-lamur, to Tiglath-pileser III explicitly mentions that Qedarites attacked and defeated Moab, SAA 19, 29. According to a letter from Nabu-shumu-lishir, an officer active in Babylonia, to Esarhaddon, a caravan of the Nabayateans was attacked and looted by a Mas'aean tribe, SAA 18, 149. In another letter he reports that Assyrians coming to trade at the Babylonian city of Birtu were attacked and kidnapped by Arabs, SAA 18, 148. Incidents related to Arabs plundering cities in other regions are also attested in the royal correspondence of Sargon II (SAA 1, 82 [Ḫindānu] and 84 [Sippar]); further problems with Arabs are reported in SAA 1, 178 and 179.

[91] *PNA 1/I*, 139 and Knauf (2002, 192, n. 65).

[92] Knauf (2002, 191–192). That Asuhili was a descendant of one of those tribal leaders appointed by Tiglath-pileser III (Idibi'ilu, RINAP 1, no. 22, 13'; no. 42, 34'; no. 44, 16'; no. 47, Rev. 6'; Siruatti, the Me'unite, no. 48, 22' and possibly no. 44, 15'–16', for the restoration see RINAP 1, p. 111, comment to lines 15'–16') or Sargon II (a tribal leader of the city of Laban, RINAP 2, no. 63, ii', 7'; no. 82, iii, 2') to guard the south-western border (Knauf 2002, 192, n. 65), is also speculative.

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The Chronology of Edom in the Iron Age: A Critical Review of the Evidence

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The chronology of the Iron Age southern Levant is constructed like a half-built jigsaw puzzle, with the few synchronisms, made of joint pieces of written records and archaeological data, sitting next to large empty spaces of unknowns. This is most poignant for the chronology of ancient Edom, the most geographically peripheral of the Iron Age Transjordanian polities. During the last decades, new archaeological excavations in the Faynan lowlands and novel analyses of the 'Edomite' pottery found in the northern Negev have greatly enhanced our knowledge, both extending the archaeological chronology of Edom back to the early Iron Age and refining that of the last century of Edom's existence. However, problems in the chronology of Edom still abound, to the extent that it is still difficult to establish a most basic chronological frame and geographical setting for its history. This article presents a re-evaluation of the synchronisms between the few written Egyptian, Neo-Assyrian and Neo-Babylonian sources referring to Edom and the local archaeological and epigraphic evidence. It studies critically the relationship between the external historical sources and the local evidence – two different sets of data that are often analysed as entirely comparable entities.

Introduction

Recent excavations in the lowlands of Faynan in southern Jordan have reignited the debates over the chronology of the region known as Edom (see *Figure 1*). The history of Iron Age Edom is now divided into two phases of settlement, although their dating and the relationship between them are still hotly debated: an 'early' phase with settlements in the copper region of Faynan, traditionally dated to the 10th–9th centuries BC; and a 'late' phase with settlements in the Edomite highlands, usually dated between the late 8th–mid 6th centuries BC. This article will present a critical re-evaluation of the main archaeological and epigraphic data used as chronological anchors, suggesting lower dates for both phases. For the 'early' phase, the research will particularly reassess the interpretation of the Bayesian radiocarbon dates from Khirbet en-Nahas and the nature and dating of the Egyptian amulets found at Faynan (used to suggest synchronism with pharaoh Shoshenq I's campaign). For the 'late' phase, the research will re-evaluate the evidence of the Neo-Assyrian, Egyptian, Neo-Babylonian and Edomite military interventions in the Negev, the dating of the Assyrian-influenced 'Edomite' pottery and the references to Edom in the Hebrew ostraca from Tel 'Arad.

Edom in the Iron Age IIA–B

External written sources

The earliest possible attestations of the names Edom and Seir (both names are closely associated in later biblical sources) appear in inscriptions dating to the Late Bronze Age (*Figure 2: nos. 1–13*). New Kingdom Egyptian sources depict the Shasu Edomite and Seirite groups as semi-pastoral nomadic groups living within the Egyptian sphere of influence (Sinai Peninsula, Negev) rather than in distant southern Transjordan (Kitchen 1992, 26–27; Tebes 2021).[1]

Two suggested possible references to Edom appear in the triumphal scene of the 22nd Dynasty pharaoh Shoshenq I (946–925 BC; for a later date, see James and van der Veen 2015, 127–136) in Karnak (*Figure 2: no. 15*). The first occurrence is the name *'idmm*, which appears twice; Noth (1938, 295) and Helck (1971, 243) proposed translating it as Edom. Most recently, Wilson (2005,

[1] During the Late Bronze Age, the name Edom was not exclusively used for the region of southern Transjordan. There are a few references in Egyptian, Ugaritic and Canaanite sources to place-names called 'Edom' or alike located in northern Canaan, although they most likely were completely unrelated to the southern Transjordanian Edom (Tebes 2021).

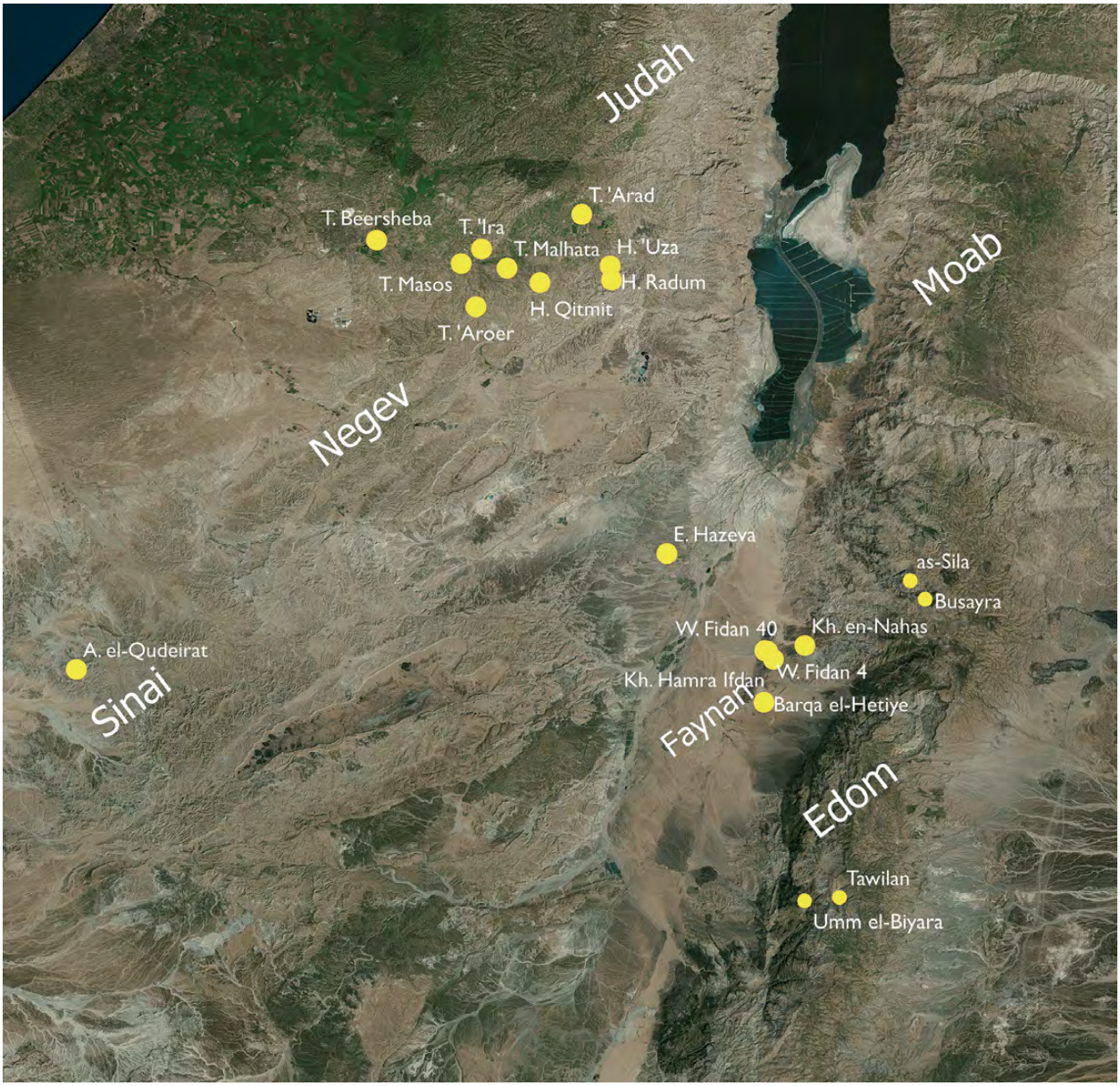

Figure 1. Bing Map with main sites mentioned in the article. © 2021 Microsoft.

125) noted that this reading is possible on phonological grounds, although there is no other toponym in the list that refers to an entire country. Lipiński related this name to Thutmose III's *'itmm* and was sceptical about the Edom connection, but for chronological, not philological, reasons.[2] He suggested, following the root *'ṭm* in Mishnaic Hebrew and Aramaic, to identify the name as a reference to a kind of hydrological structure or fortified place (Lipiński 2006, 118). It has been proposed that a third name, usually completed *idr* (Adar), 'easily allows restoration' to Edom (Dijkstra 2016, 86).

From a geographical point of view, these references are mentioned together with place names located in the Negev (Shoshenq List row VI, nos. 66–150 and XI,1a-5), with likely allusions to Arad, Sharuhen, Yursa, Raphia and Laban, including several compound names with the term *n-g-b* (Kitchen 1973, 439–442; Wilson 2005, 118–133; Morenz 2007, 111–117; Dijkstra 2016, 86). Despite early attempts to locate some of these place names in southern Transjordan (Albright 1937–1939, 385–386; Noth 1938, 301–303; Aharoni 1979, 327–330), few if any of these identifications have stood the test of time. The identification of the name *g-b-r-i* (VI, 74) with biblical Ezion-Geber (Mazar 1957, 65; Aharoni 1979, 328) is by no means certain (Kitchen 1973, 439; Lipiński 2006, 382). If the references to Edom in the Shoshenq I list are correct, it appears that they portray encounters

[2] '"Edomites" as toponym or tribal name in the 10th century B.C. is certainly no plausible guess' (Lipiński 2006, 118).

No.	*Text*	*Translations*	*Conventional Date*	*Synchronisms*	*Source*	*Publications*
1	*ỉ-t-m-m* *š-m-š ỉ-t-m*	Adamim Shemesh-Edom	15th cent. BC	Thutmose III	Karnak topographical list	Simons 1937, I:36, 51; *ANET* 243; Ahituv 1984, [53]
2	*š-m-š ỉ-t-m*	Shemesh-Edom	late 15th cent. BC	Amenhotep II	Memphis Stela	*ANET* 245; Ahituv 1984, [174]
3	*š-m-š ỉ-t-m*	Shemesh-Edom	late 15th cent. BC	Amenhotep II	Karnak Stela	*ANET* 245; Ahituv 1984, [174]
4	URU *Ú-du-mu*	the city of Udumu	mid-14th cent. BC	ND	Amarna Letter 256	Rainey 2015, 256: 24
5	KUR.ḪI.A *Še-e-ri*	the mountains of Seir	14th cent. BC	Abdi-Heba of Jerusalem	Amarna Letter 288	Rainey 2015, 288: Obv. 26
6	*Ḏw-n(.y)-S[ʿr*	Mountain-of-S[eir	13th cent. BC	Ramesses II	Gebel Shaluf Stela	*KRI* II, 303:6; Cooper 2020, 516–517
7	*Ḏw-n(.y)-Sʿr*	Mountain-of-Seir	13th cent. BC	Ramesses II	Tanis Obelisk	*KRI* II, 409:1; Cooper 2020, 516–517
8	*t3 š3św-Sʿr*	Shasu land of Seir	13th cent. BC	Ramesses II	Amara West topographical list	*KRI II*, 217:10.92; Cooper 2020, 518–519
9	*mhw.t š3s.w n(.y) ʾi/dm*	the tribes of the Shasu of Edom	*c.* 1206 BC	Merenptah	Papyrus Anastasi VI	Gardiner 1937, Anast. VI. 54–55; *ANET* 259; Cooper 2020, 501
10	*ʾi-d-m-m*	Adummim	end of the 13th cent. BC	ND	Papyrus Anastasi I	Gardiner 1911, 22:1; *ANET* 477
11	*Ršpw ḥnʿJ-tw-mʾ*	Rešep and Adum/Edom, Adamma	New Kingdom	ND	Magical Papyrus Leiden I 343+345	Beck 2018, 13 R: V7/V: VIII5; Ahituv 1984, [175]
12	*ỏdm rbt* *ỏdm trrt* *ỏdm ytnd/*[*t*] *ỉl* *ktnḥm ỏdm*	great Udm well-watered Udm Udm the gift of El people of Udm	13th–12th cent. BC	ND	Ugarit tablet, Keret epic	Gibson 2004, 14:iii.108–109, 134, 135, iv. 210, 211, [v.257–258], vi. 276–278; 15: i.7
13	*Sʿr.w m mhw.t / Š3s.w*	Seir(ites?) in the families of the Shasu	12th cent. BC	Ramesses III	Papyrus Harris I	*ANET* 262; Cooper 2020, 518–519
14	*n3y Sʿr*	Seir(ites), those of Seir	late 20th–22nd Dynasties	ND	Papyrus Pushkin 127, A Tale of Woe	Cooper 2020, 518–519

Figure 2a. Suggested references to the names Edom and Seir in Late Bronze and Iron Age sources.

No.	*Text*	*Translations*	*Conventional Date*	*Synchronisms*	*Source*	*Publications*
15	*ʾi-d-m-m* *i̯-d-r*	Edom, Edomites? Adumim, Adar	late 10th cent. BC	Shoshenq I	Karnak topographical list	Simons 1937, XXIV: 98, 128, 116; Ahituv 1984, [54]; Ritner 2009, 208
16	KUR.*ú-du-mu*	Edom	*c.* 796 BC	Adad-nirari III	Nimrud slab	*ARAB* 1, 739; *ANET* 281; RIMA 3, 8.12
17	^{m}*qa-uš-ma-la-ka* KUR.*ú-du-mu-a-a*	Qausmalak of Edom	734–732 BC	Tiglath-pileser III	Nimrud tablet	*ARAB* 1, 801; *ANET* 282; RINAP 1, 47.rev.11’
18	KUR.*ú-*⌜*du*⌝*-*[*me?*]	[Ed]om	*c.* 713 BC	Sargon II	Nineveh prism (Prism A)	*ARAB* 2, 195; *ANET* 287; RINAP 2, 82.vii.26’’
19	KUR.⌜*ú*⌝*-du-*⌜*mu*⌝*-a.a*	Edom, the Edomite	*c.* 721–712 BC	Sargon II	Nimrud letter, report on a festival	CTN 5, ND 2765:41; SAA 1, 110.r.12 and 19, 159.r.12
20	⌜*ú*⌝*-d*[*u-u*]*m-mu*	E[d]om	8th cent. BC	presumably Sargon II or Sennacherib	Sargon geography	Horowitz 1993; 1998, Sargon Geography 49.B.rev.8’
21	^{md}*a-a-ram-mu* KUR.*ú-du-um-ma-a-a*	Aiarammu of Edom	701 BC	Sennacherib	Nineveh, Nimrud and Assur cylinders and prisms: Cylinder B; Cylinder C; King Prism or Heidel Prism; Chicago Prism and Taylor Prism; Jerusalem Prism	ARAB 2, 239; *ANET* 287; RINAP 3/2, 4.37; 16.iii.22; 17.ii.82; 22.ii.57; 23.ii.54; 140.r.1; 142.o.1’; 165.iii.2
22	^{m}*qa-uš-gab-ri* LUGAL URU.*ú-du-me*	Qausgabri king of Edom	673 BC	Esarhaddon	Nineveh Prism A; Nineveh Prism B	*ARAB* 2, 690; *ANET* 291; RINAP 4, 1.v.56; 5.vi.[8’]
23	^{m}*qa-uš-gab-ri* LUGAL KUR.*ú-du-me*	Qausgabri king of Edom	667 BC	Assurbanipal	Cylinder C	*ARAB* 2, 876; *ANET* 294; RINAP 5, 6.ii.28’
24	*lqwsg[br] mlk ʾdm*	belonging to Qos-Ga[br], king of Edom	670s–660s BC	ND	Umm al-Biyara bulla	van der Veen 2011, 79-81; *WSS* 1049
25	*lqwsgbr* [*mlk ʾd*]*m*(?)	belonging to Qos-Gabr, [king of Edo]m(?)	670s–660s BC	ND	Scaraboid from Babylon	*WSS* 1048

Figure 2b. Suggested references to the names Edom and Seir in Late Bronze and Iron Age sources.

No.	*Text*	*Translations*	*Conventional Date*	*Synchronisms*	*Source*	*Publications*
26	*ina gi-ra-a* URU.*a-z/ ṣa-ar*-DINGIR URU.*ḫi-ra-ta-a-qa-z/ ṣa-a-a ina* URU.*ú-du-me* *ina* URU*sa-'a-ar-ri*	in the girâ of Az/ṣarilu, Ḫiratâqaz/ṣaya in Edom in Seir	*c.* 650 BC	Assurbanipal	Rassam Cylinder (Prism A)	*ARAB* 2, 818; *ANET* 298; Borger 1996, 7:108–109; RINAP 5, 11.vii.108–109, 112
27	[KUR.]*ú-du-ma-a.a* [KUR].*ú-du-ma-*⌜*a*⌝.*a*	Edom	late 8th–7th cent. BC	presumably Tiglath-Pileser III or Sargon II	Wine list	CTN 3, 135.o.7, o.12
28	[…MA].⌜NA⌝ KUG.UD / […] / […] (KUR[*u-du-ma*]-*a-a*)	[…mi]nas of silver from [Edom]	7th cent. BC	between Sargon II and Esarhaddon	Nineveh tablet, receipt of tribute	*ANET* 301; SAA 11, 33.o.7–10
29	URU.*ú-du-u-mu*	Edom	7th cent. BC	ND	Nineveh tablet, lexical list	SAA 11, 1.ii.11
30	*yd'th* [*hmktbm m*] / *'dm* *'thr'h.'š* [*r*] *'d*[*m 'sth*]	you knew [about the letters from] Edom the evil that Edo[m has done]	late 8th or 7th cent. BC	(unnamed) king of Judah	Tel 'Arad ostracon 40	Aharoni 1981, 40: rev. 9–10, 15
31	*'.pn.tb'.'dm.wmh*	lest Edom should come there	7th cent. BC	(unnamed) king (of Judah?)	Tel 'Arad ostracon 24	Aharoni 1981, 24: 20
32	*w'dmm*	and Edomites	7th cent. BC	ND	Tel 'Arad ostracon 3	Aharoni 1981, 3:12
33	*'dm ḫyh*	Edom, by God (??	7th cent. BC	ND	Tel 'Arad ostracon 21	Aharoni 1981, 21:5
34	[…kur*ú*]-*du-um-mu in-ta-du-ú* / […] x-*ma ummāni*meš *ma-du-tu*/ […*a*]*bul* URU.RUG-*di-ni* / […] x *idūk-šú*	they/he set up camps against [E]dom […]…the large army […the g]ate of Rugdini […] …he killed/ defeated him	553–551 BC	Nabonidus	Nabonidus Chronicle	*ANET* 305; *ABC* 7.1.17–20

Figure 2c. Suggested references to the names Edom and Seir in Late Bronze and Iron Age sources.

Figure 3. Bing Map satellite view of Khirbet en-Nahas, with excavation areas. © 2021 Microsoft.

with Edomites in the northern Negev, rather than in southern Transjordan.

Roughly contemporary, at least with respect to the extant copy, is a reference to Seir in *Papyrus Pushkin* 127 (*Figure 2: no. 14*), a pseudo-historical letter that dates sometime between the late 20th and the 22nd Dynasties (Caminos 1977). At the end of the letter, Wermai, an Egyptian official who was wrongly ousted from his post, mentions an individual (his saviour or his oppressor) whom he wishes to send to Naharain to bring a hidden *tmrgn* (?) with whom he had gone to 'those of Seir'. Kitchen (1992, 27) suggested this event was contemporary to the flight of Hadad the Edomite to Egypt (1 Kings 11:14–22), and thus evidence of sporadic relations between Egypt and Edom. However, as already indicated by Ash (1999, 46–48), the interpretation of the text is riddled with so many uncertainties – including the date of the original text, the historicity of the story, and the correct translation of the phrase *n3y S'r*[3] – that it cannot be used as a source for reconstructing the history of Edom.

To this period belongs the earliest reference to Edom in a Neo-Assyrian inscription. The so-called Calah (Nimrud) Slab records the submission of tribute of several western countries to Adad-nirari III, including Tyre, Sidon, Israel, Philistia and Edom (KUR *ú-du-mu*; *Figure 2: no. 16)*. Adad-nirari III carried out four military campaigns in the west; unfortunately, none of his royal inscriptions are dated, and therefore the chronology of these events has to be reconstructed from the dates of the Eponym Chronicle. From this source, the western campaigns can be reconstructed as follows: to Gūzāna in 808, to north Syria from 805–803, to Lebanon and Arwad in 802, and to Aram-Damascus in 796 BC (Siddall 2013, 63–67; Younger 2016, 524–529). Since the Calah Slab records the surrender of Damascus right after the tribute from the western polities, the submission of Edom should be dated after 796 BC. How much later is not possible to say, but it is clear that Adad-nirari III did not campaign south of Aram-Damascus nor did he exercise any form of direct political control over Cis- and Transjordan. Thus, these submissions of tribute could have occurred as late as 'the end of the 790s' (Siddall 2013, 68–69).

Khirbet en-Nahas

During the Iron Age IIA–B period, there is evidence of the emergence of a highly specialized metallurgical industry in the copper mining region of Faynan in

[3] The phrase *n3y S'r* most likely refers to 'those of Seir', but it could also be translated as 'those *from* Seir' (living in Egypt; Caminos 1977, 68).

Date (century)	*Area A*	*Area M*	*Area F*	*Area S*	*Area T*	*Area R*		*Area W*
Post-abandon.	A1a A1b		F1a F1b	S1a S1b	T1a	R1a R1b ♦♦		W1a
9th	A2a	M1		S1 ♦			C. R3	W1b ♦♦
	A2b	M2a ♦♦	F2a	S2a ♦				W2AI ♦♦ W2AII W2BI W2BII
Late 10th-early 9th	A3a	M2b	F2b	S2b	T1b T2a	R2a		
10th	A3b	M3	F2c		T2b	R2b		
Early 10th	A4a			S3	T3	R3a R3b		W3
Late 12th-11th		M4		S4				
13th-12th		M5a M5b						
Virgin soil	A4b		F3	S5				

Colour key for types of layers: Dark grey = architectural remains, Light grey = industrial activities, White = unoccupied. Finds of Egyptian amulets are marked with the ♦ sign.

Figure 4. Simplified correlation of KEN excavation areas and dates according to the excavators, with finds of Egyptian amulets (adapted from Levy et al. *2014, Table 2.1).*

southern Transjordan. The key site is Khirbet en-Nahas (hereafter KEN), a large 10ha site with over 100 buildings, but more sites dated to this period have been surveyed and a few excavated during the 2000s by the Edom Lowlands Regional Archaeology Project (hereafter ELRAP; Levy, Najjar and Ben-Yosef 2014). However, the interpretation of the finds, particularly the dating of the monumental structures unearthed at KEN, has been contested by several scholars.

Excavations at KEN found the remains of a fortress gatehouse (Area A) and inside building (Area F); several buildings dispersed across the site served as accommodations for the local inhabitants or were used for metalworking activities (Areas S, R, T and W), while a deep slag heap contained the discarded remains of the metallurgical industry (Area M; *Figures 3*, *4*). According to the excavators, the fortress and most buildings were built and occupied during the 10th–9th centuries BC, to be abandoned at the end of the 9th century BC. This dating was based predominantly on a series of 104 radiocarbon dates taken from several archaeological contexts, although the stratigraphic discussion was heavily dependent on Bayesian modelling (Levy *et al.* 2014). This combined data was understood as evidence of the emergence of social complexity in Edom during Iron Age IIA–B, much earlier than the traditionally accepted date – i.e., Iron Age IIC, no earlier than the late 8th century BC. For the ELRAP team, there existed an evident genealogical link between this early polity and the 'classic' Iron IIC Edomite 'kingdom'; therefore, the Faynan polity is called an Edomite 'nomadic kingdom' (Ben-Yosef 2019; 2020) or 'super chiefdom' (Levy and Najjar 2006, 13).

Criticism on ELRAP's interpretation of the KEN finds began as soon as the first preliminary reports were published; they involved many issues, but they focused on four main points:

1) *Use of the radiocarbon dating:* Van der Steen and Bienkowski (2006) criticized the use of Bayesian modelling, which would have pushed the dates artificially earlier than the calibrated radiocarbon dates. They also noted the improper use of the maximum probability range of the Bayesian calibrated results, leading to the rejection of dates that were considered too early or too late (see Levy, Najjar and Higham 2007 for the ELRAP's responses).

2) *Stratigraphic contexts of the radiocarbon samples:* Finkelstein (2005, 2020) and Finkelstein and Piasetzky (2006, 2008) suggested that most of the radiocarbon samples came from industrial wastes and fills below the floors of the fortress, which antedated its construction.

3) *Interpretation and dating of the ceramic assemblage:* According to Finkelstein and Singer-Avitz (2008, 2009), it is the pottery evidence, not the radiocarbon data, which should be used to date the main phase of occupation at KEN. Since they noted that sherds dating to the Iron IIB–C comprise more than half of the local ceramic assemblage, the site should be dated to this period.

4) *Parallels of the KEN fortress and gatehouse*: In a related vein, Finkelstein and Singer-Avitz (2008, 17) were also critical of the excavators' re-dating of the Iron IIB forts and gates excavated at Tell el-Kheleifeh and 'En Hazeva to the Iron IIA period, based on their similarities to the architecture of the KEN gatehouse.

In a recent article (Tebes 2022), the present author made a comprehensive re-analysis of the archaeological evidence of KEN, the first critical analysis using the data provided by the final excavation report published by Levy *et al.* (2014). The first part concentrated on the methodology followed by the ELRAP team, following the earlier criticism reviewed above. The use of Bayesian modelling in slag mounds was found to be methodologically faulty, as the stratigraphic sequences do not necessarily reflect the chronological order of deposition, especially in industrial layers like those found at KEN where the layers of ash and slag were deposited and re-deposited several times. The absence of datable artifacts found in secure contexts makes it difficult to synchronize slag mounds with absolute dates. Furthermore, the use of radiocarbon data is not entirely free of problems, particularly regarding the variations in the atmospheric radiocarbon and the presence of a flat calibration curve around 800–400 BC (Tebes 2022, 115-119).

The second part (Tebes 2022, 119-132) explored the relationship between the stratigraphic find-spots, the radiocarbon samples and the pottery evidence retrieved at KEN, free from the bias of Bayesian modelling, leading to a reconstruction of the Iron Age history of the site different from the one suggested by ELRAP but also from the ones outlined by its critics. The complete 104 radiocarbon dates from KEN were re-calibrated using the most recent IntCal 20 atmospheric data, which led to a lowering of eleven dates and the raising of six. The interpretation of the ELRAP team was most likely correct in dating the earliest permanent architecture to the 10th century BC, but this occupation only involved domestic dwellings such as those excavated at Areas F, M, T and W. The reassessment of the evidence found across the site demonstrated that the construction of monumental and defensive architecture can only be dated to the 9th century BC. Most importantly, the earliest surface structures found at the gatehouse fortress in Area A belong to layer A2b (and not layer A3b as postulated by the excavators), lowering its construction to the 9th century BC. Similarly, the monumental building with perimeter wall excavated at Area R should be dated *after* the layer of heavy industrial activity upon which it is sitting stratigraphically, thus dating again to the 9th century BC (*Figure 5*).

The conclusion is that KEN was probably abandoned, or its population significantly reduced, in the late 9th or early 8th centuries BC, and therefore its relationship with the later Edomite polity based in Busayra has yet to be found. Judging from the Iron IIC pottery found in the site, KEN was probably occupied during the late 8th–7th centuries BC, although the scale of the metallurgical production paled in comparison with the Iron IIA–B industry (Tebes 2022, 133-136).

Very important in this regard is the reference in the Calah Slab to the tribute from Edom to Adad-nirari III, around the end of the 790s BC. Oddly enough, this first, laconic reference to a polity in Edom has not received the attention it deserves; most importantly, it is an inconvenient inscription for scholars defending an Iron IIC date for political complexity in Edom. The reference fits extraordinarily well into our re-dating of the Faynan chiefdom, making Adad-nirari III's Edom a reference to the still-existing Faynan chiefdom in the late 9th–early 8th centuries BC (as suggested already by Tebes 2013, 42). This early Edom reference makes also clear that the name Edom was not exclusively applied to the Iron IIC Busayra polity. Significantly enough, the inscription does not mention Ammon or Moab (Bartlett 1989, 124), and Faynan's tribute may only have involved some sort of recognition of Assyria's new gained role in the Levantine affairs, but little else.

Egyptian finds at Faynan

As will be obvious from this review of the evidence, no local epigraphic find from Faynan can be clearly associated with absolute dates. Potentially significant is the corpus of 17 Egyptian amulets found in the Faynan region (*Figures 6*, *7*), the majority of which belong to the so-called early Iron Age mass-produced seals (EIAMS), said to be produced in the Eastern Delta during the reigns of pharaohs Siamun and Shoshenq I, and arguably found in Iron I–IIA contexts of the southern Levant (Münger 2003, 2005). All but one of the amulets found by ELRAP came from stratified contexts (Münger and Levy 2014, 742–758). Ten were found at KEN, including two from an Area S metallurgical building, two from an Area M structure, two from the Area R monumental building, and four from an Area W building complex. Other three were found in a tomb excavated at the tumuli field at Wadi Fidan 4 (the tombs are presumably Iron Age, although the cemetery dates predominantly to the Early Bronze Age); and two from two tombs in the Iron Age cemetery of Wadi Fidan 40. Unfortunately, the most significant scarab, attributed to the reign of pharaoh Shoshenq I, was found by chance in an

Date (century)	*Area A*		*Area R*		
	Levy et al. 2014	*Tebes 2021a*	*Levy et al. 2014*		*Tebes 2022*
7th-6th		Metallurgical phase			Metallurgical phase
Post-abandon.	A1a A1b		R1 a R1 b		
9th	A2a	A2 (gatehouse)		C. R3	R2 (building)
	A2b				
Late 10th-early 9th	A3a	A3 (footing)	R2a		C.R3 (metallurgical phase)
10th	A3b		R2b		
Early 10th	A4a	A4a	R3a R3b		
Late 12th-11th					
13th-12th					
Virgin soil	A4b				

Colour key for types of layers:
Dark grey = architectural remains,
Light grey = industrial activities,
White = unoccupied

Figure 5. Re-dating of KEN excavation areas A and R according to Tebes 2022.

Figure 6. Bing Map satellite view of the distribution of the 17 Egyptian amulets found in the Faynan region. © 2021 Microsoft.

Amulet #	*Site name*	*Context*	*Suggested dates*
1	Wadi Fidan 4	Tomb 1, l. 758	960-900/880 BC
2	Wadi Fidan 4	Tomb 1, l. 701	Iron IB/Early Iron IIA
3	Wadi Fidan 4	Tomb 1, l. 720	Late Bronze IIB/Early Iron I
4	Wadi Fidan 40	Grave 92, l. 531	Middle Bronze IIB 14C: 1015-845 BC (1 sigma)
5	Wadi Fidan 40	Grave 91, l. 3050	21st Dynasty
6	Khirbet Hamra Ifdan	surface	"Shoshenq I" scarab
7	KEN	Area S2a, l. 330	18th-19th Dynasties
8	KEN	Area S1, l. 316	960-880 BC
9	KEN	Area M2a, l. 635	Iron Ib/IIA
10	KEN	Area M2a, l. 707	960-900/880 BC
11	KEN	Area R1b, l. R09L053	19th-22nd Dynasties
12	KEN	Area R1b, l. R09L101	22nd Dynasty
13	KEN	Area W1B, l. W09L098	19th-20th Dynasties
14	KEN	Area W1B, l. W09L124	Iron IIB
15	KEN	Area W2AI, l. W09F178	Late Bronze Age
16	KEN	Area W2AI, l. W09L187	19th-20th Dynasties
17	Barqa House 4	l. 478	"Menkheperre" (Thutmose III?)

Figure 7. Egyptian amulets found in Faynan (#1–16: Based on Münger and Levy 2014; #17: Adams et al. *2010, 10–11).*

unstratified context at Khirbet Hamra Ifdan. Another scarab was found by the Barqa Landscape Project in a Late Bronze or Iron Age Egyptian burial in House 4 at Barqa (Adams *et al*. 2010, 10–11).

The ELRAP team associated these finds with Shoshenq I's military campaign in the Negev and Arabah, with KEN as a possible target of the Egyptian campaign (Levy *et al*. 2008, 15465).[4] This interpretation, however, presents several problems:

1) The nature and find-spots of these amulets do not differ much from similar finds of Egyptian objects in the Levant, finding their way through more traditional channels of trade and gift-giving. Amulets were normally passed as heirlooms through several generations (Keel 1995, 247; Ash 1999, 76–77; Tebes 2004, 94); one of the amulets found at Wadi Fidan 40 is, in fact, dated to Middle Bronze IIB (*Figure 7: no. 4*). Moreover, some scarabs are notoriously difficult to date. Particularly troubling is the interpretation of the scarab found in Barqa House 4 bearing the name *mn-ḫpr-r'* (Menkheperre; *Figure 7: no. 17*), a name that was used by New Kingdom pharaoh Thutmose III that continued to be popular during the first millennium BC. It was also used by at least three individuals: Menkheperre, High Priest of Amun of the 21st Dynasty; 25th Dynasty king Piye;[5] and 26th Dynasty pharaoh Necho I (Keel 1995, 251–254).

2) Five of these amulets were found in tombs. Tomb finds are notoriously problematic for dating, due to the re-use of older graves, a phenomenon well-known in the desert mortuary traditions of the Negev and Edom (Tebes 2020a). In fact, Münger and Levy (2014, 747) note the reuse of the amulet of one of the tombs of Wadi Fidan 40 (*Figure 7: no. 5*). If we add that amulets were traditionally deposited as gifts to the dead, then these items cannot be used for absolute dating.

3) The amulet finds at KEN do not support the chronological conclusions of the excavators. A detailed analysis of their original stratigraphic contexts demonstrates that all of them come from layers radiocarbon dated to the 9th century BC or from post-abandonment activities: S2a, S1, M2a (two finds), R1b

[4] Even Finkelstein, an otherwise strong critic of ELRAP, takes their conclusions at face value and concludes that 'it seems logical to propose that the target of the Sheshonq I campaign in the south was the copper production center of Wadi Faynan in the Arabah' (see Finkelstein forthcoming).

[5] Although Yoyotte (1989) suggested that this name does not correspond to Piye but to Theban king Iny Siese Meryamun.

(two finds), W1B (two finds) and W2AI (two finds; see *Figure 4*). None of them comes from 10th century BC levels, but they were found in contexts at least one century later than the time of the Egyptian involvement at KEN postulated by the excavators. As Münger (2005, 400) notes for the repertoire of Egyptian scarabs at other sites in the Iron Age Negev (Tel ʻArad XII, Tel Beersheba VII, Tel Masos II), these finds only provide a *terminus post quem* of occupation at these sites.

4) Despite the hype, the so-called Shoshenq I scarab, described as 'the most significant Iron Age Egyptian artifact found to date in southern Jordan' (Münger and Levy 2014, 748), is not without problems. The first, of course, is that it was found out of context, and so it is devoid of chronological value. Further, its attribution to Shoshenq I is not as secure as claimed. The scarab has the throne name *ḥḏ-ḫpr-Rʿ stp.n-(J)mn-Rʿ* (Hedjkheperre Setepenamunre); as Münger and Levy recognize in an endnote, this prenomen 'is currently not attested in the available epigraphic corpus' (Münger and Levy 2014, 760 n. 15). The most common prenomen attested for Shoshenq I is Hedjkheperre Setepenre; however, as they also note, this prenomen was also used by the 21st Dynasty pharaoh Smendes and by the 22nd Dynasty pharaohs Takelot I, II and Shoshenq IIIa (also known as Shoshenq IV). To Münger and Levy's list, we have to add king Shoshenq VIa, who also bore the same title (Aston 2020, 695, table 32.1), leaving a total of six pharaohs with the same prenomen (but, again, not one exactly the same as the one found on the Khirbet Hamra Ifdan scarab). Only Shoshenq I and IIIa (IV) have both Hedjkheperre Setepenre and Hedjkheperre Setepenamun (Dodson 1993). Therefore, one cannot rule out the possibility that this scarab belongs to another pharaoh, most likely Shoshenq IIIa (IV).

In sum, the corpus of Egyptian amulets does not provide absolute dates for the Iron Age occupation at KEN and beyond, let alone evidence of Shoshenq I's military campaign in Faynan. In fact, the archaeological context where the Egyptian amulets were found at KEN confirms a 9th century BC date for the monumental architecture. The 9th century BC certainly marked a period of increasing Egyptian influence at Faynan; however, the nature and find context of the Egyptian items point more to strong trade links with Egypt or local elite emulation of the Egyptian culture rather than direct domination. There is not the slightest archaeological evidence that the army of Shoshenq I visited Faynan, confirming the absence of southern Transjordanian places in the Karnak list; most importantly, the corpus of Egyptian amulets found at Faynan cannot be used for dating the reign of Shoshenq I. Most of the data seems to point to a peaceful abandonment of the buildings at KEN during the late 9th or early 8th centuries BC, probably because of internal reasons (Bienkowski 2022).

Excursus: The chronology of Edom in the Bible

The analysis of the chronology of Edom as reflected in the biblical text, a secondary source in itself with several problems involved in its dating, has to be dealt with separately. The Deuteronomistic History recounts a long, conflicted history between Israel/Judah and the Edomites throughout the 10th and 8th centuries BC (David: 2 Sam. 8:13–14; Jehoshaphat: 1 Kings 22:47; Israelite Jehoram: 2 Kings 3:9; Judaean Jehoram: 2 Kings 8:20–22; Amaziah: 2 Kings 14:7; Uzziah: 2 Kings 14:22; Ahaz: 2 Kings 16:6. Bartlett 1989, 103–128; Lemaire 2010, 227–233; Na'aman 2015a). These annalistic references to Edom are troubling, as that they allude to events occurring in the 10th–8th centuries BC, when archaeological evidence of settlement in the Edomite highlands is scarce and at best disputed. Except for the reference to Hadad, the Edomite of 'kingly stock' who was exiled in Egypt during David's time and came back during Solomon's (1 Kings 11:14–22), no Edomite king is mentioned by name in Samuel–Kings. The Hadad story is suspect, as there could be confusion between the names Edom and Aram; if the latter, Hadad could have been an Aramaean king (Lemaire 2010, 228–229). It could be adduced that these events correspond to the Edomite kings listed in Gen. 36, but despite the confidence of some scholars (Lipiński 2006, 388–389) this list can hardly be used to reconstruct the history of Iron Age Edom.[6] It would be tempting to associate the Deuteronomistic references (and Gen. 36) to the chiefdom of Faynan; interestingly, Bozrah (modern Busayra in the southern Transjordanian highlands and arguably the Edomite 'capital') is never mentioned by name in them.

Edom in the Iron IIC

External written sources

Following Adad-nirari III's reference (*c.* 790 BC), and after a hiatus of some 60 years, we have the second reference to Edom in the Neo-Assyrian sources, dated to the reign of Tiglath-pileser III (*c.* 730 BC). Between *c.* 730–650 BC, about a dozen references to Edom appear in Neo-Assyrian records, most of them within royal inscriptions mentioning the submission of tribute of Levantine polities, in some cases indicating the names of some Edomite kings. A short allusion in a Neo-Babylonian inscription dated to Nabonidus (*c.* 550 BC) will extend our knowledge on Edom about 100 years more.

The earliest reference is a note about the submission of tribute from Qausmalak of Edom to Tiglath-pileser III, along with the kings Sanipu of Ammon, Salamanu

[6] The Edomite king list in Gen. 36 has been dated to the Persian period (Knauf 1985), and again, related to Aramean kings, rather than to Edom (Lemaire 2010, 228).

of Moab, Mitinti of Ashkelon, Jehoahaz of Judah and Hanno of Gaza (*Figure 2: no. 17*). Qausmalak is the first extra-biblical Edomite 'king' known by name, and the first presenting the theophoric element Qos. This event probably occurred after Tiglath-pileser III suppressed the revolt of Ashkelon and Gaza in 734 BC or after the taking of Damascus in 732 BC (Bartlett 1989, 128).

From the reign of Sargon II, we have a record of tribute from 'Ed[om]', alongside Philistia, Judah and Moab (*Figure 2: no. 18*). This submission should be dated after the rebellion of Ashdod in 713 BC (Bartlett 1989, 130, dates the campaign to 712 BC). Another source from Sargon II's reign is a Nimrud letter reporting the arrival of envoys from several countries, including Gaza, Ashdod and Edom (*Figure 2: no. 19*). The date of this event is not clear; Bartlett (1989, 130) relates this tribute to Sargon II's Ashdod campaign in 712 BC, while Lipiński (2006, 390) dates it to the earlier part of his reign (he started reigning in 721 BC). The name Edom also appears in the so-called *Sargon Geography*, a geographic treatise dating to Sargon II or Sennacherib that describes the borders of the kingdom of Sargon of Akkad 'updating' it with the borders of the Neo-Assyrian empire (Horowitz 1998, 67–95). The name E[do]m can be reconstructed alongside the names of [Mo]ab, Tema (Tayma; a parallel source lists it as Til Temania) and Ginnirtum (*Figure 2: no. 20*; Horowitz 1993; 1998, 74 n. 6).

During his third campaign in the west (701 BC), Sennacherib is said to have received tribute from 'all the kings of the Amurru' (i.e., the Levant), including Menahem of Samsimuruna, Tuba'lu from Sidon, Abili'ti from Arvad, Urumilki from Byblos, Mitinti from Ashdod, Buduili from Beth-Ammon, Kammusunabdi from Moab and Ayarammu from Edom (*Figure 2: no. 21*).

The name of another king of Edom, Qausgabri, is mentioned in a list of 12 tributary 'kings of the country Hatti and on the other side of the river (Euphrates)' subjugated by Esarhaddon (673 BC; *Figure 2: no. 22*). The list also includes Ba'lu of Tyre, Manasseh of Judah, Musuri of Moab, Sil-Bel of Gaza, Metinti of Ashkelon, Ikausu of Ekron, Milkiashapa of Byblos, Matanba'al of Arvad, Abiba'al of Samsimuruna, Puduil of Beth-Ammon and Ahimilki of Ashdod. The same Edomite royal name is mentioned in an undated list of 22 kings 'from the seashore, the islands and the mainland' who sent tribute to Assurbanipal (*Figure 2: no. 23*). These kings are probably the same '22 kings from the seashore, the islands and the mainland' who are mentioned in the Rassam Cylinder as delivering tribute to Assurbanipal during his first campaign against Egypt (667 BC; Bartlett 1989, 138). The former list also includes Ba'al of Tyre, Manasseh of Judah, Musuri of Moab, Sil-Bel of Gaza, Mitinti of Ashkelon, Ikausu of Ekron, Milkiashapa of Byblos, Iakinlu of Arvad, Abiba'al of Samsimuruna, Amminadbi of Beth-Ammon, Ahumilki of Ashdod, Ekishtura of Edi'li, Pilagura of Pitrusi, Kisu of Silua, Ituandar of Pappa, Erisu of Sillu, Damasu of Kuri, Admesu of Tamesu, Damusu of Qarti-hadasti, Unasagusu of Lidir and Pususu of Nure.

Although tributary to Assyria, Edom was probably never occupied by the Assyrian armies. The only time the Assyrians seem to have set foot in Edom is during Assurbanipal's war against the Arabs, when his armies are said to have reached as far south as Edom (*c.* 650 BC). The chronology of Assurbanipal's wars is difficult to reconstruct, but it is probable that he engaged in two main wars against Yawtha', chief of the Qedarites, the first one before 652 BC and the second one during 651–648 BC. The reference to Edom appears in the Rassam Cylinder, one of the latest sources that were produced referring to these events, thus probably conflating different events into one outline (Weippert 1973–1974; Eph'al 1982, 147–169; Gerardi 1992; Retsö 2003, 166–167; Tebes 2017, 71–73). According to this inscription, in his ninth campaign Assurbanipal marched against Yawtha', moving 'in the *girâ* of Az/ṣarilu, Ḫiratâqaz/ṣaya in Edom, in the mountain pass of Iabrud, in Bît Ammani, in the district of Ḫaurîna, in Moab, in Seir, in Ḫargê, in the district of Zoba' (*Figure 2: no. 26*). This extremely important inscription contains the only reference to two placenames in Edom and the only reference to Seir (*Sa'arri*) in a Neo-Assyrian source. The two toponyms have been interpreted as Aramaic or North Arabian composite names – *Az/ṣarilu*, 'enclosure/sheepfold of (the) god' and *Ḫiratâqaz/ṣaya*, 'cultivated land of Qos' – probably referring to Edomite sacred places visited or garrisoned by Assyrian armies (Tebes 2016; 2017, 71–81). Their location is unknown, while it is clear that the toponyms are not presented in proper geographical order, evident from the position of the references to Transjordanian Ammon (*Bīt Ammani*) and Moab.[7]

Reference should be made to three Neo-Assyrian inscriptions mentioning Edom, including a wine list, a receipt of tribute and a lexical list (*Figure 2: nos. 27–29*). Although undated and for the most part fragmentary, these lists attest the role of Edom as a tributary polity of the Assyrian empire (see Bartlett 1989, 132; Lipiński 2006, 390 for suggested dates).

The last evidence we have for Edom in the Iron Age is the so-called *Nabonidus Chronicle*, a Neo-Babylonian inscription recording the campaign of king Nabonidus in his third year to Transjordan and northern Arabia (553 BC). The text mentions a campaign against [*u*] *dumu* (*Figure 2: no. 34*), most likely a reference to the conquest of Edom by Nabonidus in preparation for

[7] Iabrud is probably a reference to Yābrud, in Syria; Ḫaurîna is traditionally associated with the province of Hauran or with Ḥawārīn in Syria; Ḫargê has been identified as ^{uru}*Ar-gi-te* in Syria; Zoba is traditionally identified as the Aramean kingdom of Zoba (Weippert 1973–1974, 61–62; Eph'al 1982, 149 n. 514; Lipiński 2001, 319–345; Parpola and Porter 2001, Map 8:B2, B3, C2; Tebes 2017, 73 n. 17; Rohmer 2020, 428–430).

his campaign in northern Arabia (Crowell 2007, 78). The two following lines, which are badly preserved, mention a 'large army' and later the 'g]ate of RUG*dini*', read as Rugdini or Šinṭīni, a probable reference to Rekem, the Semitic name for Petra (Lipiński 2006, 420), or as-Sila (Sela), northwest of Busayra (Da Riva 2020, 192). A badly preserved relief of Nabonidus discovered at as-Sila is probably related to this event; an extant text likely reads 'I am Nabonidus, king of Babylon (...)' and is possibly dated to his fifth year (Da Riva 2020, 190; *RINBE* 2, 55). If the reading is confirmed, it implies Nabonidus' campaign in Edom took at least two years, between his third and fifth regnal years (553–551 BC; Da Riva 2020, 190).

Local archaeological and epigraphic evidence

Despite their relatively large number, it is very difficult to associate the references to Edom in the Neo-Assyrian and Neo-Babylonian sources to the local archaeological and epigraphic evidence. Owing to the lack of well-dated epigraphic sources and the absence of archaeological sites with deep stratigraphies, the chronology of the Iron Age 'kingdom' of Edom is difficult to establish.

The only local epigraphic evidence we have of an Edomite 'king' is that of Qausgabri, who as we have seen was mentioned in sources from the reigns of Esarhaddon and Assurbanipal dating to 673 and 667 BC respectively. A clay impression found in an unstratified context at Umm el-Biyara, in the Petra mountains, can be restored as 'belonging to Qos-Ga[br], king of Edom', thus presenting the only clear chronological synchronism with the Neo-Assyrian sources (*Figure 2: no. 24*). A scaraboid found in Babylon ('belonging to Qos-Gabr, [king of Edo]m(?)') probably refers to the same king (*Figure 2: no. 25*). Based on the Qos-Gabr impression, Bennett, the excavator of the Edomite sites of Umm al-Biyara, Tawilan and Busayra during the early 1960s–early 1980s, concluded that the Edomite settlement encompassed mainly the Iron Age IIC (7th–early 6th centuries BC). Bienkowski, who published Bennett's final archaeological reports decades later, extended the range to include the whole of the Persian period (Bennett and Bienkowski 1995; Bienkowski 2002; 2011). The main problem with the Qos-Ga[br] impression is that, as it was found in an unstratified context, it cannot be used to determine the period of occupation of Umm el-Biyara – even if the Iron Age settlement is considered to have consisted of a one-period occupation, less so of the whole Iron Age settlement in southern Transjordan.

The few radiocarbon dates taken from sites in the Edomite highlands confirm a general Iron Age dating, but do not allow a more precise chronology. Recent soundings at Khirbet Al-Malayqtha, Khirbet Al-Kur and Tawilan produced five radiocarbon dates from seeds, with dates falling in the 8th–6th centuries BC. The only exception was the date from Tawilan, which extended into the 9th century BC (Smith, Najjar and Levy 2014, Table 3.2). More recent radiocarbon datings have been obtained from lime-based mortars from hydraulic structures of the archaeological site of as-Sila (Da Riva *et al.* 2021). The 12 radiocarbon datings indicate three main chronological horizons: mid–late 2nd millennium BC, 1st millennium BC, and mid-1st–early 2nd millennia AD. Unfortunately, their interpretation poses several problems, particularly in relation to the process of mortar formation. A cautious interpretation of the data would suggest a date of the construction of at least part of the hydraulic system at as-Sila in the Iron Age II (Tebes 2020b).

'Edomite' pottery and destruction levels in the Negev

A potentially useful chronological peg is provided by the presence of 'Edomite' pottery in Iron Age sites of the Negev, considered reliable given its association with strata of destruction arguably dated by Neo-Assyrian, Neo-Babylonian, and (less so) Egyptian military campaigns (*Figure 8*). Unfortunately, neither the nature nor the dating of these destructions is free of difficulties.

In the Negev, 'Edomite' pottery, also known as 'Busayra painted ware' or 'Southern Transjordan Negev Pottery' (STNP), is traditionally dated between the late 8th and early 6th centuries BC. The strong Assyrian influence in the morphology of some of these vessels has led some to coin the term 'Edomite-Assyrian' pottery (Singer-Avitz 2014, 267). The bulk of it is considered to be 'sandwiched' between two wide waves of destruction: Sennacherib's third campaign against the Levant and Judah in 701 BC, and Nebuchadrezzar II's second campaign against Judah in 587/86 BC (Tebes 2011, 66–70). Its earliest attestation in the Negev was discovered in Strata III–II at Tel Beersheba – an 'Edomite' globular bowl with stylistic Assyrian influences: since the pottery of Stratum II resembles the ceramic assemblage of Lachish Level III (the key layer of Iron Age Judah considered by most scholars to be destroyed in 701 BC), the 'Edomite' pottery, by necessity, should date before the end of the 8th century BC (Singer-Avitz 2004).

Destruction strata attributed to Sennacherib's campaign include Tel 'Arad VIII (Aharoni 1981, 149; Herzog 2002, 98), Tel Beersheba II (Herzog 2016, 1475) and Tel 'Aroer III (Thareani 2011, 306). For Tel 'Ira, the excavators noted that Stratum VII ended in destruction at the end of the 8th/first half of the 7th centuries BC, but they did not associate this with any historical event (Beit-Arieh 1999, 176). Similarly, for Tel Malhata the excavators only stated that the 8th-century BC fortification system (Stratum IV) was destroyed, but they did not attempt to establish a precise date for the transition to the following 7th-century BC layer (Stratum III; Beit-Arieh and Freud 2015, 741–742).

Date (century)	*H. Qitmit*	*T. 'Aroer*	*'A. el-Qudeirat*	*T. 'Ira*	*T. 'Arad*	*T.Beersheba*	*H. Uza*	*T. Masos*	*T. Malhata*	*E. Hazeva*	*H. Radum*	*H. Tov*
	Beit-Arieh 1995	Thareani 2011	Cohen & Bernick-Greenberg 2007	Beit-Arieh 1999	Herzog 2002	Herzog & Singer-Avitz 2016	Beit-Arieh 2007	Fritz & Kempinski 1983	Beit-Arieh & Freud 2015	Cohen & Yisrael 1995	Beith-Arieh 2007	Itkin 2020
8th BC		IV III	III	VII	X-IX VIII	II			IV	V		
7th BC		II	II			I		Area G	III	IV		fort
	shrine			VI	VII		fortress				fort	
6th BC					VI							

Figure 8. Stratigraphy of the Iron Age Negev sites with traditional destruction dates.

The year 701 BC has become a cornerstone for the archaeology of Iron Age Negev. Whole strata have been re-dated according to the relationship with the pottery found in the '701 BC' destruction levels. Thus, based on the similarities between the pottery of Tel 'Ira VII and that found at Tel Beersheba II, Singer-Avitz (2004, 85–86) raised the end of the former to the late 8th century BC. In a reassessment of the 'Edomite' pottery in the Negev, she summarized the most commonly held opinion (Singer-Avitz 2014, 269):

> The sites in Cisjordan in which Edomite pottery has been observed in eighth-century strata are as follows: Tel Beersheba Strata III and II, Arad Strata X to VIII, Tel 'Ira Stratum VII, Tel Malhata Stratum IV, 'Aroer Strata IV and III and Kadesh Barnea Substrata 3a and 3b.

The prosperous post-'701 BC' settlements are now regarded as part of the new economic opportunities raised during king Manasseh's reign, based on the so-called *Pax Assyriaca* (Thareani-Sussely 2007). Given this state of affairs, attention has been focused on Assyrian-influenced material culture, particularly some specific types of 'Edomite' carinated and globular bowls with morphological features that can be traced back to fine Assyrian pottery that was popular in the Assyrian heartland during the heydays of its power. As we have seen, the earliest attestation of this pottery appears in Tel Beersheba III–II, whose destruction is traditionally dated to 701 BC (Singer-Avitz 2004). This would imply an unlikely, rapid process of assimilation of the local population, or at least the elite, to the culture of the conqueror power. This seeming contradiction has led some to dissociate these fine 'Edomite' vessels with their alleged Assyrian prototypes. Thus, for Na'aman and Thareani-Sussely (2006), the style of those vessels originated in Transjordan, rather than in Assyria. The Assyrian inspiration of these vessels has been defended by Singer-Avitz (2007), who suggested that the local inhabitants became 'familiarized' with the foreign styles through the trade contacts with the north.

But what is the actual inscriptional evidence for the destruction of Iron Age Negev sites by the Assyrians? A close analysis of the historical sources shows that evidence that the Assyrian armies reached the Negev region is meagre or non-existent, making any links with local destruction strata hypothetical at best. For the campaign of Sennacherib in 701 BC, the key event for dating a whole series of archaeological sites in the Negev to the late 8th century BC, scholars resort to three textual sources: inscriptions of Sennacherib himself (RINAP 3/2, 14–15), the biblical account (2 Kings 18:13–37) and Tel 'Arad ostracon 40 (Aharoni 1981, 70–74). The Assyrian and biblical sources only refer to movements of troops in the core of Judah and the Shephelah, while there is no reference to the northern Negev (Knauf 2002, 187–190; Bagg 2011, 244–252; see Bagg in this volume, pp. 311–321; van der Veen 2020, 100–102 and van der Veen in this volume, pp. 291–310). An alleged allusion to a 'king of Judah' in ostracon 40 from Tel 'Arad was interpreted within the context of Hezekiah's dealing with the threat of Edom during Sennacherib's campaign (Aharoni 1981, 74, 149). However, as we will see in the next part, neither this reading nor the ostracon's dating to the late 8th century BC are secure.

Other Assyrian kings, including Tiglath-pileser III, Sargon II and Esarhaddon, are known to have campaigned in the arid southern parts of the Levant, but the toponyms that their inscriptions mention are restricted to the coastal area between Gaza and the Nahal Musur (Na'aman 1979). There was no need for the Assyrian kings to penetrate in the deep desert, as the control of the inland regions was secured through agreements with local Arab 'chiefs', such as Siruatti the Me'unite, Idibi'lu (with Tiglath-pileser III) and the city of Laban's sheikh (with Sargon II). These sheikhs were given the task to control the traffic of people and goods travelling through the area (Tebes 2013, 48).

Some scholars have noted the possibility that the Iron Age strata of the Negev allegedly destroyed by Sennacherib in 701 BC were in fact destroyed at other times. Na'aman (1986, 13–15) proposed that Tel Beersheba II was destroyed before 701 BC, raising the possibility that it was caused by Sargon II or the Edomites. Early on, Kenyon (1976) had dated the end of Lachish III to the Babylonian campaign in 598 BC, a dating that is nowadays considered too low for most Levantine archaeologists. She also suggested that Tel Beersheba II has to date later than the Sennacherib campaign but earlier than 598 BC, proposing a mid-7th-century BC date for its destruction. Kenyon's dating has been lately defended by Lipiński, lowering the '701 BC' layers to the 7th century BC, including Tel 'Arad VIII to 598/97 BC and Tel Beersheba II to *c.* 600 BC (Lipiński 2006, 398, 411–412, 415–416). Knauf (2002) suggested a date in the early–mid 7th century BC for the destruction of Tel Beersheba II, pointing to a possible raid by Arab tribes. Most recently, van der Veen (2020, 100–102) has raised the possibility that the destruction levels found in the Negev sites commonly attributed to Sennacherib's 701 BC campaign should be lowered to the mid-7th century BC – that is, they post-date Lachish III. This would not only explain the occurrence of Assyrianizing 'Edomite' pottery during the zenith of Assyrian intervention in the southern Levant, but also this pottery would be chronologically contemporary with the core of the Edomite settlement in the southern Transjordanian highlands, as dated by the Umm el-Biyara impression (van der Veen 2020, 106–112).

The destruction of the Negev sites during the 7th century BC can also be attributed to local agents, which could have also acted as proxies of external powers. Knauf (2002, 191–192) suggested that the destruction

of Tel Beersheba II could have been accomplished by an Arab sheikh, Asuhili, during Esarhaddon's campaign against Egypt in 679 BC. Similarly, the turbulent years of Assurbanipal's Arabian wars (*c.* 650 BC) could have caused havoc among the Negev tribal groups. As we have seen, Assurbanipal's records mention that the Assyrian troops reached as far south as Edom and Seir (*Figure 2: no. 26*); if Seir is here taken literally as the Negev, there is the possibility that the Negev was also affected by these events. Internal events might as well have caused the destruction of local sites. Thareani (2014, 200) suggested that the society of the northern Negev imploded from internal tensions, causing the destruction of many of the local cities. Even if Thareani is referring to the end of the Iron Age sites of the Negev, she raises the important point that the 'Edom' mentioned by biblical and local epigraphic sources may represent a local semi-nomadic entity rather than an external power.

The end of the Iron Age Negev sites and the Tel 'Arad ostraca

If the use of the Neo-Assyrian campaigns for purposes of chronology is problematic, the dating of the end of the Iron Age strata of the Negev sites presents similar challenges. The last Iron Age layers in the Negev are reasonably well dated between the late 7th and the early 6th centuries BC, although there is no consensus as to when and who caused their end. Their destruction has been attributed to three main 'suspects':

1) *Egyptian pharaoh Necho II (609 BC)*. According to some scholars, the intervention of the 26th-Dynasty pharaoh Necho II in the Levant on behalf of the Assyrians would have caused the destruction of the Negev fortresses. This view is mostly based on the finds of texts in Egyptian hieratic script at Tel 'Arad VII (Aharoni 1981, 149) and 'Ain el-Qudeirat II (Lemaire and Vernus 1983; Lipiński 2006, 381).

There are several problems with this view. To begin with, the biblical account has Necho II's intervention occurring in the north, including the killing of Judaean king Josiah at Megiddo (2 Kings 23:29), and not in the Negev. Aharoni (1981, 149) somewhat circumvented this problem by arguing that the Egyptians ordered the destruction of the Negev fortresses after Necho II placed a new king, Jehoiakim, in Josiah's place (2 Kings 23:34). However, it would have been unreasonable for the Egyptians to destroy the defences of their newly acquired ally, just when they would have to contend with the invading Babylonian armies. A related problem is that hieratic writing was also known to the Judaean scribes (Calabro 2012) and they could easily have produced the texts found at Tel 'Arad and 'Ain el-Qudeirat. These texts document the economic activities going on in the Judaean fortresses in the Negev, and none makes explicit allusion to any Egyptian presence in the sites.

2) *Babylonian king Nebuchadrezzar II (590s–580s BC).* A Babylonian take-over of the Negev is the preferred explanation for the end of the late Iron Age sites of the Negev. Some scholars prefer a first wave of destruction around 598/97 BC, during Nebuchadrezzar II's first campaign against Jehoiachin (2 Kings 24:10–17; Lemaire 2010, 236–237) or 596/95 BC, following Zedekiah's forced travel to Babylon in his fourth year (Jer. 51:59; Aharoni 1981, 150). The oracle of Jeremiah, noting that 'the cities of the Negeb are shut up' (Jer. 13:19), is usually mentioned in the context of Jehoiachin's exile. Archaeologically, these events would be represented by the presence of two strata at Tel 'Arad that were apparently destroyed closely in time, Stratum VI for Aharoni (1981, 150) and VII for Herzog *et al.* (1984, 26).

A (second?) wave of destruction apparently followed Nebuchadrezzar II's campaign against Zedekiah in 587/86 BC (2 Kings 25:1–21), including the end of Tel 'Arad VII (Lipiński 2006, 398), Tel 'Aroer II (Thareani 2011, 307); 'En Hazeva (Cohen and Yisrael 1995, 224), Tel Malhata III (Beit-Arieh, Freud and Tal 2015, 741), 'Ain el-Qudeirat II (Cohen and Bernick-Greenberg 2007, 17) and Tel Masos Area G (Zimhoni 1983, 130).

A possible (third?) destruction has been dated to 582/81 BC, following the third deportation during Nebuchadrezzar II's reign (Jer. 52:30), including the destruction of Stratum VI at Tel 'Arad (Lipiński 2006, 398).

3) *Edomites (590s–580s BC)*. Other scholars have focused attention on the role of Edom in the demise of the Iron Age Negev sites. Some point to an Edomite interference following king Jehoiakim's rebellion against Nebuchadrezzar II, in 600/599 BC. The key text is 2 Kings 24:2, where the 'bands of Aram' that Yahweh sent against Judah are understood as a reference to Edom (emending *dalet* for *resh*; Lemaire 2010, 236–237). This upheaval would have caused the destruction of Tel 'Arad VI (Herzog *et al.* 1984, 29; Lemaire 2010, 237), Horvat 'Uza IV (Beit-Arieh and Cresson 1991, 134), or Tel Masos Area G (Zimhoni 1983, 130). However, as already noted by Bartlett (1989, 148–149), the 'Edom invasion' model would stand alone were it not for the references to Edom found in the Hebrew ostraca discovered at the fort of Tel 'Arad.

Four ostraca from Tel 'Arad mention the name Edom (*Figure 2: nos. 30–33*), although only two of them (*nos. 40* and *24*) are detailed enough to provide historical conclusions, having been used to date the destructions of the Iron Age Negev sites. Aharoni, the excavator of the site, originally dated the two ostraca to two different strata that were apparently separated for about a century.

The first ostracon (*no. 40*) consists of a letter sent by Gemaryahu and Nehemyahu, two commanders of a fortified post, to Malkiyahu, their chief commander at

'Arad. Although some of the text is missing, there are two allusions to Edom: 'And behold you knew [about the letters from] Edom (that) I gave to [my] lord [before sun]set (....) The King of Judah should know [that w]e cannot send the [...and th]is is the evil that Edo[m has done]' (*Figure 2: no. 30*).

This inscription was found in one central room (l. 429) that Aharoni originally associated with Stratum VII, but that he later re-assigned to Stratum VIII, dated to the end of the 8th century BC (Aharoni 1970, 29; 1981, 74 n.1). He further read *yd'.mlk.yhwd* (l. 13) as *yd' mlk yhwd*[*h*], 'the king of Judah should know', identified by him as Hezekiah, here struggling with the growing strength of an Edom taking its chances during the times of Sennacherib's campaign (Aharoni 1981, 74, 149). It is hard not to conclude that Aharoni's re-dating of ostracon 40 to the Stratum VIII was heavily influenced by his identification of Hezekiah in the inscription. An odd fact was that the name Malkiyahu also appears in ostracon 24, an inscription dated by him to the 7th century BC, but Aharoni considered this just a 'coincidence', because 'many names reappear in different strata' (1981, 74 n. 10). More recently, Lemaire remarked the palaeographic differences between both inscriptions (he pointed to the diagnostic letter *yod*), which would suggest they do not belong to the same period (Lemaire 2010, 236; followed by Richelle 2017, 18). Herzog, in his re-evaluation of 'Arad's stratigraphy, still attributed the context where ostracon 40 was found (l. 429) to Stratum VIII (Herzog 2002, 14).

The second ostracon (no. 24) was found in an unstratified context on the western slope of the tell, although it was regarded as epigraphically identical to the Eliashib archive – dated to Stratum VI, the last Iron Age layer (Aharoni 1970, 16–18). The ostracon is inscribed on both sides. In the obverse side, probably a draft of a response to the letter written on the reverse side, recent multispectral imaging discovered the phrase *lspr hmlk* ('To the letter/scribe of the king', l. 2; Faigenbaum-Golovin *et al.* 2020, 62). The reverse side contains a letter with orders to Eliashib, chief commander at 'Arad, of dispatching reinforcements led by Malkiyahu from 'Arad to Elisha, commandant at Ramat-Negeb (most likely modern Horvat 'Uza). The key text describes the situation as urgent:

> And the word of the king (*dbr hmlk*) is incumbent upon you for your very life! Behold, I have sent to warn you today: [Get] the men to Elisha': Lest Edom should come there. (*Figure 2: no. 31*)

Aharoni saw the last years of Iron Age 'Arad as a succession of two closely related phases, Strata VII and VI – so close in time that Eliashib, the Stratum VII fortress commander according to an ostracon and three seals, was still in charge during the last days of Stratum VI, the last Iron Age fortress. Therefore, he attributed the end of Stratum VII to the campaign of pharaoh Necho II in *c.* 609 BC. For Stratum VI, he interpreted ostracon 24 as evidence of an Edomite take-over of the Negev sites in the early 6th century BC, coinciding with the collapse of the Judaean administration in the wake of Nebuchadrezzar II's campaigns in the southern Levant. He actually dated 'Arad's fall in 596/95 BC, following ostracon 20 that refers to the 'third year', presumably Zedekiah's third year of reign (Aharoni 1981, 149–150). Lemaire prefers to date the destruction of Tel 'Arad VI a little bit earlier, around 598/97 BC, coinciding with the attacks of the 'bands of Edom' against Jehoiakim (2 Kings 24:2, emended). He dated the Stratum VI ostraca according to the 'tenth month' that is recorded in most of them, which he understands as the month of Tebet 597 (Lemaire 2010, 237).

The 'Edomite threat' model became common opinion for some scholars (Dobbs-Allsopp, Seow and Whitaker 2005, 47–53, 69–74; Lipiński 2006, 405, 417; Lemaire 2010, 236–241; Na'aman 2011, 89; Richelle 2017). Others however see these inscriptions as too ambiguous to allow for significant conclusions regarding Edom's role in the fall of Judah (Bartlett 1989, 149; Finkelstein 1995, 142; Lipschits 2005, 143–144; Guillaume 2013; Thareani 2014). In Lipschits' reassessment of the Babylonian destruction levels in Judah, he concluded that it is not possible to determine the exact chronology of the destruction of the Iron Age Negev sites, and it is more likely that a gradual collapse of the Judaean defensive system occurred. The Babylonians seem unlikely candidates for these destructions, as they did not have 'any reason to expend effort to conquer border forts in remote districts' (Lipschits 2005, 229–230). Most recently, Thareani (2014, 200) pointed to an internal rendering of the delicate northern Negev ethnic fabric at the end of the Iron Age, leading to the destruction of most of the local sites. Within this context, 'Edom' could be interpreted as a local semi-nomadic power seeing an opportunity during Judah's collapse. Guillaume's (2013) criticism goes farther, suggesting that the 'Arad ostraca's allusions to Edom can be interpreted as quarrels over sheep-stealing and grazing rights rather than over open military confrontations.

Few scholars have realized that changes in the understanding of the 'Arad ostraca go beyond the interpretation of the role of Edom in the fall of Judah. Potentially significant for the dating of the Late Iron Negev sites is the disassociation between the destruction of the Late Iron Negev sites and the threat of Edom and, by association, Sennacherib's campaign. Let us study the relevant 'Arad ostraca in more detail.

As we have seen, a most significant linchpin for the dating of Tel 'Arad VIII was the reference in ostracon 40 to a king of Judah facing Edom, interpreted by Aharoni as king Hezekiah's actions in 701 BC. However, some scholars have re-dated 'Arad ostracon 40 to Stratum VI, making it contemporary to ostracon 24, both

on palaeographical and historical grounds. The two inscriptions could refer to the same historical context: both apparently deal with movements of troops in the face of an Edomite attack, both refer to a king of Judah, and intriguingly both refer to an official with the same name, Malkiyahu. Palaeographically, the inscription should date to the late 7th century BC, particularly regarding the shape of letters *aleph*, *bet* and *mem*, and the presence of *matres lectionis* in the forms *'yš* and *ywhd[* (Na'aman 2003, 199; 2015, 50; Dobbs-Allsopp, Seow and Whitaker 2005, 69–70; Faigenbaum-Golovin *et al*. 2016, 4666).

Moreover, the allusions to the unnamed kings of Judah in ostraca 40 and 24 are far from providing absolute dates. As Guillaume has demonstrated in his article on the Edomite threat 'myth', both allusions are less strong than assumed. For ostracon 24, we have the allusions to 'the letter/scribe of the king (*lspr hmlk*)' (obverse, l. 2) and 'the word of the king (*dbr hmlk*)' (reverse, l. 17). The historical context clearly points to a Judaean king, but the dating is open to debate (Guillaume 2013, 100).

For ostracon 40, Guillaume (2013, 103–104) suggests that Aharoni's reading of 'the king of Judah should know' (*yd' mlk yhwd*[*h*]; l. 13) rests on flimsy grounds. First, it is based on a hypothetical restoration of a *he*-letter at the end of *yhwd*[; Aharoni's reading 'king of Judah' was based on his desire to find king Hezekiah in the letter. The unrestored text clearly reads 'king of Yehud' (*mlk yhwd*) rather than 'king of Judah' (*mlk yhwdh*). Guillaume however takes a step back and notes that his own reading is too conjectural to claim that it refers to Nebuchadrezzar II's installation of Gedalyahu as 'king of Yehud' at Mizpah, shortly after the taking over of Jerusalem (2 Kings 25:22). In looking for a solution, he further suggests that there is a *resh-dalet* confusion at the beginning of the line: instead of *yd'*, one should read *yr'* ('will graze/befriend'). Since he notes that a translation 'the king of Yehud will graze/befriend' would not make sense, he prefers the more prosaic reading *yr'.mlkyhw*, 'Malkiyahu will graze/will get involved'.

From this review of the evidence, it is clear that the epigraphic record associating Edom with the destruction of sites in the Iron Age Negev is fragile. For chronological purposes, the dissociation between ostracon 40, Tel 'Arad stratum VIII and king Hezekiah is even more damning, adding to the lack of evidence in the Neo-Assyrian and biblical sources of a wave of destruction in the Negev related to Sennacherib's 701 BC campaign.

In sum, we have reviewed the textual sources that form the cornerstone for dating the Iron Age Negev sites – Neo-Assyrian inscriptions, ostraca in Egyptian hieratic script from 'Ain el-Qudeirat and the ostraca from Tel 'Arad, finding that they are not as secure as usually claimed. None provides firm absolute dates for the destruction of the sites of the Negev by the Assyrians, Egyptians, Babylonians or Edomites. Taking into account these problems, it is clear that a more cautious approach for establishing a chronology of the late Iron Age Negev is needed. Going back to an earlier generation of archaeologists that excavated the Edomite sites in the southern Transjordanian plateau (Bennett, Hart) and following the recent proposal by van der Veen (2020, 106–112), I suggest that the bulk of the 'Edomite' pottery in the Negev dates between the mid-7th and the early 6th centuries BC. A *terminus post quem* for this material culture is established by the Qos-Ga[br] seal impression from Umm el-Biyara (*c.* 670/660 BC), thus coinciding with the apex of the Assyrian influence in the southern Levant. It seems that the 'Edomite' material culture does not post-date the end of the Iron Age Negev sites, even though the exact date and perpetrators of their destruction are not clear. The Hebrew 'Arad ostraca show the existence of Edom as an entity to be dealt with by the Judaean fortresses of the northern Negev. The palaeography, the content, the Hebrew names and the two (problematic) allusions to Judaean kings make clear that we are dealing with Iron Age inscriptions (see also Na'aman 2015b for inscriptions in other northern Negev sites), even if their absolute dates are disputed. The nature of this Edom – an external power or an internal force – and the relationships between Judaeans and Edomites, are still debated.

Conclusion

The purpose of this critical re-evaluation has been to shed light on some pieces of evidence forming the block of the chronology of Iron Age Edom, showing that behind the predominant consensus lie serious problems that need to be addressed. Our reassessment of the evidence suggests that the main phase of occupation at Khirbet en-Nahas should be lowered by at least one century; the elite and monumental architecture dates to the 9th century BC and was deserted by the end of that century or the beginning of the 8th century BC. To the 9th century BC belongs the bulk of the assemblage of Egyptian amulets found at Faynan, certainly a period of strong Egyptian influence in the region, although far from the model of Shoshenq I's military intervention postulated by the excavators. The archaeology of the classical 'kingdom' of Edom presents similar challenges; our re-evaluation has highlighted the several problems involved in the methodology of dating the late Iron Age sites of the Negev by association to presumed military campaigns of the Assyrians, Egyptians, Babylonians and Edomites. A chronology free of these constraints and more consistent with the evidence of southern Transjordan would date the 'Edomite' material culture in the Negev between the mid-7th and early 6th centuries BC.

Twenty years ago, it was still possible to write on the history of Edom with the help of the biblical sources, the Neo-Assyrian inscriptions and some allusion to the archaeology of Busayra, Tawilan and Umm el-Biyara. The more recent research in the Faynan lowlands and the Negev should be commended for having expanded exponentially our knowledge about the early and late phases of Edom's development, but at the same time they have raised more problems. It is hoped that further investigations will help to unravel the chronology of one of the most fascinating regions of the southern Levant.

Abbreviations

ABC	Grayson 1975
ARAB	Luckenbill 1926–1927
CTN 3	Dalley and Postgate 1984
CTN 5	Saggs 2001
KRI	Kitchen 1993–2008
RIMA 3	Grayson 2002
RINAP 1	Tadmor and Yamada 2011
RINAP 2	Frame 2021
RINAP 3/2	Grayson and Novotny 2014
RINAP 4	Leichty 2011
RINAP 5	Novotny and Jeffers 2021
RINBE 2	Weiershäuser and Novotny 2020
SAA 1	Parpola 1987
SAA 11	Fales and Postgate 1995
WSS	Avigad 1997

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Text-Impeded Archaeology: Assyrian Involvement at Samaria and Tell es-Sa'idiyeh

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Scholars frequently speak of the Levantine Iron Age as 'text-aided archaeology', meaning that the existence of historical texts enables the accurate dating of the archaeological strata. This glosses over assumptions concerning the nature and dating of the documents, and the reliability of the links between them and the strata.

Introduction:

The Problem with Linking Texts to Stratigraphy

Documents can be linked to strata either directly, by the discovery in the stratigraphy of a datable inscription, or indirectly, using the taxonomy of stratified pottery. In the first case one must carefully consider the stratigraphic context of the discovered document, and the formation processes (Schiffer 1993) by which that context came into being, whether the document is in primary or secondary deposition, along with the nature of the document, because the nature of the text, its intended audience, can affect the truthfulness of the contents. Small artefacts, such as scarabs, can be moved up or down the stratigraphic sequence by burrowing animals, or by the human re-deposition of soil. Objects may be handed down for generations in use, 'residuality-in-use' (Farmer and Farmer 1982, 104) or may even be discovered and re-used by later occupants of a site.

A second problem of linkage between documents and stratigraphy is the assumption that historical events *must* have left traces of a specific type. This assumed stratigraphy has plagued Levantine archaeology above all other, either directly, on the assumption that a conquest known from texts must be marked in the archaeology by a horizon of destructions on sites across the region, or indirectly by the assumption that significant events in the historical record, such as the reigns of powerful monarchs, must be represented in the archaeological record by the rebuilding of sites. It was assumed that documented conquests *must* have left massive destruction layers. We now know that this is not true. Not only can this assumption influence the interpretation of the archaeology itself, but it can also influence the interpretation of the texts which, for example, speak of the conquest of a city, and are read by the archaeologists as implying the sacking of the city and its violent, fiery destruction. The converse can also be true, of course, as archaeologists, having found a destruction layer, may then go in search of a text to which to connect it, by overlooking the possibilities that a destruction, assuming there is evidence that it was actually due to a violent attack, might not be reflected in any surviving text. Alternatively, it might have been the result of an accident, as in the Great Fire of London in 1666, or the Great Fire of Chicago in 1871. As has been shown, both Egyptian (Hoffmeier 1989, 1990, 1991; however, see Weinstein 1991) and Assyrian (Tappy 2007, 2018) texts which describe conquests have been so misinterpreted. For these reasons I have coined my own term to refer to the Levantine situation: 'text-impeded archaeology'.

The Application

Megiddo: The Disappearing Destruction of Tuthmose III

When I began studying Levantine archaeology as an undergraduate, in 1968, one of the greatest problems being debated was the connection of the conquest account of Tuthmose III, the general truth of which no one seriously doubted, to the stratigraphy of Megiddo, with the implications that connection carries for the chronology of the end of the Middle Bronze Age and the beginning of the Late Bronze Age in the Levant. The problem was that scholars' understanding of Tuthmose's narrative of his great victory in the Battle of Megiddo assumed that he must have sacked and burnt the city, but no such destruction level appeared at the

appropriate point in the stratigraphy of the site. Then Hoffmeier (1991) initiated a debate which re-examined the language of the text itself, concluding that there was nothing in it which implied a destruction of the city. This did not, of course, resolve the problem of which of the strata at Megiddo was contemporary with Tuthmose III, but it did remove the justification for the search for a non-existent destruction level. While this debate continued for some time, by 1995 Bunimovitz (1995, 320–322) could write of it in the past tense.

Tell es-Sa'idiyeh: The Anonymous Destructions

At Tell es-Sa'idiyeh we encountered three major destruction levels, each of which could be characterized by both what was, and what was not, present. Using

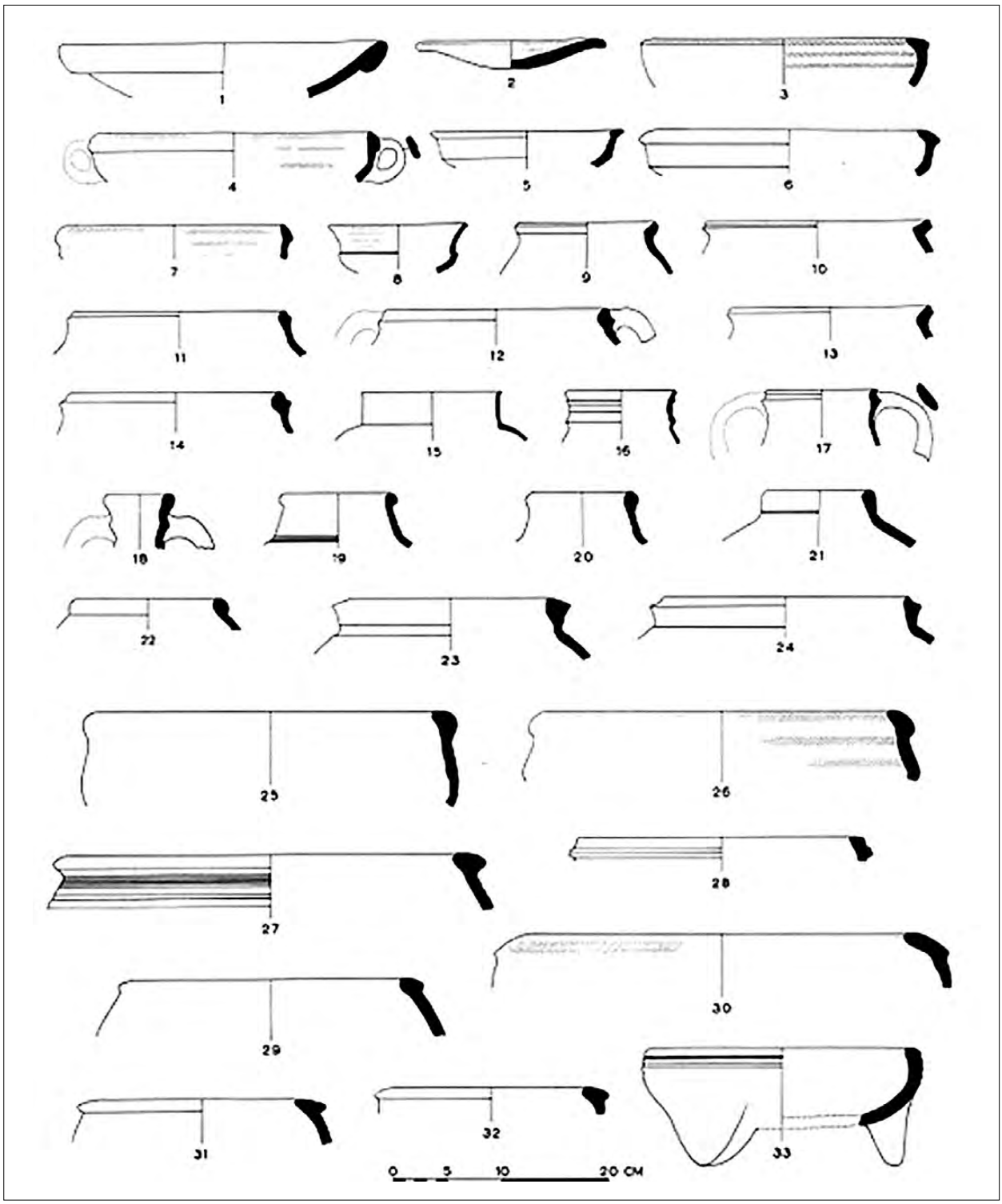

Figure 1a. Pottery from Tell es-Sa'idiyeh found in Stratum IV (after Pritchard 1985).

these criteria, we came to very different conclusions as to the nature and causes of the three destructions.

1) *Early Bronze Age II: Accidental destruction*

The British Museum excavations at Tell es-Sa'idiyeh, Jordan, directed by Jonathan Tubb, reached the EB II occupation on the Lower Tell, where it was discovered that the entire area was covered by a single building, within which there were areas which were devoted to the processing of grapes, olives and weaving. Four strata were encountered, of which L1 and L2 were extensively excavated. L1 was discovered to be a squatter occupation on the ruins of the L2 occupation, which had been destroyed by an intense fire. The fire appears to have begun in the South-East corner of the building, almost certainly in a sunken room which was filled with pithoi of olive oil. In an adjacent room, which we labelled 'the scullery', were found the remains of a feast for eleven people, featuring fresh grapes, pomegranates and olives. No human or animal remains were found, nor were any remains of weapons, neither projectile points nor slingstones nor cutting weapons. The unwashed dishes in the scullery indicated that the fire came as a surprise, while the fresh fruits indicated that the meal and the subsequent fire took place in late May or early June, which, in the Jordan Valley is awesomely hot, so the meal probably took place after sundown, when it was slightly cooler. Taken together, the evidence suggests an accidental fire.

2) *Late Bronze Age III: Deliberate, but not hostile, destruction*

The last LB occupation, Stratum XII, was an Egyptian fort, guarding the fords of the Jordan and the north-south road along the east side of the Jordan Valley. Wherever Stratum XII was reached, a thick layer of ash from the fiery end of the stratum was the first sign that we were reaching it. In several rooms we found large numbers of store jars crushed in place by the fall of the roofs or upper floors of the buildings. In one sunken room I encountered a thick deposit of grain, charred by the fire, but there were no other objects,

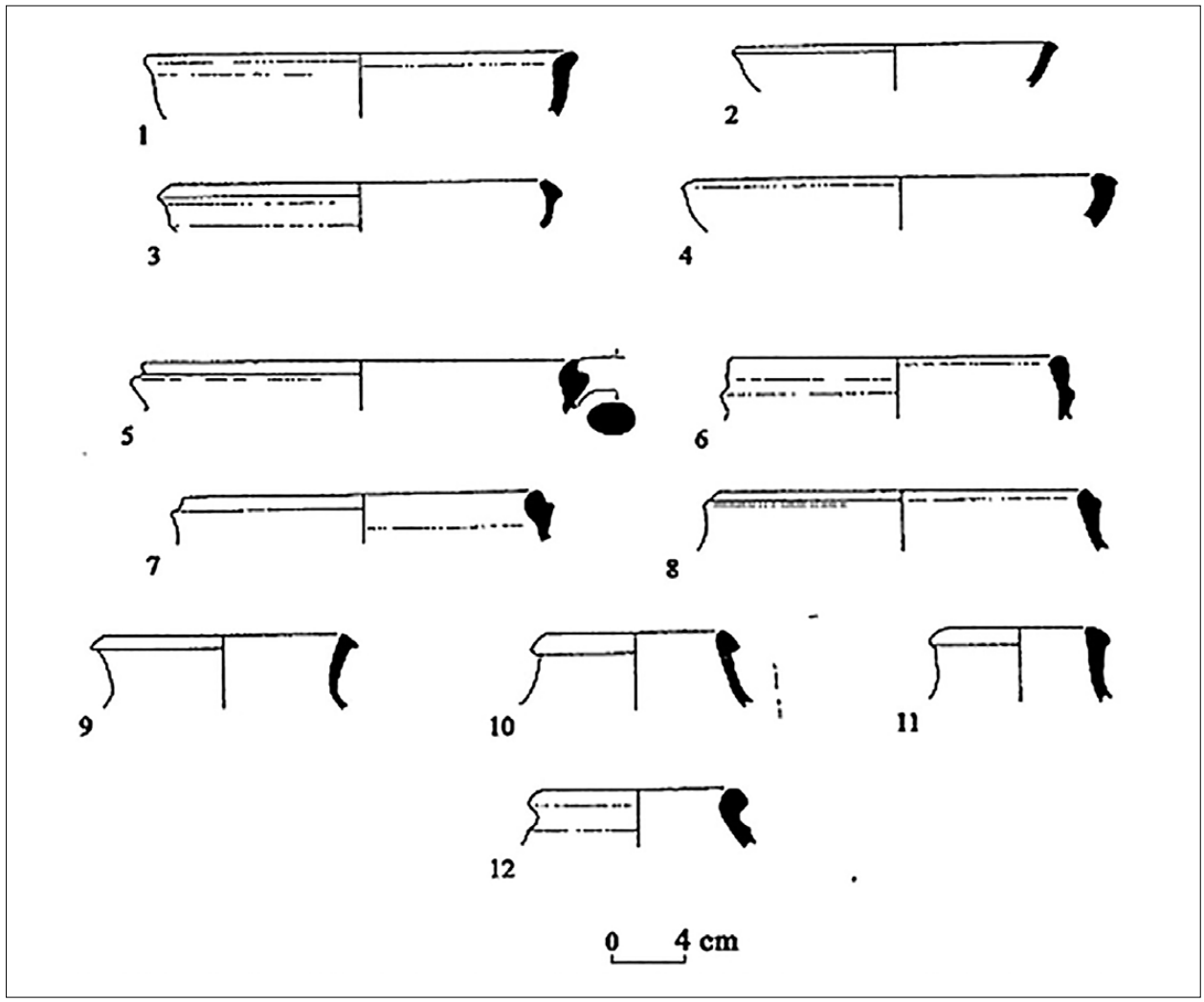

Figure 1b. Pottery from Tell es-Sa'idiyeh found in Stratum IV (after Tubb 2020).

Strata	Joint Expedition Periods	Chapman 1989 Column	Chapman Culture-Historical	Kenyon's Dates	Wright's Dates♣	Avigad's Dates♦	Tappy's Dates@	James' Dates♠
XX	I	X=Iron IIA	Kingdom of Israel	Omri (882-871 BCE)	Omri & Ahab	Omri	880-841	880-790 BCE
XIX	II	Y=Iron IIA	Kingdom of Israel	Ahab (871-852 BCE)	Jehu	Ahab		790-750 BCE
XVIII	III	Y=Iron IIA	Kingdom of Israel	Jehu (842-814 BCE)	Jeroboam II	Jehu	841-815	750-722 BCE
XVII	IV	Z-AA=Iron IIB	Kingdom of Israel	Jeroboam II (784-748 BCE)	749-	Jeroboam II	(IV)815-765 (IVa)765-732	722-?675 BCE
XVI	V	Z-AA=Iron IIB	Kingdom of Israel	749-722 BCE		749-	732-722	?675-
XV	VI	Z-AA=Iron IIB	Assyrian Empire	722-630 BCE	-722 BCE	722 BCE	-Early VII) 700-(	-?630 BCE
XIV	VII	AB=Iron IIC	Babylonian Empire	630-539 BCE		Assyrian		?630-539 BCE
XIII	VIII	AB=Iron IIC	Persian Empire	539-323 BCE		Babylonian & Persian	(Late VII & VIII) 650-587	539-323 BCE
XII	IX	AC-AD=Persian Empire	First Hellenistic	First Hellenistic# 323-??? BCE		Hellenistic		Hellenistic
XI	Early Hellenistic	Hellenistic	Early Hellenistic	Early Hellenistic		Hellenistic		
X	Hellenistic Fort Wall	Hellenistic	Antiochus IV Epiphanes	Hellenistic Fort Wall		Hellenistic Fortress		
IX	Late Hellenistic	Hellenistic	Late Hellenistic 1	Late Hellenistic		Maccabean		
VIII	'Latest Pre-Herodian'	Hellenistic	Late Hellenistic 2	'Latest Pre-Herodian'		Maccabean		
VII	First Roman/Herodian	Roman	Foundation of Sebaste 30 BCE	First Roman/Herodian		Foundation of Sebaste		
VI	Second Roman	Roman	Roman	Second Roman		Roman		
V	Third Roman	Roman	Roman	Third Roman		Roman		
IV	Fourth Roman	Roman	Roman	Fourth Roman		Roman		
III	Byzantine	AN-AO=Byzantine	Byzantine	Byzantine		Byzantine		
II	Crusader	AS=Crusader	Crusader	Crusader		Crusader		
I	Islamic	AT-AV=Islamic	Islamic	Islamic				

Figure 2. Samaria Strata and suggested dates. For the explanation of symbols, see legend on p. 351.

no metalwork, no bone or ivory objects, nothing which would have been worth carrying away. Most significantly, there were no weapons, neither cutting weapons nor slingstones nor projectiles. But the most significant discovery in the ruins of Stratum XII was that several doors had been blocked before the fire, as if the fort had been partially abandoned before the end of the occupation. Our conclusion is that the Egyptian garrison was withdrawn, perhaps over a period of time, and before the final detachment abandoned the fort, they 'slighted' it, setting fire to the buildings so that no one else could use it against them in the future. The end of Stratum XII was a destruction, but not one by enemy action (for a similar view on coastal cities during this period, see Millek in this volume on pp. 205–217).

Iron Age II: Destruction by Enemy Action

At Tell es-Sa'idiyeh the destruction of Stratum V, marked by burning and ashy destruction debris, bodies of animals, arrowheads and slingstones of the type that were also in use during the Neo-Assyrian period, dates to *c.* 730 BC, and was followed by an accumulation of erosion debris, leaving a single administrative building on the high southeast corner of the tell to be re-used in Stratum IV. Over the whole of the rest of the Upper Tell large deep pits have been found, with residues of chaff and grain surviving in the bottoms, indicating that they were used for temporary storage of the harvest from the fields of the surrounding district. Significantly, these pits are cup-shaped and unlined, unlike the cylindrical and brick-lined pits used for grain storage at Persian-period sites in the southern Levant. The dating of this destruction rests on three bases: (1) in terms of the pottery, it falls at the latter end of the Iron IIB sequence at Sa'idiyeh; (2) the pottery from all areas in which Stratum IV was reached belongs to the same horizon (*Figure 3*), which is also the ceramic horizon found in the Stratum IV building; and (3) the radiocarbon dating obtained for the Stratum IV pits by the University of Pennsylvania excavations.

Samaria: The Disappearing Assyrian Destruction

At Samaria, John W. Crowfoot, stated that there was no Assyrian destruction (Crowfoot, Kenyon and Sukenik 1942, 24), while Kathleen Kenyon declared that there was (Crowfoot, Kenyon and Sukenik 1942, 110). Ron Tappy's restudy of Kenyon's excavation records supports Crowfoot (Tappy 2001, 222–226). This does not necessarily question the accuracy of the documents, or even her connection of the texts to the stratigraphy (as my chart shows *Figure 2*) but it means that Kenyon's understanding of the texts and her interpretation of certain aspects of the stratigraphy must be scrutinised. On these points I am in complete agreement with Tappy. Kenyon suggested that Period V was the last Israelite stratum; and Tappy agrees (1992, 253; 2001, 222–226), although, like Forsberg (1995, 49–50), he sees no evidence of destruction. Three observations are significant here: (1) sufficient evidence exists from the excavations to date Period VI to the Assyrian Empire, Period VII to the Babylonian Empire, Period VIII to the Persian Empire and Period IX to the earliest Hellenistic Period; (2) robber trenches were cut in the reign of Antiochus IV Epiphanes to remove the masonry from the platform and palace walls; and (3) the Samaria Ivories were dumped as fill during Antiochus IV's remodelling of the summit. It remains my conclusion (Chapman 2017) that the use of the Omride Palace in Samaria continued until Antiochus IV's remodelling of the summit of the site, however, this does not imply that the rest of the Omride Royal Compound continued to be used for its original purpose.

Thus, the stratigraphy of Samaria offers us some points which can be linked to the historical record with some certainty, the most significant for Iron Age Chronology being (1) the construction by Omri of the first phase of the Royal Compound, *c.* 880/870 BC, and (2) the end of the Kingdom of Israel at the end of Period V. The scholarly argument, therefore, must be about the dating of Periods II–IV. My own current view is that if we accept that Period V is the last period of the Kingdom of Israel, then we must account for the apparent period

§Construction of Fortress by Antiochus IV Epiphanes	♣Samaria - EAEHL	♦Samaria - EAEHL2	‡Samaria-Sebaste I:Page 27	*Defeat of Jehoahaz by Ben-Hadad III
ђDestruction by John Hyrcanus I	@Tappy 1992:253-Appendix A	#Samaria-Sebaste III:217	♠Centuries 1991:187	†Recovery under Jehoash

Figure 2: Legend to Samaria Strata.

Kenyon, Wright and Avigad's Assignment of Samaria Pottery to Strata (New EAEHL Volume 4:1303)						
Kenyon's Culture-Historical Assignment	Kenyon		Wright		Avigad	
	Building	Pottery	Building	Pottery	Building	Pottery
Early Bronze		Present		Present		Present
Property of the Shemer Family (10th-early 9th Century B.C.E.)				1-2		1-2
Omri (882-871 B.C.E.)	I	1	I	3	I	3
Ahab (871-852 B.C.E.)	II	2			II	
Jehu (842-814 B.C.E.)	III	3	II		III	
Jereboam II (784-748 B.C.E.)	IV	4	III	4	IV	4
749-722 B.C.E.	V-VI	5-6	IV-VI	5-6	V-VI	5-6

Figure 3. Assignment of Samaria pottery to strata by Kenyon, Wright and Avigad.

of semi-abandonment which is represented by Period IV. It seems to me that the easiest way to do this is to assign this stratum to the period between the defeat of Jehoahaz by Ben-Hadad III in *c.* 810 BC and the recovery under Jehoash in *c.* 790 BC.

An Unhinged Dating

The real problem with the dating of the Iron Age in the southern Levant is with the assumptions made by the pioneers of Levantine archaeology in the 1920s. Convinced that the biblical account of the reigns of David and Solomon required a period of major public architecture in the Iron Age, they assigned the Iron IIA horizon, which featured such a cultural efflorescence, to the tenth century BC. This was a perfectly reasonable working hypothesis, but it should never have been treated as a fact, but as a hypothesis for testing. When Kenyon's final report on Samaria appeared in 1957, instead of rethinking their interpretation of the dating and their reading of the biblical account, Wright (1959) and Avigad (1978) chose to seize on one sentence in Kenyon's report and interpret that to mean that all of the pottery from each of her strata came from beneath the floors of that stratum. This *post hoc* accommodative argument (Binford 1981, 84) enabled them to save their *earlier* dating. Unfortunately, this fallacious argument was aided and abetted, although not justified, by that single sentence in Kenyon's report (Crowfoot, Crowfoot and Kenyon 1957, 90), plus the fact that her report did not include the information recorded by G. M. Crowfoot in the excavation records as to the exact deposit from which each registered artefact came, which has now been published by Tappy (1992, 258–260, Appendix F; 2001, 587–590, Appendix A). Using Tappy's published versions of Kenyon's section drawings and his analysis based on the information in her field notebooks, I created a set of Harris matrices of all of the stratigraphic information, and then, using G. M. Crowfoot's register, I was able to link the pottery to the stratigraphy and show that, contrary to the argument of Wright and Avigad, and, indeed, to Kenyon's own statement, the pottery from the stratigraphic 'periods' did *not* all come from beneath the floors of those periods. This false notion, which separates the artefacts from the layers in which they were found and arbitrarily re-assigns them to different layers from which they did *not* come, persists in the literature to this day. Because of this de-coupling of the artefacts from the stratigraphy to save the attribution of the Iron IIA-horizon to David and Solomon, there is still a debate over the chronology of the Iron Age stratigraphy of the southern Levant, with the proponents of the traditional dating even going so far as to demand that the dating of the rest of the eastern Mediterranean basin be adjusted to fit the anomalous dating of the material in the southern Levant. Samaria is, therefore, the classic illustration of text-impeded archaeology, both in terms of the ancient Biblical and Assyrian texts and the modern site report. In fact, while those who wish to reassign the pottery to strata from which it did not come derive their understanding of her methodology from that single sentence in the Samaria report, she provided an entire book which sets out in detail her method, in the field and after, to which they never refer. Perhaps they considered her publication *Beginning in Archaeology* (Kenyon 1952) to be too basic to be relevant, and yet if they had looked at it more carefully, they would have found a detailed methodological treatise, which includes direct references to the Samaria material, written and published while she was preparing that material for publication.

Conclusions

Consequently, I can now suggest my own conclusions as to the dating of the strata at Samaria and Sa'idiyeh (*Figure 4*). In addition to the arguments cited above, I should note that the lower dates given for the

Comparative Chronology of Sa'idiyeh and Samaria Strata						
Sa'idiyeh Strata				Samaria Strata		Ceramic Horizon
General	Acropolis	Acropolis Building	Dates	Periods	Dates	
III	III A	Building IIIA/1	400-332	VIII	539-323	Persian (Iron Age II C/III)
	III B	Building IIIB/1	499-400			
	III C					
	III D	?	600-500	VII	630-539	Babylonian (Iron Age II C/III)
IV	III E	Building V-IV=IIIG-IIIE/1/iii	733-600	VI	722-630	Assyrian (Iron Age II B)
	III F	Building V-IV=IIIG-IIIE/1/ii				
V	III G	Building V-IV=IIIG-IIIE/1/i	750-733			Iron Age II B
VI	Village		790-750	V	790-722	Iron Age II A
VII	Planned Cloth Production Town		825-790	IV	810-790	Iron Age II A
				III	???-810	Iron Age II A
VIII	Industrial Processing Site		860-825	II	???-???	Iron Age II A
				I	880-???	Iron Age II A

Figure 4. Comparative strata at Tell es-Sa'idiyeh and Samaria.

Iron Age IIB are mostly compatible with the recent publication on the subject by Pieter van der Veen (van der Veen 2020). So far as it is within my ability, I have endeavoured to ensure that these conclusions are based on the archaeological evidence, and not on a reading of the texts shaped by what I might wish they said, one which avoids those logical errors which led the great pioneers of Levantine archaeology to reach conclusions which do not stand up to critical scrutiny.

There are three specific sources of error which lead to text-impeded archaeology. The first is, if you like, the most innocent, the simple uncritical acceptance of untested hypotheses about the meanings of texts, such as the assumption that the mere mention of an armed conquest implies an archaeologically obvious destruction level, or that the reign of each new monarch implies a new stratum on an archaeological site, or even that an ancient text necessarily tells 'the truth, the whole truth, and nothing but the truth'. The second source of error is, perhaps, the most ancient, namely, the uncritical acceptance that religious dogma, regardless of the religion concerned, constitutes a necessary truth. The third source of error is the most recent, that is, nationalist propaganda. In every part of the world there are examples of the malign effects of these three sources of error on our understanding of the past of humanity, even of current events.

This short paper focusses on the Assyrians at Samaria and Tell es-Sa'idiyeh, but if you want to take a deep dive into just how serious this problem can be, I recommend a careful reading of *Selective Remembrances: Archaeology in the Construction, Commemoration, and Consecration of National Pasts* (P. L. Kohl *et al.*, 2007)

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Discussion 3
Neo-Babylonian and Achaemenid Period Pottery Markers from Tell es-Sa'idiyeh Stratum IV?

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As was mentioned by Rupert Chapman on pp. 347–354 above, the destruction of Stratum V at Tell es-Sa'idiyeh was assigned by the excavators to the Neo-Assyrian conquest of Tiglath-pileser III, *c.* 730 BC. Consequently, the original excavator James Pritchard assigned Stratum IV (Upper Tell) – an area 'devoid of buildings' with '98 circular pits and two rectangular bins' overlying houses of Stratum V and used for storage of wheat (Pritchard 1985, 39; Chapman *supra*) – to the late 8th and 7th centuries BC (Pritchard 1985, 80). Chapman follows his view, except that he also assigns the re-use of an administrative building to this stratum. Chapman further argues that the storage pits are 'cup-shaped and unlined, unlike the cylindrical and brick-lined pits' of the Persian period in the southern Levant and that 'the pottery of all areas in which Stratum IV was reached belongs to the same horizon'. Hence if Stratum V ended in *c.* 730 BC (as per Pritchard), Stratum IV would have soon followed without much of a hiatus in occupation. Hence the excavators suggest that Stratum IV started during the late eighth century. But this author finds this conclusion quite unacceptable as some of the more diagnostic pottery and other markers suggest a considerably later start for that stratum. While Pritchard is correct that 'smaller sherds could easily have been washed down from the later buildings to the south' (1985, 39), the first two pieces presented here are not particularly small:

a) A 'wedge-impressed' bowl sherd (Pritchard 1985, Fig. 16:14): this bowl sherd (which measures 15 x 16cm) is decorated with cuneiform-style wedges (see *Figure 1*), which had been applied to the soft clay before firing. The style has been studied by Ephraim Stern (1982, 133–136) and Jeffrey Zorn (2001, 689–698), both of whom assign it to the final years of the Neo-Assyrian empire and to the successive Neo-Babylonian and Achaemenid periods, i.e. from *c.* 600–330 BC (van Volsem 1987, 48–53; Zorn 2001, 689–698; van der Veen 2014, 48–49).[1] Although the 'wedges' resemble the impressions of a cuneiform stylus, only a few specimens have been found in northern Mesopotamia, especially from the Neo-Assyrian period (Stern 1982, 133; van der Veen 2014, 48; Itach *et al.* 2017, 89). The situation is slightly different in central Mesopotamia, where they have been found in the Wadi Diyala region, east of Kish. Strikingly, these date to the 6th century BC or later (Itach *et al.* 2017, 85). Most of the attested pottery, however, has been found in the territories of Judah and Benjamin (van der Veen 2014, 92), the central highlands and east of the Jordan River (Itach *et al.* 2017, 74–76, 80 with Fig. 8).[2]

[1] Due to the discovery of some 'wedged-impressed' and related 'circle-impressed' pottery in Stratum VA at Ramat Rahel and in Stratum III at Beth Zur, Nancy and Paul Lapp have argued for an origin of this pottery towards the end of Iron Age IIC, suggesting a date near 600 BC (Lapp and Lapp 1968, 68; also Zorn 2001, 694). Even so, many pieces were found in strata which continued undisturbed after the Neo-Babylonian incursions after 604 BC and in those that were reoccupied during the early Persian period (van der Veen 2014, 48). A squarely sixth–fifth century date is also supported by discoveries underneath the so-called 'Nehemiah Wall' on the eastern slopes of the City of David. The strata in which these occur (i.e. Layers 10-2/1 to 9-8), contain accumulated material stretching from the Babylonian destruction through the Exilic period and into the early years of the resettlement under Achaemenid rule (Mazar 2009, 74–76; Shalev 2015, 217 and Figs. 4.1.34, 4.2.7, 4.4.12–16, 4.5.10, 4.7.32, 4.9.38–39, 4.11.21 and 23–24, 4.12.44–47). These layers for instance contained official seal-impressed Neo-Babylonian bullae (Winderbaum 2015, 375, no. 2) and early Persian period carrot-shaped juglets (Shalev 2015, 217 and Figs. 4.4.6–7, 4.7.31, 4.10.19).

[2] In their detailed article (listing several more pieces of this style, Itach *et al.* (2017, 72–97) suggest higher dates for this pottery style (starting during the late 8th century BC), but their attribution is largely based on the traditional dates for Iron Age IIB, e.g. at Horvat 'Eli (north of Shiloh), Tell Jawa and Tell es-Sa'idiyeh (!). However, some of the cited strata at several sites were of long duration and hence the exact timeframe for this pottery cannot be firmly established. At other sites, their early date has been questioned, for instance at Tell Qudeirat (Stratum 3a–b), which David Ussishkin dates to the seventh century. Many sherds also derive from surveys conducted in the Manasseh Hills, whose dates likewise are uncertain. While the authors suggest a relationship of this pottery with Babylonian migrants during the 7th century BC, they too admit that many 'wedge-impressed sherds' are late in date and occur predominantly in late Iron Age IIC strata and levels that date to the Neo-Babylonian and Persian periods.

Some sherds were also found on the Arabian Peninsula, at Qasr al-Hamra near Tayma (Zorn 2001, 690–692, Fig. 3.22.12, 245). Because of their occurrence in Arabia, Zorn has suggested a relationship with a Mesopotamian controlled Arabian trade network, which flourished there during the late 7th to 4th centuries BC (Zorn 2001, 693–695). As most stratified 'wedge-impressed' bowls occur in fairly late contexts (as in Babylonia), it appears unlikely that their introduction should be dated much before the end of the 7th century BC.

Figure 1. The 'wedge-impressed' sherd from Tell es-Sa'idiyeh Stratum IV (obj. no. 86-18-364, courtesy of the Penn Museum).

b) A flat-based open oil lamp (Pritchard 1985, Fig. 16:18): this type substantiates the later date for Stratum IV. It too is not found before the final years of the Neo-Assyrian period, becoming more popular during the Neo-Babylonian (see Samaria Pottery Period VII: Tappy 2015, 327, 340, Plate 3.2.8, no. 3) and Achaemenid periods (Stern 1982, 127–129; 2015, 613, nos. 1–7).

Figure 2. A similar flat-based oil lamp from Tell es-Sa'idiyeh Stratum III (obj. no. 86-18-788, courtesy of the Penn Museum).

Another identical flat-based lamp was found at Tel es-Sa'idiyeh, evidently in Stratum III from the Persian period, and it is currently kept at the Penn Museum (object no. 86-18-788[3] ; *see Figure 2*), where the curators date it to the 5th century or later.[4]

c) Aramaic ostraca (Pritchard 1985, 86–87, Fig. 175:1–2): only one of them was evidently found in a stratified context, namely in Stratum IV (S889/S3). Both ostraca are written in Aramaic script, and both seem to squarely belong to the same palaeographic horizon. Both were studied by Jonas Greenfield and Frank Cross and were drawn and translated by Ada Yardeni (Pritchard 1985, 86–87, Fig. 175:1–2). As the albeit unstratified ostracon (S1143/S13; obj. no. 86-18-634, *see Figure 3*) could be studied by the author on the Penn Museum website,[5] the palaeographic traits (including open headed *ʿayins* and *reshs*) suggest a date not before the end of the 7th century. A sixth-century date seems even more likely.[6]

Figure 3. The unstratified ostracon from Tell es-Sa'idiyeh, likely also from Stratum IV (obj. no. 86-18-634, courtesy of the Penn Museum).

Also other objects suggest a later date for Stratum IV, but a full discussion would go beyond the scope of this discussion.[7]

[3] https://www.penn.museum/collections/object/264116.

[4] But their broad date range (410–180 BC) is evidently based on ^{14}C results already referred to by Pritchard (1985, 79) which should no longer be trusted by today's standards.

[5] https://www.penn.museum/collections/object/258732.

[6] For similar Aramaic ostraca from Transjordan (for instance from Tall Hisban and Tall al-Khalayfi), see Cross (1969, 224–229, Plate XXV: B) and DiVito (1993, 55–57, 58–62, Plates 81–82). Concerning the development of Aramaic diagnostic traits in Ammonite script toward the end of Iron Age IIC, see van der Veen (2014, chapter 2).

[7] Including an inscribed jar from Stratum IV, studied by A. Lemaire, who dated it to the end of the 7th century BC, see Pritchard (1985, 79, 86, Fig. 175:3a–d).

If we accept the conclusions arrived at above – that a) Stratum V and Stratum IV were close in time, and b) that the pottery styles from Stratum IV suggest a late seventh- to fifth-century date – the end of Stratum V would also need to be lowered. A date for its destruction near the middle of the seventh century (or even later) would be considered likely, even if it remains unknown for the time being who was responsible for the destruction at the site. As a matter of fact, this only confirms Rupert Chapman's overall warning, not to put too much trust in 'text-impeded stratigraphy', something with which we would fully agree (van der Veen 2020, 72–74, 106–112).

In a similar vein, Peter James has suggested lower dates for Hazor Stratum VA (James 2008, 137–183), whose pottery repertory according to Pritchard is apparently reminiscent of Tell es-Sa'idiyeh Stratum V (1985, 79). The same can be said of Building Phase V at Samaria (James *et al*. 1991, 183–188). Its lower dates have been accepted by the present author, especially because of the imitated Neo-Assyrian pottery styles found in that layer (van der Veen 2020, 93; also in this volume see pp. 291–310).[8]

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[8] For a similar suggestion on lower dates for traditional 'Iron Age IIB' sites, see Fantalkin and Tal (2015, 193–195).

Discussion 4: *In dubio pro reo.* The Case of Sennacherib and the Beersheba Valley*

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I welcome Ariel Bagg's paper (in these proceedings, pp. 311–321) and his persuasive arguments that Sennacherib did not destroy the cities in the Beersheba Valley in 701 BC. He shows clearly how the scope of Sennacherib's campaigns in the southern Levant has been exaggerated by modern historians. Given this, I am puzzled by his adherence to the date of 701 BC for the destruction of Lachish Level III. In note 16 he explains his position:

> Most scholars accept the destruction of Lachish III in 701, and there are no serious reasons to doubt it. The dating based on the archaeological work on the site is supported by the written and iconographical sources and fits the data; see discussion in van der Veen (2020, 172–176). The excavations by David Ussishkin confirmed 'beyond any doubt' that Stratum III was destroyed by Sennacherib (Finkelstein 1994, 171). The objections to this dating are based on assumptions and are merely speculative. A later date – presumably 648 – was proposed based on 2 Chron. 33:11–13, where an assumed military campaign by Assurbanipal and Manasseh's captivity in Babylon are mentioned (van der Veen 2020, 171–172, with literature and discussion). There is no evidence for such a military action in Assurbanipal's inscriptions. James (2007, 214–215) considers that 'the logic for dating of Lachish III's destruction to 701 is seriously flawed' because Lachish III was destroyed by fire and the burning is attested neither in Sennacherib's inscriptions nor in the reliefs. This is in fact no convincing argument as firstly, the conquest of Lachish is not mentioned at all in the Assyrian written sources, so that the necessary information is not available. Secondly, the reliefs focus on the siege and assault of the city, on what happened before the Assyrians got in and burnt it. James (2007, 216) also refers to the problematic relation between the two stages of construction of the siege ramp; for a possible explanation see Ussishkin (1990, 64–69). Even if there are still open questions concerning this matter, most archaeological evidence points to Sennacherib's ramp fitting its depiction in the reliefs.

First, the statement that 'the conquest of Lachish is not mentioned at all in the Assyrian written sources' is simply incorrect. It appears in the caption to the Lachish reliefs in the British Museum (see below) – without it we would not know that the city depicted as under siege was Lachish! Second, the citation of Finkelstein (1994) is pointless as that author (not a Lachish excavator nor someone with no new observations to add) was merely giving the consensus opinion, citing David Ussishkin (the excavator) as his authority. Endless citation of the same thing seems endemic in discussions about Lachish and gets us nowhere. Nor does invoking the claim that Ussishkin confirmed 'beyond any doubt' that Level III was destroyed by Sennacherib. That is merely opinion and no more than that, however much it is repeated.

So what exactly did Ussishkin find at Lachish as proof of his claim? Rather than merely repeating others' opinions, we need to wind back and delve in some detail into the early site reports – for the benefit of those who may not have read them. As far as the published results of the renewed excavations are concerned, there is nothing in them to show that the 701 BC date was confirmed by 'new data', as is frequently claimed.[1] Rather, as can be seen from Ussishkin's key paper (1977), his case was based entirely on a negative argument: that he could find no 'suitable candidate' for the alleged destruction of the city by Sennacherib in any level before Lachish III.

The fundamental assumption behind this argument is questionable. In his main study of the Assyrian attack, Ussishkin (1977, 30) admitted that: 'the burning and destruction of Lachish are not specifically recorded in Sennacherib's annals'.[2] Yet later in the same paper the idea that the city was burnt to the ground by Sennacherib is treated as fact and used to eliminate

[1] Before the conclusion of the excavations, Ussishkin (1976, 3) rightly eschewed the idea that the royal storage stamps (*lmlk*) were the decisive dating factor in dating Lachish III, and expressed his hope that the stratigraphy would provide a date for them: 'the destruction date of Level III becomes crucially important for fixing the chronology of the royal storage jars. The destruction date of Level III is a controversial issue, and it is hoped that the renewed excavation will uncover new data which will enable us to settle this point conclusively.'

[2] Unfortunately, a remark of this kind is missing from the brief discussion in the final reports, (Ussishkin 2004, 88–89), though it contains the familiar claim that the link between Sennacherib's 701 BC campaign and the destruction of Lachish III is 'presently unanimously accepted in the scholarly world'.

possible candidates lower in the stratigraphy than Level III. Lachish Level IV, immediately beneath III, was ruled out because its destruction was not sufficiently catastrophic (Ussishkin 1977, 51–52): 'considering that the fortifications remained intact, we can hardly identify this level with the city which was stormed and completely destroyed in the fierce Assyrian attack.'

But one has to ask why, when the Assyrian kings generally delighted in recording the devastation and burning of rebellious cities, such a fate was not mentioned for Lachish or the other fortresses of Hezekiah. The most detailed version of Sennacherib's Judaean campaign in 701 BC reads as follows (trans. by Grayson and Novotny 2012, 115–116):

> Moreover, (as for) Hezekiah of the land Judah, I surrounded (and) conquered forty-six of his fortified cities, fortresses, and small(er) settlements in their environs, which were without number, by having ramps trodden down and battering rams brought up, the assault of the foot soldiers, sapping, breaching, and siege engines. I brought out of them 200,150 people, young (and) old, male and female, horses, mules, donkeys, camels, oxen, and sheep and goats, which were without number, and I counted (them) as booty.
>
> As for h[im] (Hezekiah), I confined him inside the city Jerusalem, his [roy]al [city, like a bird] in a cage. I set up blockades against him and made him dread exiting his city gate. I detached from his land the cities of his that I had plundered and I gave (them) to Mitinti, the king of the city Ashdod, Padî, the king of the city Ekron, and Ṣilli-Bēl, the king of the city Gaza, and thereby made his land smaller. To the former tribute, their annual giving, I added the payment (of) gifts (in recognition) of my overlordship and imposed (it) upon them.

Note that the ramps, battering rams and other siege methods referred to do not necessarily mean the destruction and burning of a given city's walls. The fortifications of Iron Age Judah were extremely strong and, to achieve the goal in mind of seizing control of a city and its booty, aggressors would have concentrated their efforts at breaking through the gateway, usually the weakest point of a city. In lieu of any reference by Sennacherib to destruction by fire at Lachish, Anson Rainey (1975, 52) cited passages from completely different campaigns in Sennacherib's Annals to show that he could order such devastations, for instance in one campaign against a Babylonian rebel (Luckenbill 1924, 35): 'His towns I overthrew, I razed, I turned into ruins.' But such references only make the silence of the Annals regarding any burning of Lachish and the other cities of Hezekiah even more conspicuous.

Rainey's paper was published in the somewhat overlooked *Lachish V* volume, published by Yohanan Aharoni, and is revealing regarding how the Sennacherib 701 BC link with Lachish III came to be the prevailing model. Rainey (1975, 53) was convinced that *all* 46 cities or towns mentioned were destroyed and burnt to the ground. Yet Rainey was aware of and cited the commonsense opinion of Dougherty (1930, 166, emphasis added) regarding Sennacherib's Judaean campaign:

> The destruction of cities was partial. The devastation of the land seems to have been superficial rather than thoroughgoing. Phraseology usually employed in cuneiform inscriptions to denote complete destruction is not found in the texts referring to the capture of the cities of Judah. They were not 'destroyed, torn down and turned to mounds,' neither were their walls 'leveled to the ground.' *The words expressing these processes are not used in a single one of the four Assyrian versions of what Sennacherib accomplished in Judah.* One may infer from this that the cities of Judah were not left in a condition of absolute ruin. Their walls were enormously thick, and it would have required an immense amount of time and energy to raze them completely. It is apparent this part of the task of rendering the people defenceless was not performed. Recovery on the part of Judah was probably rapid.

Dougherty was right regarding the difficulty of destroying the Judahite cities. Mighty as the Assyrian war machine was, the idea that Sennacherib destroyed all 46 is completely far-fetched. It is almost as ludicrous as the hackneyed assumption that the topographical list of Pharaoh Shoshenq I is composed of 150 cities levelled in a wave of destructions, and allegedly identified in the archaeological record as the 'Shishak horizon' (see James 2002).

Regarding Dougherty's second point – about the words normally used by Assyrians to describe complete destructions – Rainey (1975, 53) conceded that 'these phrases have been omitted with regard to the Judaean campaign' but attempted to skirt around the problem by noting that 'the emphasis in the Judaean section is on the military technology employed, something which is lacking in the other passages.' But Assyrian documents could, of course, emphasise both military technology and thorough destruction for a given site. See, for example, Sennacherib's account of the capture of a city in southern Anatolia (Grayson and Novotny 2012, 136):

> Against the city Tīl-Garimme, a city on the border of the land Tabal ... I sent archers, shield and lance bearers, chariots, (and) horses of my royal contingent against it. They besieged that city and took possession of that city by means

Figure 1. Assurbanipal's army destroys the fortress of Hamanu in Elam (c. 645–635 BC; British Museum Trustees, London, BM 124919).

> of piling up earth, bringing up battering rams, (and) the assault of foot soldiers. They counted the people, as well as the gods, living inside it as booty. They destroyed (and) devastated that city. They turned (it) into a mound of ruins (lit. 'a mound and ruins').

Further study of the records of Sennacherib reinforces the impression that the wording used in the descriptions of the Judaean campaign is highly significant. As alluded to by Dougherty,[3] the Nebi Yunus Slab Inscription specifies the destruction or burning of enemy towns in the entries for every one of the first eight campaigns listed, *except* for that against Judah,[4] where the campaign record simply reads (Grayson and Novotny

[3] I was unaware of Doughtery's excellent article when I wrote my review of Ussishkin's final excavation reports (James 2007, 214). There I noted that: 'In fact, his Nebi Yunus Inscription (Luckenbill 1927, 153–154) specifies the burning and/or utter ruination of enemy towns in the entries for each of his first seven campaigns – except for that against Judah.'

[4] And with the further exception of the third entry, not a military campaign but a report on the demotion of Luli, king of the vassal state of Sidon, and his replacement by Tu-Ba'lu.

2012, 222): 'I ruined the wide district of the land Judah (and) imposed my yoke on Hezekiah, its king.'

The Annals recording this campaign are supplemented by the famous reliefs depicting the siege of Lachish found in Room 36, dedicated to their display, in Sennacherib's South-West Palace at Nineveh and are now in the British Museum. A series of 12 panels show (from left to right) the attacking Assyrian army, the storming of the city, the execution of prisoners, the removal of captives, Sennacherib watching the proceedings from his throne, the royal entourage and the Assyrian military camp. Fire brands are shown in the siege scene, but it is clear that these are being hurled down by the inhabitants against their attackers – presumably to set fire to the enemy's war machines. The inscription accompanying the reliefs reads as follows (trans. Oppenheim 1977, 288): 'Sennacherib, king of the world, king of Assyria, sat upon a *nimedu*-throne and passed in review the booty (taken) from Lachish (*La-ki-su*).' It is the looting of the city, not a destruction, that is emphasised here. Contrast, for example, the epigraph to a scene showing Sennacherib's conquest of a Babylonian city (Russell 1999, 137): 'The city Bit-Kubatti I besieged, I captured, I carried off its booty, I burnt it.'

Sennacherib's grandson Assurbanipal recorded the capture of an Elamite capital in similar terms (Luckenbill 1927, 394): 'Hamanu, the royal city of Elam, I besieged, I captured, its spoil I carried off, I destroyed, I devastated, I burned (it) with fire.' A relief depicts the destruction of Hamanu: it shows flames on the battlements, Assyrian soldiers dismantling the fortifications with pickaxes and masonry and timbers falling from the walls of the city (see *Figure 1*). Nothing like these details appears in the Lachish reliefs though they were very extensive.

In the case of the Lachish reliefs, no flames or deliberate destruction with pickaxes can be seen in the surviving portions. Rainey's argument (1975, 53) that there would have been no room to depict the burning of the city is poor – there would surely have been space to (in 12 panels!), as well as mention it in the Annals. Taking the evidence at face value one is forced to take seriously the conclusion reached long ago by Dougherty (1930, 166) that 'the cities of Judah were not left in a condition of absolute ruin' by Sennacherib's campaign. Against this, Rainey (1975, 53) added the argument that 'it would be extremely naïve to suppose that Lachish escaped burning after such a terrific battle.' The word naïve more appropriately describes the assumption that every siege involving a 'terrific battle' must end with a conflagration. Unfortunately Ussishkin (1977, 30) offered the same argument: 'in view of the importance given by Sennacherib to the conquest of this city, it seems highly unlikely that the city was not razed to the ground after its conquest.' Such an argument presupposes knowledge of Sennacherib's motives – it could equally be argued that such an important prize as Lachish, of immense strategic and economic value, would *not* have been laid waste. Indeed, the cities captured from Hezekiah, far from being devastated, were given as rewards to his Philistine subjects (see above), the kings of Ashdod, Ekron, Gaza and Ashkelon. These kingdoms were being strengthened, not only to reduce the status of Judah, but also as a bulwark against the Egyptian threat to Assyrian power in the southern coastal plain. The Ethiopian commander Taharqo, commanding a large force of Egyptian and Nubian troops, had only just been repulsed at Eltekeh (north of Lachish), in the same year as the siege (Kitchen 1996, 383–386; van der Veen 2020, 144–153). The placing of fortresses such as Lachish (in a relatively intact condition) into the hands of loyal vassals would have made good strategic sense. Their destruction would have left a completely unprotected swathe of territory leading from the south into the central hill country, and would have tempted further Egyptian interference in the area.[5]

Without more detailed accounts, the idea that Sennacherib razed Lachish to the ground remains an unproven hypothesis. It cannot be emphasised too strongly that it is not supported by any direct historical evidence, nor by logic, and thus must not be used as a criterion for dating. There is therefore no reason to argue that an earlier stratum could not reflect the Assyrian attack because it shows insufficient signs of destruction and burning – making Ussishkin's rejection of Level IV as the candidate for the city seized in 701 BC arbitrary. He noted that 'Level IV apparently came to a sudden end', with possible traces of fire in one building. One explanation, offered by Moshe Kochavi, was that an earthquake may have been responsible (Ussishkin, 1977, 51–52). This was the famous earthquake known from the reign of king Uzziah (Amos 1:1; Zech. 14:5). In the final reports of the excavations (Barkay and Ussishkin 2004, 447) Gabriel Barkay and Ussishkin wrote, with a difference of emphasis, that: 'It is quite possible that this phase was destroyed by an earthquake rather than intentionally destroyed by human attackers, though no unequivocal proof of this is available'. Fantalkin and Finkelstein (2006, 22–23) argue against the idea that an earthquake *c.* 760 BC affected Lachish IV and other Judahite sites.

Ussishkin wisely restricted himself to stating that the Assyrian reliefs 'indicate that Lachish was . . . conquered by the Assyrian army'. In view of the above discussion, the evidence from Level IV would actually make a good match with a 'sudden end' of the kind we would expect from the Assyrian siege and capture of the city. The late

[5] Elsewhere (James 2017, 105–114; James and Bimson in prep.) I argue that it was by driving a corridor through the Shephelah, including the sack of cities such as Lachish (Level VI), that Ramesses III moved his troops northwards, aiming for the Jezreel valley and beyond.

Edward Lipiński (2006, 416), one of the most renowned scholars of the ancient Near East, arrived at the same conclusion. He notes:

> … the perfect correspondence between the destruction traces of Level IV, which did not end by fire, and the records of the Assyrian campaign against Judah and Lachish in 701 B.C.[6]

Bagg also refers to the siege ramp at Lachish, noting that while there are problems with its stratigraphy (as admitted by Ussishkin), that the excavated remains of the ramp fit 'its depiction in the [Sennacherib's] reliefs'. As Ussishkin (1978, 69) noted, Barnett (1958, 162–163) predicted from the Lachish Reliefs that remains of the Assyrian siege ramp might be found at the southwestern corner of the city. But the fact that the excavated ramp lies at this point, does not necessarily prove its connection with the Assyrian attack. Here, at the southwestern corner, was a saddle of land connecting the mound of Lachish to the surrounding hills, forming both the easiest approach to the city and, as called by Ussishkin (1978, 67) 'the Achilles heel in the defence system'. For tactical reasons, therefore, one would expect that more than one attack might have been launched on the city from this direction. The Babylonians in 587 BC may well have used siege ramps to cow the city, as they did at Jerusalem and other fortified towns in Judah. Indeed, Ussishkin (1980, 189) admitted that:

> Its attribution to the Assyrian campaign cannot be proved in the absence of proper stratigraphy, and is based on the assumption that the Assyrian attack was made on a grand scale and was extremely ferocious. Theoretically, the siege ramp could have been laid during the Babylonian attack of 588/6 B.C.E. which is historically and archaeologically attested.

Hence the appeal to the evidence of the ramp, as made by Bagg, is very uncertain. Though of course it is possible that it was originally built to subdue Lachish III and then was renewed at a later period, for instance at the time of the Babylonian conquest (similarly van der Veen 2020, 103).

Regarding the date of Lachish III, I long ago abandoned the idea that it fell to the Babylonian conqueror Nebuchadrezzar in 587 BC[7] – as previously argued in James *et al.* (1991, 176–179). Although Nebuchadrezzar attacked Lachish (see Jer. 34:7), I now feel that Lachish III was destroyed in the mid-7th century BC. A date several decades later than 701 BC, as accepted by Bagg, seems eminently possible from the archaeology. Van der Veen has demonstrated (from numerous finds of Late Assyrian and Late Assyrian-style objects such as sealstones) that whenever Lachish III fell, its 'horizon' of pottery including the *lmlk* stamped jars was still current in the mid-7th century BC (see likewise van der Veen's article in this volume, pp. 291–310).[8]

To review other possible candidates for the demolishers of Lachish III there are of course the Arabs that Bagg invokes in the region of Beersheba. Alexander Fantalkin also referred to Arabs in his BICANE 4 presentation (unfortunately not submitted for publication) though he also referred to the wars of neighbouring chieftains and the Assyrians quelling rebellion as possible causes of destructions in the southwestern part of the country. This was the time when the Neo-Assyrian empire began its slow fragmentation. In 651 BC Šamaš-šum-ukin, king of Babylon, rebelled against his brother and overlord Assurbanipal of Assyria, starting a protracted civil war. Encouraged by Šamaš-šum-ukin, rebellions followed throughout the empire, including the Levant. As Rainey noted, Assurbanipal complained that Arabian tribes had been raiding and plundering all the provinces of 'Amurru' (i.e. the Levant) during the civil war,[9] while we should also note the Scythian hordes, who swept through western Asia around 630 BC.

At the same time there seem to have been rebellions against Assyria. During the reigns of Esarhaddon and Assurbanipal the Assyrians transplanted further populations (from Syria but mainly Babylonia) into the province of Samaria (Samerina), as they had done in the 8th century BC[10] – presumably to replace rebellious Israelites whom they had removed. They added to the mix of the future 'Samaritans', who were later despised by the Judaeans as not being ethnically Judaean as well as for doctrinal reasons. (Hence the significance of the parable of the 'Good Samaritan'; Luke 10:30–37.) The transplants seem to have affected the more northerly Assyrian province of *Magiddu* (including the cities of Megiddo and Hazor). On the *CoD* model archaeological

[6] Lipiński (2006) also dated the fall of Lachish III to an attack of Nebuchadrezzar in 597/587 BC, as in the old low chronology for this stratum. But it is clear from the differences in the pottery of Levels III and II that there must have been some chronological distance between the two (see Zimhoni 1990).

[7] And with the advice of Pieter van der Veen.

[8] In 2003 Peter James and Nikos Kokkinos gave a paper ('Dating Lachish III: The Problem of the *lmlk* stamps', unpublished) – arguing for a seventh-century date for these royal jars – at a German conference (organised by van der Veen) in Löwenstein, southern Germany. (For some published detail, see James 2007, 215.) The case has been developed much further by van der Veen in various publications.

[9] Rainey (1993, 158–159). Arabs succeeded in making increasing footholds in Edom/southern Judah (later called Idumaea) from this period onwards (see Kokkinos 1998, 40–42, 44–45).

[10] Na'aman and Zadok (2000, 159–188). These transplants of population into Samaria by Esarhaddon and Assurbanipal (the 'great and noble Asnapper') were remembered in Ezra 4:2 and 4:10 (see James 2008, 142). See also Na'aman (1993, 116–117), where it is argued that Babylonians were also settled in Gezer in the northern Shephelah around this time (also Becking 1981–1982, 76–89).

evidence of these deportees is found in Stratum V at Hazor, in the form of Babylonian-style pottery, burial-jars and distinctive bathtub burials. In its second phase, VA, the fortifications were reorganised and strengthened, likely reflecting Assyrian reorganisation after the revolts of the 650s/640s. The rebuilt city was itself covered by a thick layer of ashes, presumably the result of conflict during the continuing collapse of Assyrian authority.

A sack of Lachish by an unknown party during these troubled times is eminently plausible. Bagg dismissed van der Veen's tentative suggestion that the Assyrian suppression of Manasseh's rebellion may have been the time of the destruction of Lachish III. This is too casual. It should be self evident, despite the paucity of contemporary records from the late Assyrian Empire, that they would have sent forces west in order to sustain their disintegrating empire. While it may be easy to dismiss the story of Manasseh's capture, deportation to Babylon and eventual release because it was first recorded in the late work of the Chronicler (2 Chron. 33:11), this overlooks the wider context as spelt out here – as well as the archaeological case as discussed by van der Veen (2020, 16–17, 171–172) which, on a lowered dating, attributes fortification work and the construction of newly-founded Judaean settlements to Manasseh, arguably against the threat of Assyrian attack (van der Veen 2020, 129–144, 176–178).

By process of elimination, if the siege ramp dates to the time of Lachish III, Arabs and Scythians can probably be ruled out as candidates for building it. Essentially nomadic, these peoples are unlikely to have been familiar with siegecraft technology. This would leave the Assyrians as likely candidates, but we should not forget here the Egyptians. By 656 BC Egypt had achieved unification and quasi-autonomy under Psamtik I, who was eventually to become the master of Syria and Judah and the main ally and supporter of his erstwhile Assyrian masters. A passage in Herodotus (2.157.1) states that 'Psammetichus ruled Egypt for fifty-four years; for twenty-nine of these he sat before Azotus, a great city in Syria, and besieged it till he took it. Azotus [Ashdod] held out against a siege longer than any city of which I have heard.' The alleged length of the siege is rather incredible, and it has been suggested that this may have originally been intended to refer to his 29th regnal year, i.e. 635 BC (see Spalinger 1978, 50). Lachish, of course, is not far from Ashdod and may have been the next step of Psamtik's takeover of the Levant. With regard to Judah, the name of Manasseh's son Amon strongly suggests an alliance with Egypt.

In conclusion, the literary evidence at our disposal does not inform us that Sennacherib destroyed the city of Lachish: in fact the very opposite is true. Ergo, the linking of 701 BC with the destruction and burning of Lachish III should be rejected. The latter can be sought instead in the troubled events of the mid-7th century BC, when the Assyrian empire was being threatened and rebellions throughout the region (as well as Egypt and Elam) had to be suppressed.

The dates for Lachish Level II will consequently need to be lowered as well and I hope to discuss the complications of this elsewhere.

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* [Eds This reply was completed not long before the author's untimely death.]

SESSION 3

Scientific Dating Methods

Introduction to Session 3 – 'Assyria and the West' in the Light of Scientific Dating

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The significance of employing scientific methods in Ancient Near Eastern archaeology is steadily growing. Of particular importance are those scientific studies that relate to the chronology of the ancient Levant, Egypt, the Mediterranean world and regions further afield such as Mesopotamia and Elam. This session contains three contributions, each one dealing with a different field of research, 14C, astronomical dating and DNA analysis.

As a follow up article to their previous paper (Zerbst and van der Veen 2015), Uwe Zerbst and Pieter van der Veen discuss current aspects of radiocarbon dating related to more recent developments in the field and how these relate to their previous conclusions. The increasing relevance of radiocarbon dating becomes even more important, as the problems presented by the conventional dates arrived at, based on pottery-related stratigraphy, still persist. Also, the overall refinement of the method (with consistent progress being made in the field) now allows for a better combination of 'accuracy and certainty'. The future potential of this development could eventually yield superior results, making the method more significant to archaeologists.

Moreover, the introduction of Bayesian statistics to the calibration of raw dates of sample series during recent decades, already had improved the accuracy of the results. Consequently, the smaller scatter bands already began to yield better results, even for the Bronze and Iron Ages. This has an important bearing on the ongoing discussions between proponents of the 'Low Chronology' and those still adhering to the traditional higher dates for Iron Age I and IIA. Even so, these improved results still suffer from an old problem with 14C, that the apparent elimination of outliers continues to be based mainly on mathematical-statistical grounds rather than on historical considerations. To put it bluntly: an improved precision is achieved but at the expense of accuracy. That is, while the results become more reproduceable, we can be less certain that they are also correct.

This problem may however be resolved, at least to a fair extent by the application of micro-archaeology, a method that has recently been developed by the Weizmann Institute, which aims at (among other aspects) avoiding outliers as much as possible, by better controlling the samples from the very outset. This is achieved by an interactive combination of sampling in the field, radiocarbon dating and calibration of raw data, i.e. by assuring a closer cooperation between archaeologists, physicists and chemists. After obtaining initial samples, these are dated and transformed into calendar ages, the conclusions of which become the basis for further sampling implemented in the field. If, for example, a sample series shows up in a horizontal or strongly wiggled section of the calibration curve, the data set can be extended by adding new suitable samples for calibration. Subsequently, the individual samples are examined more closely by separating those that contain inorganic carbon. According to the authors, the combination of Bayesian statistics and micro-archaeology holds the potential of achieving more accurate radiocarbon results without compromising certainty. Unfortunately, so far only a comparatively small number of high-quality date sets have been published.

While their previous queries have been widely confirmed by more recent studies, Zerbst and van der Veen show that the overall discrepancy between inflated radiocarbon dates and lower historical ages continues to widen the further back one moves in time. Some scholars within the scientific community have therefore called for a major revision of chronology, this time based on radiocarbon dating. The authors however warn that such a development must not be taken lightly. Rather, a thorough review of the entire dating chain, including the calibration curve, must be examined before such a revision takes hold. While scientists prefer to use their higher dates for Tell el-Dab'a to raise the dates for the Egyptian Middle and New Kingdoms, Zerbst and van der Veen consider such a decision to be unfounded. Moreover, they also highlight discrepancies around 600 BC, i.e. at a time when the traditional historical dates are undisputed. However, as radiocarbon is not usually employed to test these later times, the current database is still too small to get an overall picture of what is going on. Further investigation into these younger periods is therefore in order.

Secondly, in his article David Lappin provides a detailed and technical discussion of astronomical dates in the context of combined Venus-Moon observations relating to the so-called Ammiṣaduqa tablets from the First Dynasty of Babylon. By expanding the 'search window' for potential solutions, he obtains results that are consistent with an alternative lower chronology. In addition to his previous publications, the author includes further astronomical observations, such as solar and lunar eclipses referred to in ancient records. Moreover, Lappin's article deals with the astronomical determination of the date of king Ammiṣaduqa's first regnal year, which is crucial as an anchor point for establishing dates for Mesopotamian chronology, before and after the 'Dark Age' that followed the First Dynasty of Babylon.

The dates are based on so-called Venus-solutions found in the *Enuma Anu Enlil* tablets, which mention the 'disappearance' and 'reappearance' of the planet Venus over a period of some 21 years. However, as these cycles are repeated regularly every 56 or 64 years, multiple results can be obtained. Additional information is therefore necessary to narrow the search window. This is done with the help of lunar month' lengths, which are reported in omen inscriptions. Depending on the position of the moon relative to the earth and the earth relative to the sun, a lunar month can consist of 29 or 30 days. As with the movement of Venus, the month lengths recorded in the documents can also be retrocalculated with the help of modern astronomy. However, complete agreement is not to be expected. This is due to observation problems, when for instance the faintly visible new moon was only seen a day later than its actual occurrence, due to unfavourable weather conditions, or to transcription errors made by the ancient scribes. Other factors can also produce errors, such as the slowing of the Earth's rotation over time, due to tidal friction and other factors. For the age of Ammiṣaduqa, this amounts to a difference of some 10 to 15 hours. Its value is based on extrapolation for the period before *c.* 600 BC, an extrapolation which can only be reached within a margin of error. Occasionally this may contribute to a 'shift' of the new moon by a single day. Sometimes leap months were added to the calendar by the ancients, probably by decision of the king and priesthood, to keep the calendar roughly in line with the solar year.

Finally, the dating of Ammiṣaduqa is based on a combination of 'Venus solutions' with correct records of lunar month-lengths, or at least lunar solutions that receive the best set of 'hits' in the records as related to astronomical retrocalculation. Because of this, three 'solutions' have emerged in the conventional scheme referred to as 'high', 'middle' and 'low' chronologies. Usually, the middle chronology with 1646 BC for the first year of Ammiṣaduqa is preferred, based on historical considerations, even if statistically seen it does not provide the best results.

By expanding his search window, Lappin obtains a statistically better solution, namely that of 1483 BC, which fits a lower chronology much in line with the dates proposed by some authors in this volume. Moreover, Lappin both discusses the strengths and weaknesses of his solution. As the entire calendar reaching back to Early Dynastic and Akkadian times is linked to 'Ammiṣaduqa's Venus solution', it is so important that additional evidence be added. And this is precisely what Lappin does, namely by including solar and lunar eclipses available in the ancient records.

Finally, Nick Thorpe discusses the use of DNA in the field of archaeology, a subject that has received little attention so far. The reason for this is likely related to the fact that the results have been mainly published in journals focusing on genetics. In his article, Thorpe introduces us to the field of DNA and lists examples of research relating to various Bronze and Iron Ages sites. Furthermore, he discusses the strengths and weaknesses of this research for the field of archaeology. Concerning ancient migration movements, he further lists evidence from the Aegean, Egypt, the Levant and other places. While DNA studies allow conclusions to be drawn about the origins and migration of ethnic groups, they do not necessarily provide chronological information, specifying the time and sequence of these events. This disadvantage is overcome when ancient DNA from archaeological excavations is analysed. That this is possible was already demonstrated some forty years ago. Since then, the methodology has greatly improved, and a considerable data base has been generated. Even so, the distribution of this data is still not entirely satisfactory, regionally and chronologically. Thorpe therefore urges caution regarding any far-reaching conclusions.

As an example, Thorpe cites DNA studies for the early Neolithic period in Central Europe. While previous scholars have held that native hunter-gatherers had been almost completely replaced by settlers from the Middle East via the Balkans, more recent investigations, for instance of cave graves, have revealed a markedly different picture. But as it now seems, the apparently missing native population continued to exist alongside the new arrivals for another thousand years. The misleading result was simply due to the fact that a significant part of the Early Neolithic population had escaped sampling because investigations had concentrated on the population that had been buried in official cemeteries.

Concerning the Middle East, DNA analyses have shown that the coastal region of the Levant unsurprisingly exhibited a greater genomic variability than did the interior zones. The reason for this is not larger

migratory movements, but simply the high mobility of individuals. The result is an 'outlier rate' of the DNA findings of at least 7 to 10%, which must be considered as a significant threshold for statistical statements on the migration of larger groups.

Of particular interest to the topic of this volume is the influx of 'European DNA' as found at the city of Ashkelon at the transition from the Late Bronze to Iron Age. However, this situation was not to remain stable, when its citizens were more permanently replaced by a largely 'Levantine type' population. The samples taken derive from three Late Bronze Age, four Iron Age I and three Iron Age II burials. As for the settlement of the Sea Peoples, Thorpe calls for further investigation based on a broader statistical basis before any far-reaching conclusions can be drawn.

As for the use of DNA analyses in conjunction with radiocarbon dates, Thorpe refers to the dating of two skeletons from Chalcolithic Great Britain, which proved to be three and a half centuries (i.e. statistical mean, at a significance level of 95%, at least some 140 years) apart. However, DNA analysis revealed that the skeletons belonged to a father and his daughter. The re-dating of the older skeleton then revealed a significantly later date at death.

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Open Questions Regarding the Reliability of ^{14}C Dating in the Context of Ancient Near Eastern Chronology

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Since the introduction of Bayesian statistics and microarchaeological sampling, radiocarbon (^{14}C) dating has made considerable progress in recent years. For the first time ever, its results achieve an accuracy that makes it seem realistic to use them as primary data in archaeological research. On the other hand, the comparison between conventional archaeological ages and ages based on ^{14}C still reveals significant discrepancies, at least for the Bronze and Iron Ages, the periods that are of special interest to us here. Until it is universally agreed that satisfactory explanations can be found to solve these issues, there is no justifiable reason why we should replace conventional archaeological dating by the results obtained through ^{14}C. In this article we shall provide an overview of the current situation and propose a more radical evaluation of ^{14}C over and against undisputed historical-archaeological ages. Only if the necessity of this approach is sufficiently appreciated, can significant progress be made in the field.

1. Only an Apparent Contradiction? A Long-Standing Enigma

In Zerbst and van der Veen (2015) the authors drew provisional conclusions from a comparison between the conventional archaeological approach and radiocarbon dating, whose results had only recently become available through Bayesian statistics in ^{14}C calibration. The issue at stake had aroused a heated debate for more than half a century over the chronological implications of both methods. In 1971, Derricourt proposed a ^{14}C based chronology for Egypt and North Africa that was intended to correct historical chronology. A similar proposal was made a few years later by Mellaart (1979). At that time, it was still generally held that ^{14}C yielded significantly older ages when compared with conventional archaeological dates. While the work of both scholars was not well received by the archaeological community (Kemp 1980), Mallowan (1971, 242) argued in the third edition of *The Cambridge Ancient History*, that if further progress was to be made, ^{14}C could eventually shake the foundations of Egyptian chronology. Even so, most historians remained sceptical.

It was generally held that ^{14}C was simply too imprecise to provide useful information for Ancient Near Eastern chronology. Shaw (1985) noted that this would be true, even if the high-precision calibration curves, introduced during the 1980s, were included. At the utmost ^{14}C could shed some further light on the already accepted timeframe.

Major progress was however made when so-called 'short-lived samples' were being analysed, including cereal grains, twigs and nutshells, an analysis which only became possible when mass spectrometry (AMS) began to be applied in the late 1970's to early 1980's. Only by doing so, 'wrong' results could be avoided, that had hitherto been caused by the so-called 'old wood effect' (i.e. the sample being significantly older than the datable event). However, the situation did not improve much until 1987 when two major studies were published. In the first publication Hassan and Robinson (1987) employed statistical work carried out by Clark (1978) and Shaw (1985) while applying it to the existing Egyptian ^{14}C dates in a more rigorous way by discarding the material that had thus far provided unreliable results. The remaining data were 'cleansed' by using statistical outlier tests. The aim of the study was to prove that previously observed discrepancies between archaeological and ^{14}C ages were merely caused by the misinterpretation of the results, which now would prove irrelevant if objective sampling were to be applied. The approach was welcomed by scientists like Damon (1987) and Wendorf (1992), who hailed it as the long-awaited methodology.

However, at the same time, Hass *et al.* (1987) evaluated 64 samples from ten pyramids and other monuments in Egypt, among others from Giza and Sakkara. Their samples included different types of material (including wood, charcoal, straw and grass found in mortar and brick). As a result, the calibrated ^{14}C ages exceeded the archaeological ages by no less than 294 to 414 years. Eventually, in 2001, a study by Bonani *et al.* gained further attention confirming these results for the time of the Old and Middle Kingdoms. Hence, by the turn of the millennium, it was once again agreed that ^{14}C yielded ages that were several centuries higher than the standard historical chronology of the third and early second millennium BC.

Ten years later, finally a dramatic change happened when multiple studies suddenly proved major agreement between archaeological and radiocarbon ages for pharaonic Egypt. By this time, Bayesian statistics played an important part in ^{14}C calibration, allowing a substantial reduction of the remaining scatter band. The principle of the new approach will be briefly dealt with under point 2. An in-depth discussion of these studies can however be found in Zerbst and van der Veen (2015) and must therefore not be repeated here. The mention of two of these studies will suffice: Dee *et al.* (2009) re-evaluated the Bonani data of the 4th Dynasty that had been presented back in 2001. While applying Bayesian statistics (however with 'extensive use' of outlier rejections), their work revealed results that were radically different, as ^{14}C and historical data now proved to be in wide agreement. Bronk Ramsey *et al.* (2010b) too came to the same conclusion by evaluating museum artefacts. Their work included evidence covering a much longer period, stretching from the Egyptian 3rd to 20th Dynasties.

Even so, their work was still met with opposition. In 2012, Kutschera *et al.* published an in-depth study on evidence from Middle and New Kingdom Tell el-Daba (*Figure 1*). They revealed a consistent discrepancy of some 120 years on average. Before we will deal with their work under point 3, it will be necessary to briefly clarify the basics of the determination and calibration of ^{14}C data.

2. Recent Progress Made in Radiocarbon Dating

2.1 The basic principles

The basic principles of radiocarbon (^{14}C) dating do not need to be explained in detail here, as it has been adequately done elsewhere (see for instance Aitken 1990; Bowman 1990 and Hajdas *et al.* 2021). A brief summary will therefore suffice.

Radioactive ^{14}C is continuously formed in the upper atmosphere (i.e. more precisely so in the lower and upper troposphere), where cosmic radiation hits nitrogen atoms $^{14}_{7}N$. It is created by the reaction with neutrons under the emission of a proton $^{1}_{1}p$:

$$^{1}_{0}n + {}^{14}_{7}N \rightarrow {}^{14}_{6}C + {}^{1}_{1}p \qquad (1)$$

^{14}C combines with oxygen in the air and turns into $^{14}CO_2$ after which it enters the global metabolic cycle via photosynthesis in plants. Since it is radioactive, it subsequently decays, emitting β^--radiation (electrons) to form stable ^{14}N:

$$^{14}_{6}C \rightarrow {}^{14}_{7}N + \beta^- + \bar{\upsilon} + Q \qquad (2)$$

($\bar{\upsilon}$ – antineutrino; Q – decay energy)

Atmospheric nuclear tests in the 1960s have shown that global mixing of the atmosphere with ^{14}C lasts for about two or three years. We can, therefore, assume a

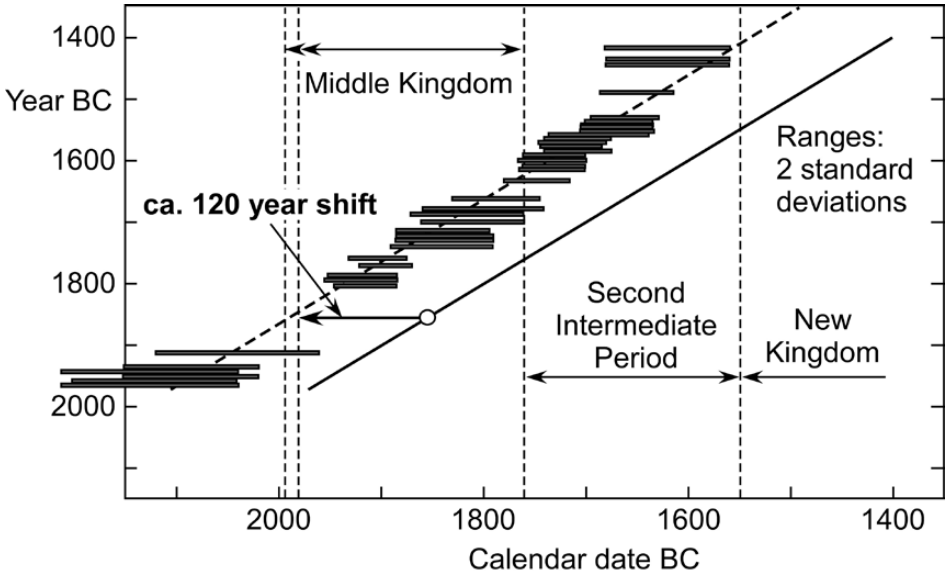

Figure 1. Comparison between archaeological and ^{14}C ages from Tell el-Daba, according to Kutschera et al. *(2012). The introduction of the more recent calibration curve (IntCal20, REF) shifts the dates to somewhat younger ages, but its results are still not sufficient for solving the overall discrepancy discussed in this article.*

global ^{14}C equilibrium, comprising of a globally uniform atmospheric $^{14}C/^{12}C$ ratio. This consists in 1.2 to 10^{12}, i.e. 10^{12} stable ^{12}C atoms compared to 1.2 ^{14}C atoms. Today we know that the ratio $^{14}C/^{12}C$ is different in the northern and southern hemispheres. Separate equilibria exist in the oceans, where isotope mixing is slower than in the atmosphere. So-called reservoir effects also occur in places where the equilibrium is altered by the permanent local influx of ^{12}C-rich carbon (e.g. in rivers or lakes, on calcareous bedrock or very close to volcanic vents), or ^{12}C-poor carbon if ^{14}C is formed *in situ* (e.g. in the vicinity of radioactive ore veins).

Radioactive ^{14}C decays with a half-life of $t_H = 5700 \pm 30$ years, i.e. after *c.* 5700 years the mass of ^{14}C atoms is halved, unless the isotope equilibrium is maintained by renewed supply of ^{14}C (Kutschera 2019). The latter happens throughout the biosphere due to food chains. However, if an organism dies, it merges from the active into the passive carbon reservoir, as no more carbon exchange occurs, i.e. no new ^{14}C is supplied and the $^{14}C/^{12}C$ ratio continually decreases according to the decay law. Because of this information and our knowledge of the initial and present $^{14}C/^{12}C$ ratio, the age, i.e. the time lapse since the death of the organism, can be determined.

The first scholar to employ this principle was the American chemist Willard Libby, who was awarded the Nobel Prize for chemistry in 1960. Early tests on archaeological objects of known ages showed that the method was indeed applicable. Later, however, with the first use of tree-ring curves for comparison, it became apparent that the measured ^{14}C ages required additional corrections to provide more reliable historical ages. The reason for this was identified as $^{14}C/^{12}C$ ratio fluctuations that occur over time, which is why the current isotope ratio cannot simply be applied to the past. Potential causes for the fluctuations are changes in cosmic radiation (for instance caused by the solar cycle), changes in the earth's magnetic field or drastic climate changes, for instance during the ice ages, when the global biomass, i.e. the ^{12}C volume of the active carbon reservoir, was reduced.

According to the current state-of-the-art methodology, radiocarbon dating comprises of the following steps:

(a) Sampling in the field

As mentioned before, the introduction of mass spectrometry (AMS) during the late 1970s made it possible to analyse so-called short-lived samples including cereal grains, grass, straw, etc. These likely relate more specifically to the archaeological strata in which they are found and to possible events that accompanied their destruction, if of course such information is known. But in the case of wood or charcoal, the dates may turn out to be significantly older than the archaeological horizon, especially if the tree had been re-used in the building in which it was found. An additional problem with long-lived trees such as cedars is that the inner rings can be considerably older than the outer ones. Accordingly, wood samples only provide a *terminus ante quem*. Today different materials are tested to combine the resulting information. The dating of sample series including additional information will be discussed below.

(b) Preparation of the samples

This includes the (mechanical) cleaning of the samples as well as the chemical extraction of the datable components (for instance cellulose for wood or collagen for bones). It is crucial that any extraneous carbon, for instance from the surrounding sediment introduced by microbiotic activity or other mechanisms, is removed. The sample is then prepared by graphitization for the accelerator, where it, converted into a gaseous state, serves as the cathode.

(c) Determination of the *preliminary* ^{14}C age (or 'Libby age')

What is determined in the accelerator is the $^{14}C/^{12}C$ ratio. To that purpose the carbon atoms emitted by the cathode are accelerated in a magnetic field to a speed of several thousand kilometres per second. A further magnetic field forces them onto curved paths. Due to the different masses of the isotopes, the deflection of ^{12}C, ^{14}C and ^{13}C is found to be different. Finally, the separated isotopes are counted individually by different sensors.

With A_0 being the $^{14}C/^{12}C$ ratio at the point of death of the given specimen and A being the rate at measurement, age t of the sample can be determined by solving the equation:

$$A = A_0 \cdot e^{-\lambda t} \qquad (3)$$

Constant λ is the decay constant, which depends on the half-life t_H as

$$\lambda = \ln 2 / t_H \qquad (4)$$

This way a determined age is designated as '*preliminary*' because it is based on several simplifying assumptions, such as a deviated half-life (i.e. up to Libby's time) and today's isotope equilibrium. Additionally, a correction for isotopic fractionation is routinely applied. This is necessary because organisms tend to, to varying degrees, absorb a slightly lower proportion of ^{14}C than the surrounding air. As this is also true, although to a different extent, for isotope ^{13}C, knowledge concerning the quantity of the latter is applied to make corrections with respect to the fractionation effect. The ratio between the stable isotopes ^{13}C and ^{12}C is approximately 1:99.

The measurement inaccuracy of the preliminary ^{14}C age is described as mean value plus/minus one

or two standard deviations of a normal or Gaussian distribution. One standard deviation describes the range within which 68.27% of all measured values can be found, two standard deviations refer to a range of 95.45%. The estimation of the measurement error is based not only on individual data from the test facilities, but also on experience gained from international intercomparison exercises. Finally, it must be noted that the preliminary ^{14}C age is dated in years BP (Before Present), while 'before present' refers to 1950, i.e. the year when atmospheric nuclear tests began.

(d) Calibration of the *preliminary* ^{14}C ages for obtaining calendrical ages

The so-called 'calibration' of the ^{14}C data, i.e. the 'translation' of the raw data of step (c) into calendar ages, is based on tree ring data. The basic principle is rather simple: trees annually grow tree rings, which are easy to count along their radii to obtain the corresponding age. Reality, however, is more complicated, since a tree ring master curve must be determined from many timbers whose ring width sequences are joined together by cross-correlation. We will briefly address this issue under 3.6. The existing calibration curves are revised from time to time including new material. Whereas the early consensus high-precision curves were based on ^{14}C ages for 20-year sections, i.e. ^{14}C age was obtained with an average of over 20 tree rings, selected sections are currently based on annual rings only. Note that the updating of curves is rather a 'fine-tuning' exercise, as can be seen in *Figure 2*. To what extent further refinement of the curve will be due in the future remains an open question. What we do know is that the isotope equilibrium (and thus the determined ^{14}C age) produces seasonal fluctuations between early-wood and latewood. In a study on wood from late-medieval England McDonald *et al.* (2019) detected a mean difference of 26 ± 15 years (during periods of high ^{14}C production the difference proves to be even higher) between early- and latewood. This is probably partly due to the incorporation of stored carbohydrates into the earlywood, which can cause a seasonal difference of ten years. Consequently, the authors proposed to base future annual calibration curves on latewood only. Braziunas *et al.* (1995) reported differences of 10^{-3} in $\Delta(^{14}C/^{12}C)$ or 8 ^{14}C years between radiocarbon results obtained at different locations (Washington state, California, Ireland and Germany) compared to the consensus calibration curve which therefore should be added to the standard deviation of preliminary ^{14}C dates. The offset could yet be greater, however, if other regions were included (Stuiver and Braziunas, 1998).

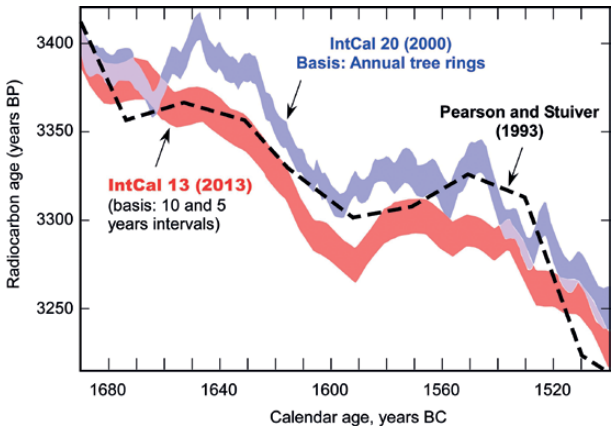

Figure 2. Consensus calibration curves for the time between 1690 and 1500 BC, according to Pearson (1993) and Pearson et al. *(2018). The results of the latter have found their way into the IntCal 20 curve of 2020.*

2.2 Problems in calibrating radiocarbon ages and strategies to be overcome

Figure 3 shows a section of the current calibration curve for the time between 1200–500 BC which, in archaeological terms, corresponds to the conventional Iron Age timeframe in the Levant. The scatter bands in the data points describe the ^{14}C measurement inaccuracy, which is assumed to be normal, or Gauss distributed. After about 750 BC the curve reaches a plateau superimposed by so-called 'wiggles'. This so-called 'Hallstatt plateau', which lasted from *c.* 800–400 BC and which is named after the simultaneous Hallstatt culture in central Europe at that time, poses a particular challenge for calibration, as is schematically explained in *Figure 4*, where the principle of calibration and related problems are illustrated.

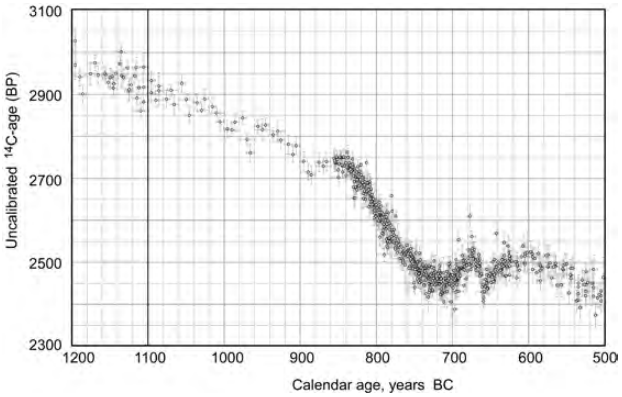

Figure 3. IntCal 20 curve between 1200 and 500 BC.

The result produced by the calibration depends on the section of the curve into which the measured ^{14}C age falls. If this is a steep one (blue), a well-defined result with little scattering becomes visible. However, if the data point falls into a rather flat curve section (red), multiple solutions usually occur, and the resulting scatter band is significantly larger than that of the

input data. Noticeably, the resulting distributions of the calibrated ages on the abscissa are normalised such that the cumulative overall probability, i.e. the summarised area of all solutions, is set to 1 or 100%.

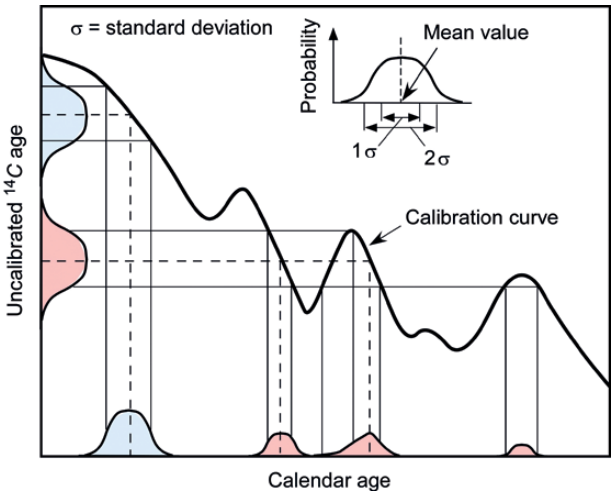

Figure 4. Calibration of 14C ages in a steep and a flat wiggled curve section.

One way to solve the multiple solutions problem, is to calibrate sample series instead of individual samples. Two techniques are possible: Wiggle matching (an example of which is shown in *Figure 5*) and Bayesian statistics. In both options, which can also be combined, the statistical analysis is complemented by the inclusion of further information.

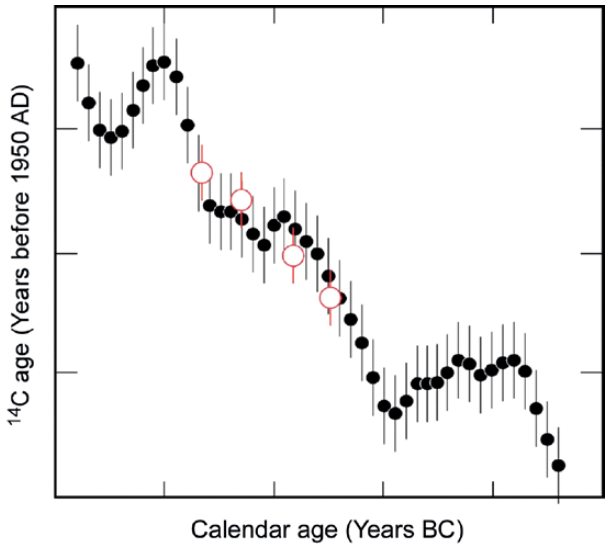

Figure 5. Example of 'wiggle matching'. The example is taken from Friedrich et al. *(2006) and shows the dating of an olive tree, uncovered* in situ *in the tephra of Santorini.*

For a comprehensible introduction to Bayesian statistics in 14C calibration, see Kutschera *et al.* (2007). *Figure 6* schematically explains the methodology with a simple example. Two samples are calibrated, both resulting in two-part solutions. If we assume that sample 1 is younger than sample 2 (information that can be derived for instance by stratigraphic evidence), any result showing sample 1 older than sample 2 logically appears to be incorrect and can, therefore, be omitted.

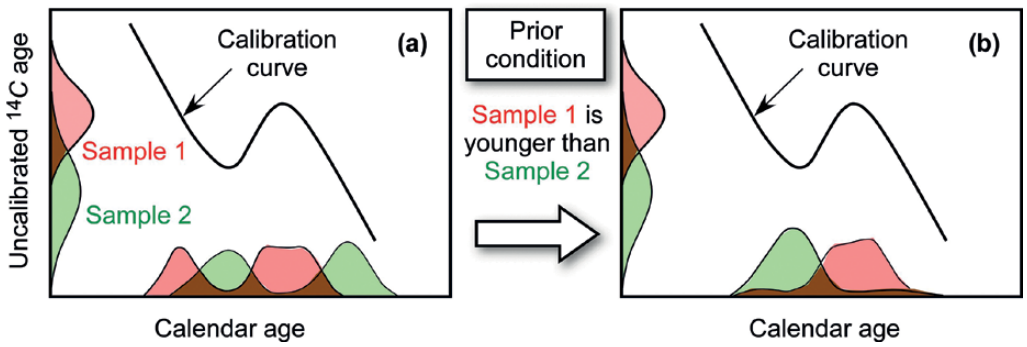

Figure 6. Application of Bayesian statistics to the calibration of two samples with overlapping preliminary 14C age distributions, after Kutschera et al. *(2007).*

Bayes' theorem describes the conditional probability of an event such that its occurrence is possible only when a certain condition is met. In radiocarbon calibration this condition is designated as 'prior'. Conditional probabilities (designated as 'posterior') are determined by multiplying the original 'likelihood probability' distribution with the 'prior' distribution ('as well as' operation in probability terms). In our example, this means that the probability of all solutions according to which sample 1 is younger than sample 2 is multiplied by a 'prior' probability of one and all opposing solutions by a priority probability of zero. In this way, the impossible solutions are eliminated due to our prior information. This is the simplest example.

When calibrating sample series, further information must be considered, however, for instance the (archaeologically known) time intervals between samples if they comprise more than one archaeological period. The entire statistical analysis is also more complex in that further aspects must be considered, for example the scatter found in the calibration curve. Regarding the prior information, a distinction is made between *informative* and *uninformative* prior, see Steier and Rom (2000), Steier *et al.* (2001), Bronk Ramsey (2000, 2009) and Weninger *et al.* (2010) and the discussion under 3.2 in Zerbst and van der Veen (2015). There is no need to discuss this in more depth here. The following description will suffice: The *informative* prior information is defined as information that is based on reliable archaeological information such as stratigraphy, pottery typology, etc. *Uninformative* prior information involves additional assumptions. For example, it may include the size of the sample series, for instance the number of samples, which is known to affect the result. It can be assumed that short-lived samples cluster towards the end of a phase and are exponentially distributed, etc. The *uninformative* prior is conceivable, but of course not verifiable, which is why there is always a subjective aspect involved.

Note, however, that beyond such detailed problems, the basic principle of Bayesian statistics based on ^{14}C calibration remains the same. Its main effect is a significantly reduced scatter band of calibrated results. In other words, we are faced with an improved accuracy.

2.3 Outlier: Elimination or prevention?

However, a principal problem arises when after calibration samples with (stratigraphically or historically) higher or lower ages, show opposite 'likelihood probability' effects of ^{14}C ages that cannot be eliminated by means of Bayesian statistics. In this case, at least part of the data must be rejected as outliers. In fact, software solutions such as OxCal offer this possibility (for a discussion see Bronk Ramsey 2009; Bronk Ramsey *et al.* 2010a; also under 3.3 in Zerbst and van der Veen 2015). There may be various reasons why a data point can be an outlier, such as an incorrect find context, insufficiently treated contamination of the sample, local reservoir effects of the isotope equilibrium, an old wood effect, errors in ^{14}C measurement. This is especially troubling, if this observation only becomes apparent during the calibration process (for if one had known this beforehand, one would not have included the data point in the analysis).

The question that arises is whether it is legitimate to eliminate outliers on a purely statistical basis. One might argue that this is justifiable if the number of problematic data points is limited (which appears not to have been the case in past studies), but even so this observation could still raise an eyebrow.

Even so, it is feasible to avoid outliers as much as possible during the sampling stage and sample preparation (for instance steps [a] and [b] under point 2.1). The keyword here is *microarchaeology*; on this see for instance Weiner (2010). This requires an extraordinarily careful sampling in the field, which needs close coordination between archaeologists and dating experts. Sampling and examination are carried out interactively, for the results of the preliminary evaluation of the first samples will have a direct influence on further sampling. The measures taken, are based on laboratory examinations of the samples and on the first preliminary ^{14}C dates. Better still, if feasible, samples should already be examined in the field. For example, microscopic examinations help specify the structure and composition of the samples (for instance under infrared light) while Fourier Transform Infrared Spectrometer (FTIR) examinations are used to distinguish between inorganic and organic carbon. This way specific portions can be separated in the sample. If not available, new samples must be taken. *Preliminary* ^{14}C dating helps to determine to which part of the calibration curve (steep or flat) the sample must be attributed. In case of an unfavourable result, a new sample must be taken. The results of the new sample then can shed light on the previous one. This procedure helps to generate data sets in which the likelihood distributions of all samples overlap as much as possible.

3. Once Again: Do We still Encounter Discrepancies?

3.1 Recent developments

Our discussion in Part 1 above ended with the ancient site of Tell el-Daba, whose Middle and New Kingdom occupation spans over half a millennium. On average, the comparison between ^{14}C and archaeological ages revealed a discrepancy of approximately 120 years (Zerbst and van der Veen 2015). This problem led the present authors to further investigate if evidence at

other Egyptian and Levantine sites would also confirm the same thrust. There is no need to repeat our arguments here, as these still hold true today.

It will suffice to provide a general discussion of more recent work. A more thorough discussion of all the recently published articles would go beyond the scope of these proceedings. Even so, these publications differ in many aspects, for instance concerning which calibration curve was used, the quality of the available sampling material, or which sampling strategy was applied.

3.2 The Egyptian Old Kingdom and the Early Bronze Age in the Levant and beyond

In our previous article on the subject (Zerbst and van der Veen 2015) we listed several studies that showed a striking discrepancy between ^{14}C and archaeological dates (see for instance Bruins and van der Plicht 2001; Regev *et al.* 2012a, 2012b). These publications suggested dates that were higher than the conventional historical dates by at least one century. Since then, the situation has largely remained unchanged.

Back in 2014, Höflmayer *et al.* published their work on Tell Fadous-Kfarabida (a site located in modern Lebanon). While the end of Early Bronze Age III is traditionally dated to about 2300 BC based on synchronisms with the end of the Old Kingdom in Egypt, their ^{14}C results yielded considerably higher dates. By employing Bayesian analysis, they applied three options with respect to their prior assumption: Option A: all samples were considered representative of the archaeological 'phase' in which they were found, while these were classified in the correct chronological order; Option B: a lacuna was assumed to have existed between phases IV (EB III) and V (EB IV); Option C: similar to option B, while the four oldest dates of phase V were excluded from the study. Their results are shown here in *Figure 7*. Depending on which option was applied, the peak values of the probability density yielded results that were between 170 and 240 years higher than the traditional dates. Comparable results were recently obtained by Quiles and Tristant (2023) and Quiles *et al.* (2023).

In their study on Early Bronze Tell Abu en-Ni'aj in the northern Jordan valley, Falconer and Fall (2016) likewise suggested that the end of EB III be raised by two or three centuries. Work on the subsequent EB IV period however did not yield straightforward results.

In 2021, Fall *et al.* conducted a study on EB IV at Tell Abu en-Ni'aj and Tel Hayat. Their study also yielded dates that were about 200 years higher than the conventional dates. Consequently, they raised the dates for EB IV to between *c.* 2500–2200 BC. The previously favoured conventional date had been *c.* 2300–2000 BC.

Lev *et al.* (2020) also date the end of EB III to *c.* 2500 BC at Khirbat El-'Alya, a site located in north-eastern Israel, and this applies in the same way to the investigations by Fall *et al.* (2022) in Khirbat Iskandar in central Jordan.

3.3 The Egyptian Middle and New Kingdom and the Middle Bronze Age

^{14}C dates for these periods have been assembled by several scholars. In 2013, Marcus published results on MB Tel Ifshar in the Sharon plain, while Höflmayer *et al.* (2016a) assembled ^{14}C dates for Tell el-Burak, a site located on the southern Lebanese coast. These include results for late MB I and/or the transition between MB I–II. *Figure 8* shows a summary of these results in comparison with contemporary dates at Tell el-Daba. The dates deviate from the traditional chronology by between 70 and 160 years, depending on which probability peak was evaluated.

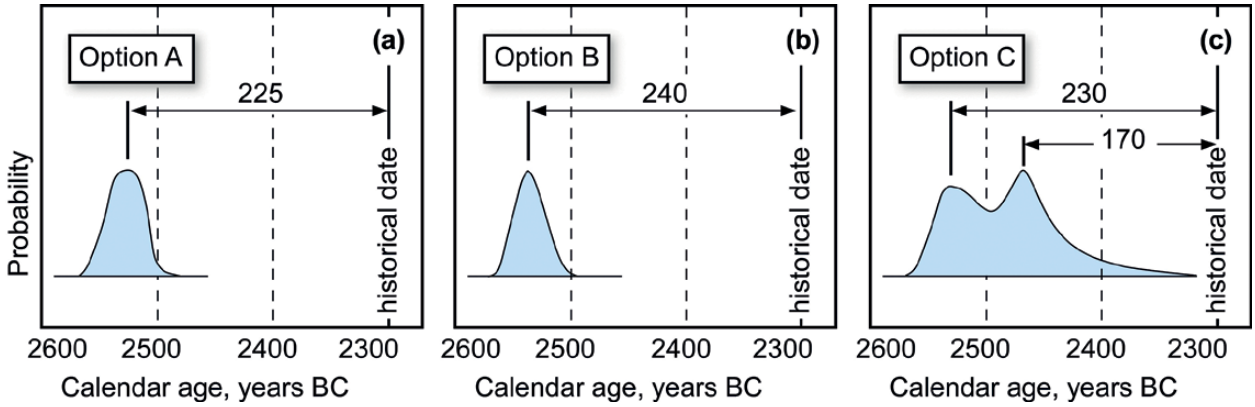

Figure 7. 14C dating of the end of EB III Fadous-Kfarabida (Lebanon). (a), (b) and (c) relate to three different prior assumptions; after Höflmayer et al. *(2014, simplified version). The numbers accompanying the arrows indicate by how many years these results deviate from the traditional archaeological/historical dates.*

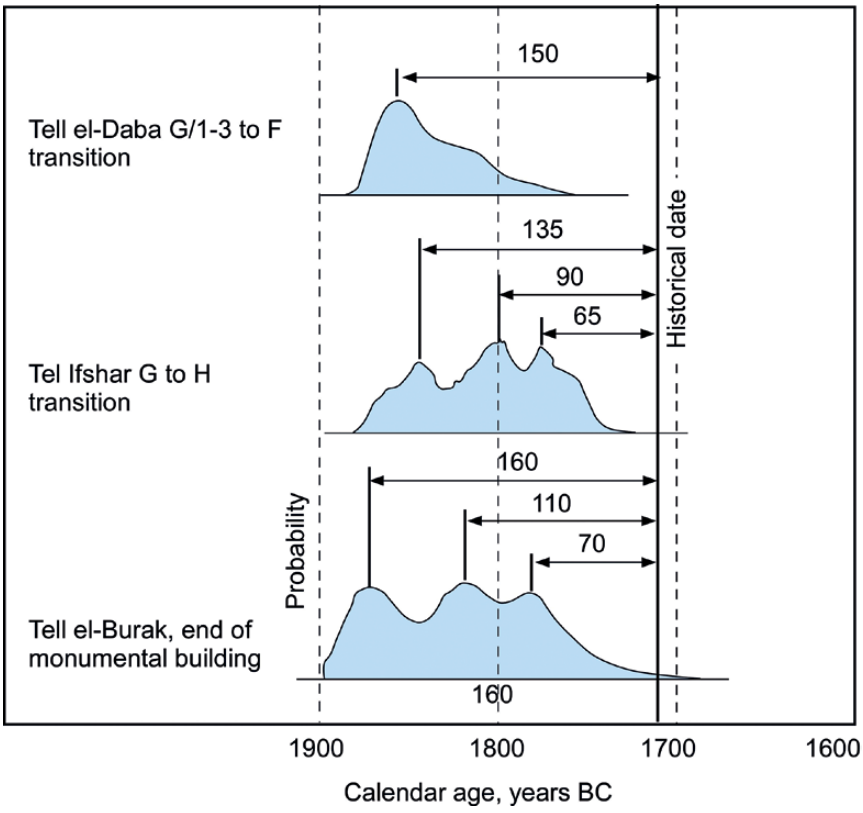

Figure 8. 14C dates of different authors for the end of the late MB I and/or the transition from MB I–II at Tell el-Daba (i.e. transition from Stratum G/1–3 to F), Tel Ifshar (i.e. the transition from Stratum G to H) and Tell el-Burak (the final stage of the monumental building). After Höflmayer et al. *(2016a, simplified version). The numbers accompanying the arrows show how many years these results deviate from the traditional archaeological/ historical dates.*

A 14C date for the MB II–III transition at the final phase III of Tel Kabri in the western Galilee was published by Höflmayer *et al.* (2016b). The authors report a discrepancy of some 50 years as compared with the traditional archaeological dates (or some 100 years in comparison with lower dates favoured by other scholars). The results are shown in *Figure 9*. Three different prior conditions were applied: Option A: All short-lived samples were assumed to represent the archaeological phases in which they were found, taking for granted that these phases were found in the correct order; B: Only samples from phase III were selected. They were assumed to be distributed exponentially towards the end of the phase; C: All samples with potentially inbuilt age (wood and charcoal) were eliminated.

Recently, Regev *et al.* (2021) investigated MB Jerusalem, based on *microarchaeological* data sampling. They date the MB I–II transition later than Höflmayer *et al.* Even so, their probability peak value at 1780 BC is still earlier than the previously favoured archaeological date of *c.* 1710 BC, while their work yielded a 50-year downward shift for the end of MB II. The results of their study are shown in *Figure 10* where also data published by other scholars have been incorporated. The bars in the upper part of the figure represent one standard deviation (with a probability range of 68.3%). Different colours are used to highlight the main archaeological phases of MB I (green), MB II (blue) and MB III (red).

Their study also highlighted another problem. Traditionally most scholars have represented the transitions between the archaeological periods largely as sharp cuts, as if those shifts occurred more or less simultaneously across the entire region. Yet, archaeologists have since long recognized that archaeological phases and sub-phases regionally overlapped by several decades, sometimes even by more than a century. For instance, the transition between EB III and EB IV could have

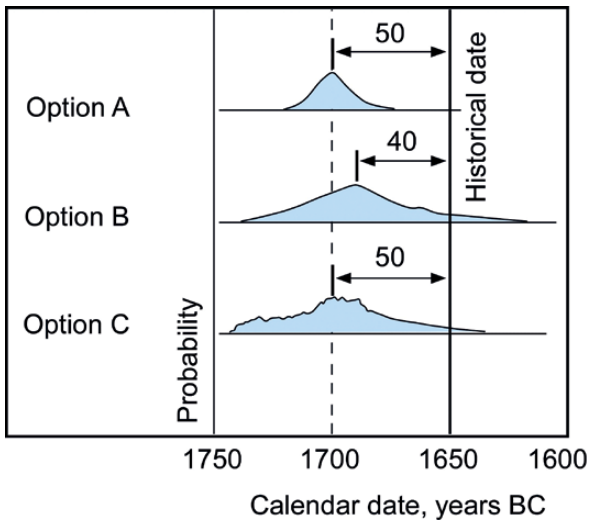

Figure 9. 14C date of the MB II–III transition at Tel Kabri. After Höflmayer et al. *(2016b, simplified version). The numbers accompanying the arrows indicate by how many years these results deviate from the traditional archaeological/historical dates; A, B and C belong to three different prior assumptions.*

lasted up to over a century, depending on how much the different regions had been affected by climate change and political upheavals (i.e. between *c.* 2300–2150 BC, see Weippert 1988; Chapman 2009; van der Veen and Zerbst 2022). Similar 'sloping horizons' are believed to have existed between EB IV and MB I (*c.* 2000–1950: Nigro 2007), MB I–II/MB II–III (*c.*1750/10–1680 BC: Bienkowski 1989; Ilan *et al.* 2019), MB III and LB IA (*c.* 1600/1550–1470/60 BC: Dever 1971; Kempinski 1983; Hoffmeier 1989; Bietak 1991), LB IB and LB IIA (*c.* 1420–1350 BC: Meitlis 2008, 2012; Leavitt 2022), LB IIB-late and IA IA-early (*c.* 1300–1150 BC: Meitlis 2008; Killebrew 2014; Zwickel and van der Veen 2017). Taken at face value, sloping horizons would have an impact on how we define the periods and on how long each phase lasted.

Naturally one would expect these overlaps to crop up in the ^{14}C results, which then would result in even greater discrepancies, aggravating the obstacles already noted between the dating methods.

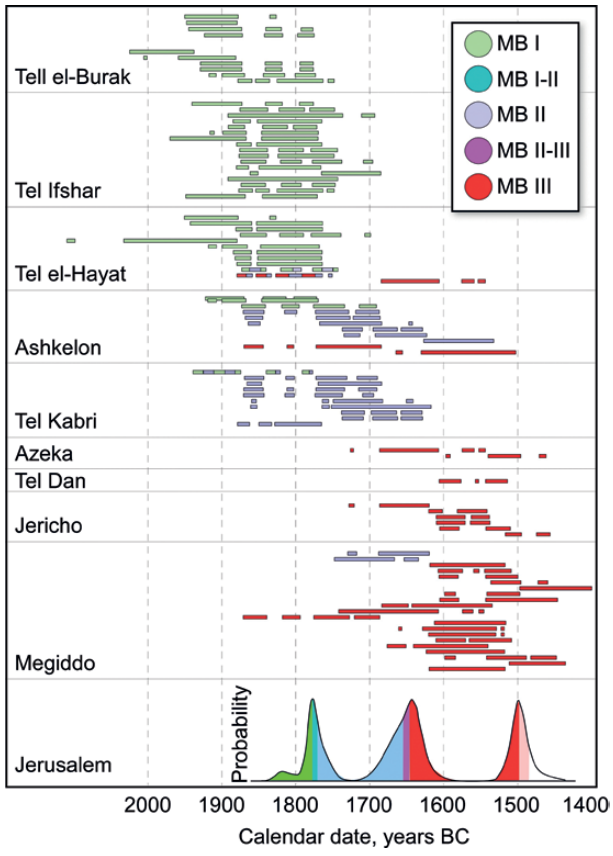

Figure 10. ^{14}C *dates of MB Jerusalem, according to Regev* et al. *(2021, simplified version). The bars in the upper part are borrowed from various publications (Marcus 2013; Höflmayr* et al. *2016b; Carmi and Ussishkin 2004; Toffolo* et al. *2014; Webster 2015; Falconer and Fall 2017; Martin* et al. *2020; Bruins and van der Plicht 2019).*

3.4 The Late Bronze Age and the Aegean World

The chronological problem in the Late Bronze Age Aegean is still closely associated with the volcanic eruption of Thera on the island of Santorini. This event too is historically related to Egypt through apparent synchronisms, especially via Tell el-Daba, an event that was formerly dated to between *c.* 1500 and 1475 BC. This date is also determined by pottery evidence from the Levant. In Zerbst and van der Veen (2015) the present authors cited a series of articles that were concerned with discrepancies of 100 + x years with ^{14}C, again yielding higher ages (Friedrich *et al.* 2006; Manning *et al.* 2006; Bruins *et al.* 2009; Höflmayer 2009). Since our publication, further studies have appeared in print. General summaries of the problem at stake can be found in McAneney and Baillie (2019) and Manning *et al.* (2020), see *Figure 11*. Recently, Lespez *et al.* (2021) investigated tsunami sediments on Crete, resulting from the Theran eruption. The radiocarbon range yielded dates between 1744 and 1544 BC. This time range, as well as the discrepancy between ^{14}C and the archaeological dates, confirm once again the earlier results, except that the scatter band has now widened. It should be noted that earlier attempts at dating the Theran eruption indirectly by Greenland ice-cores, have now been widely abandoned (McAneney and Baillie 2019).

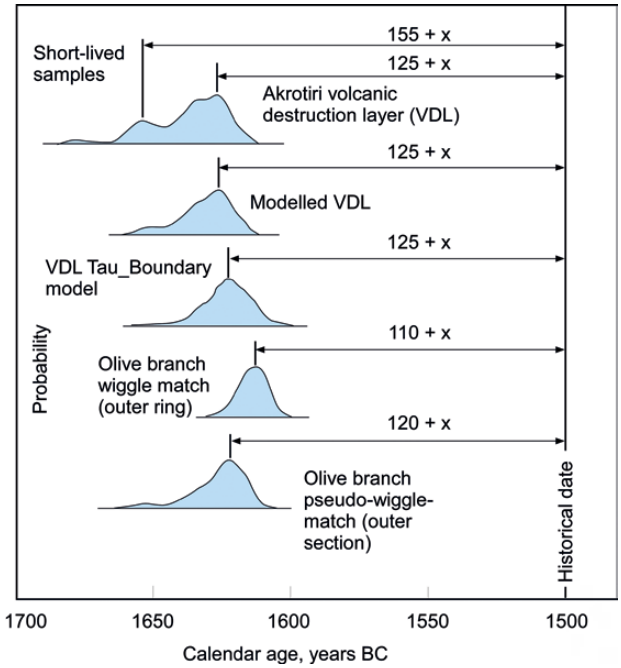

Figure 11. A simplified representation of the ^{14}C *dates of the Theran eruption; according to Manning* et al. *(2006, 2014), Friedrich* et al. *(2006) and Höflmayer (2012). The numbers on the arrows indicate by how many years these results deviate from the conventional archaeological dates.*

Moreover, a recent development within the field concerns the elaboration of the corresponding curve section, representing the calibration curve on an annual basis (Pearson *et al.* 2018). As can be seen in *Figure 12*, the former discrepancy could be reduced to some extent, but the overall problem has survived, showing that an even later ^{14}C date for the eruption can still not be entirely excluded (i.e. the small probability peak seen at the bottom right).

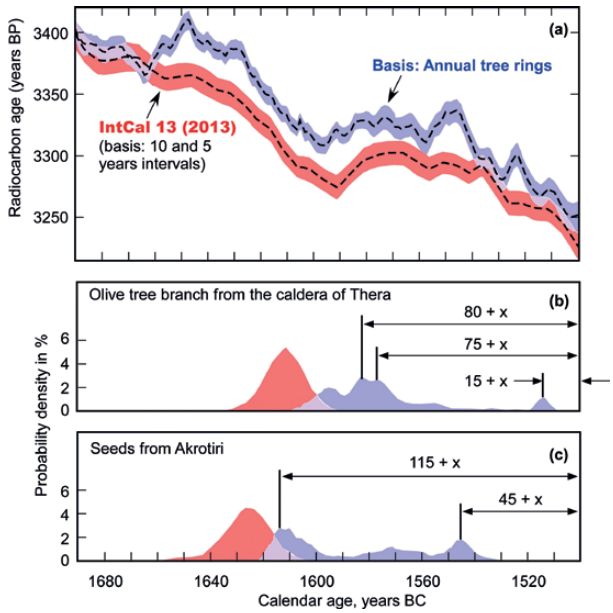

Figure 12. The effect of the new calibration curve on an annual basis for ^{14}C dating of the Theran event, according to Pearson et al. *(2018).*

As a result, a more critical issue stands out. In section 2.2 we addressed the problem of flat curve sections in the calibration curve. The Theran event, if ^{14}C dated, falls into just such a period. Hence the ^{14}C measurement accuracy of the samples as well as the calibration curve in general must be observed with great caution. For as Fantuzzi (2019) points out, a difference of only 20 ^{14}C years 'would be enough to shift the outcome of calibration from the low to the high chronology or vice versa'.

3.5 The Iron Age

A similar problem plagues the Iron Age, the second part of which coincides with the so-called Hallstatt Plateau (see *Figure 3*). Here, too, the accuracy of ^{14}C measurements must be cautioned. At the same time, the processing of the data using Bayesian statistics, including different uninformative prior information and the outlier option, offers some 'flexibility' that would not have existed if we had a steep curve. The present authors suspect that this may be one of the reasons why adherents of the conventional high and low Iron Age chronologies (yielding deviations of up to 80 years) continue to disagree. For proponents of both scenarios employ ^{14}C to substantiate their own work. Gilboa *et al.* (2009) appear to be correct, when they state that the results are 'heavily dependent on the statistical treatment of the data'. Given the great number of Iron Age studies, our discussion also here must be selective.

Concerning the early Iron Age (section 3.3) Toffolo *et al.* (2012) studied the transition from the Late Bronze to the Iron Age at Tel es-Safi/Gath. Conventionally, the transition is dated to about 1200 BC (or some 50 to 80 years later if Finkelstein's lower chronology is followed). ^{14}C investigation has been performed in Area A, where the excavators uncovered an apparent domestic floor of a small room which turned out to be the uppermost of four consecutive early Iron Age I layers. Therefore their floor could slightly postdate the actual LB/IA transition. Yet, as can be seen in *Figure 13*, the probability peaks of the oldest sample in the series predate the traditional 1200 BC-date by some 70 to 180 years.

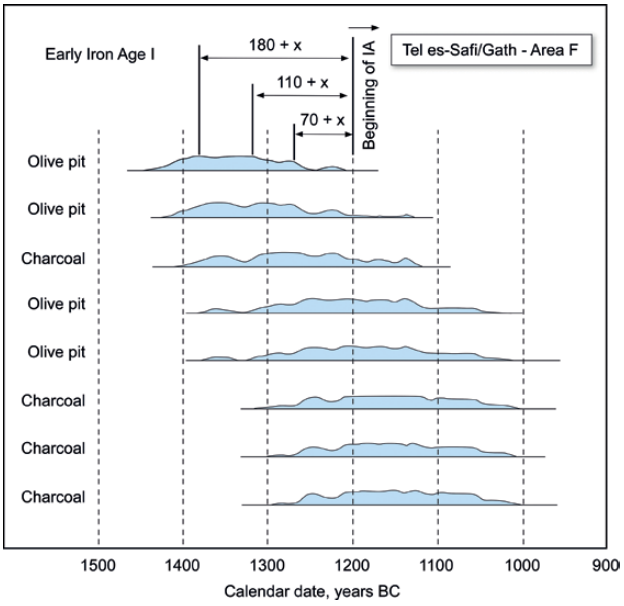

Figure 13. Simplified representation of the ^{14}C dates of early Iron Age I Tel es-Safi/Gath, according to Toffolo et al. *(2012). The numbers on the arrows indicate by how many years these results deviate from the traditional archaeological/historical dates.*

Asscher *et al.* (2015) also investigated Tel es-Safi/Gath and likewise obtained a ^{14}C date for the Late Bronze to Iron Age transition. While they admitted that the stratigraphic record below and above the transition is slightly elusive, they compensated this shortcoming by applying *microarchaeological* principles. As a result, they dated the appearance of 'Philistine pottery' to the 13th century with the highest probability peak at 1285 BC, see *Figure 14*. But this is 85 years earlier than

most scholars working with the conventional high chronology would be prepared to accept. As a matter of fact, the appearance of so-called 'Philistine' bichrome ware is generally taken to be the type-fossil defining the LB/IA transition. Due to the apparent absence of this pottery at sites where Egyptian 20th Dynasty occupation has been found, Finkelstein has lowered the beginning of IA I in his model to the second half of the 12th century BC. However, according to Asscher *et al.*, this transition would now rather need to be raised to the early years of the 19th Dynasty, at the beginning of the 13th century BC.

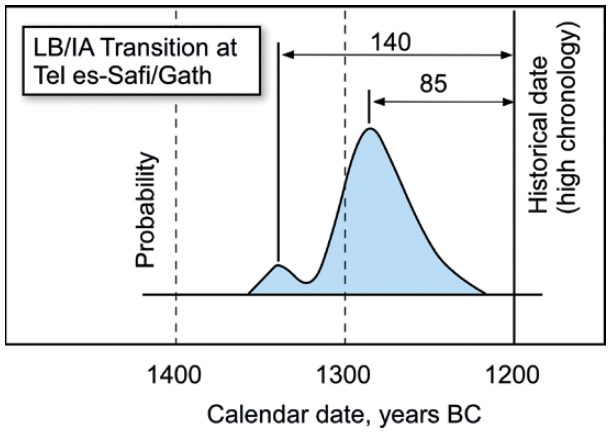

Figure 14. A simplified representation of 14C dates marking the Late Bronze to Iron Age transition at Tel es-Safi/Gath, according to Asscher et al. *(2015). The numbers on the arrows indicate by how many years these results deviate from the traditional archaeological/historical dates.*

In 2009, van der Plicht *et al.* published a paper in which they compared 14C dates with archaeological data of the Iron Age in the Eastern Mediterranean, including regions in Cis- and Transjordan. Their work yielded markedly different results.

At Tel Rehov, a site located to the south of Beth Shean on the West Bank of the Jordan valley, they obtained results that are in perfect agreement with the conventional (high) chronology. While examining the final Iron Age IB stratum, their results peaked at 990 BC, which closely conforms with the conventional date for the end of the period. Similarly, their results for the earliest phase of Iron Age IIA peaked at 965 and 935 BC, see *Figure 15*.

As for Tel Dan, the same authors obtained dates within the 11th and 12th centuries BC for Stratum V, which is conventionally dated to *c.* 1150–1050 BC in the high chronology. Even so, charred olive pits yielded a 14C date ranging between the 13th and 11th centuries, while according to them the date could 'even [be] higher'. The authors however acknowledged that their database of short-lived samples was simply too small to allow for a satisfactory Bayesian analysis.

At Tel el-Qudeirat (likely biblical Kadesh-Barnea) in the northern Sinai, they studied the oldest of three fortresses, conventionally dated to the 10th century BC. Yet, the calibrated 14C date of the lowermost destruction level peaked at 1195–1139 BC, which is almost 200 years older than the expected conventional date. The middle fortress also yielded a considerably older date, while the youngest fortress, associated with the Neo-Babylonian invasions, was correctly dated to about 600 BC.

Considerably higher 14C dates also derive from Horvat Haluqim, a site located some 45km to the northeast of Tel el-Qudeirat. The same is true for Khirbet en-Nahas, located in the east-southern Arabah Valley.

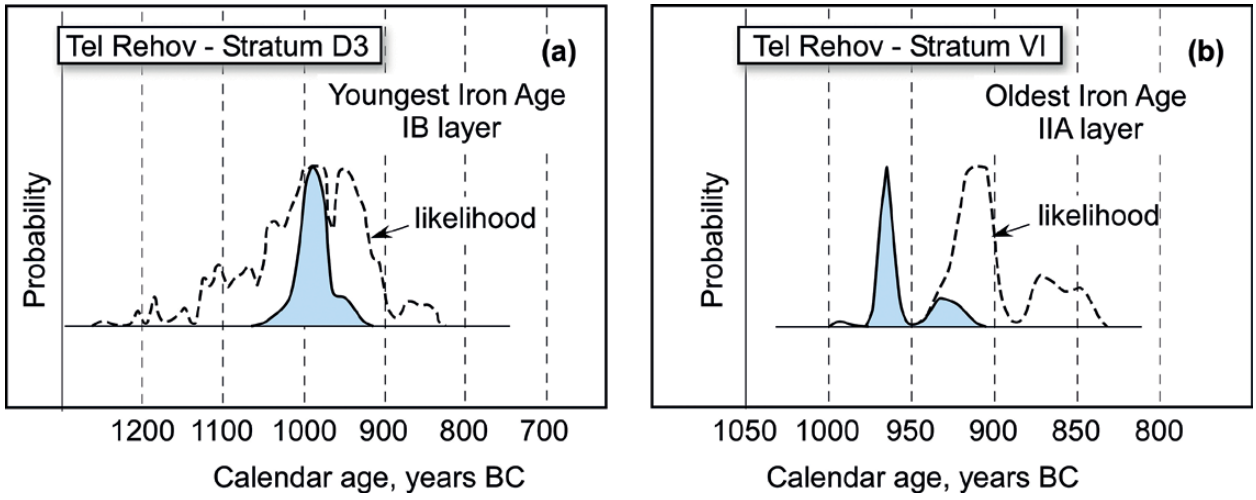

Figure 15. Simplified representation of 14C dates from Tel Rehov, according to van der Plicht et al. *(2009). (a) Latest Iron Age IB stratum; (b) Earliest Iron Age IIA stratum. The dashed lines indicate the likelihood ratio before correction by the prior information.*

Fantalkin *et al.* (2011) doubted these results because of site and sample selection, as well as with respect to the calibration carried out by van der Plicht *et al.* For the Tel Rehov data, they proposed a later date for the Iron I/IIA transition, i.e. about half a century later, in close agreement with the Tel Aviv low chronology (*Figure 16*). This illustrates the chronological 'flexibility' to which we alluded above. The different prior information used by Fantalkin *et al.* (2011) comprised of: (A) an unrevised reproduction of van der Plicht's analysis; (B) Stratum V (town) was not separated from the Stratum V destruction; (C) all short-lived samples conducted by other laboratories were included; (D) like (C), but with the Stratum V (town) now separated from the Stratum V destruction. As a result, their proposal was immediately rejected by Bruins *et al.* (2011).

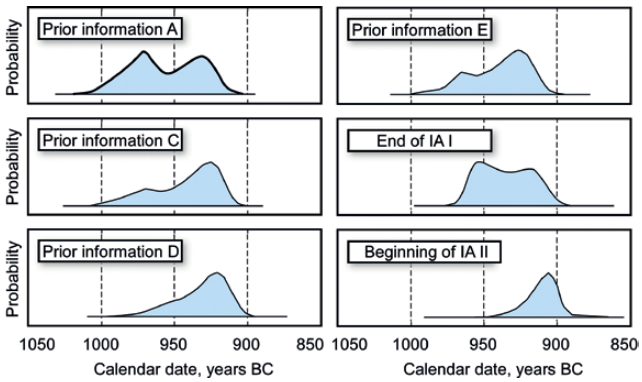

Figure 16. Simplified representation of 14C dates concerning the Iron Age I/II transition at Tel Rehov, according to Fantalkin et al. *(2011). The figures contain various prior assumptions.*

While further studies with similar results on the early Iron Age could be listed, these examples must suffice. As for Iron Age IIA, Levy *et al.* (2014) and Smith *et al.* (2014) have published 14C dates for ancient Edom in southern Jordan. *Figure 17* contains data concerning the fortress gatehouse of Khirbet en-Nahas, one of the main centres of ancient copper mining and metallurgy in the Wadi Faynan, which is located to the south of the Dead Sea. The results range from the 10th to 9th centuries BC and conform to a time range between *c.* 980 to 840/830 BC (Levy *et al.* 2014).

For the Iron Age II B–C site of Tawilan, an ancient Edomite town located due north of Petra, only one sample could properly be 14C dated (Smith *et al.* 2014). Even so its result is meaningful, as the 14C raw date cuts into a steep section of the calibration curve. See *Figure 18*. The probability peak of the calibrated age centres around 805 BC which is in total agreement with the conventional time range of 840/830 to 587 BC.

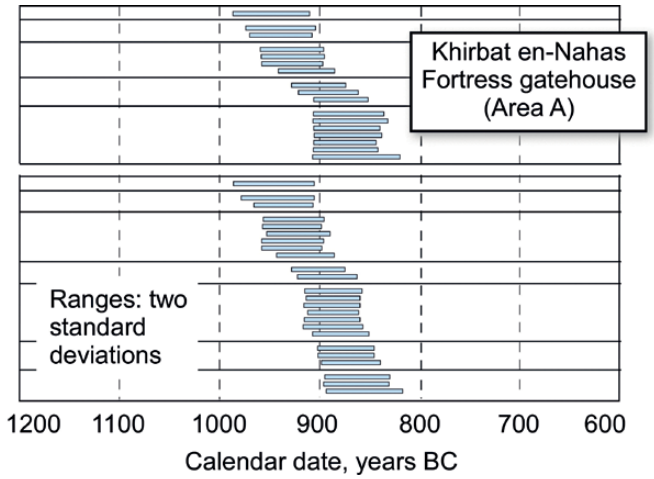

Figure 17. Simplified representation of 14C dates of the Iron Age IIA stratum at the Khirbet en-Nahas gatehouse, according to Levy et al. *(2014). The two series represent data from samples taken from inside and near the gate.*

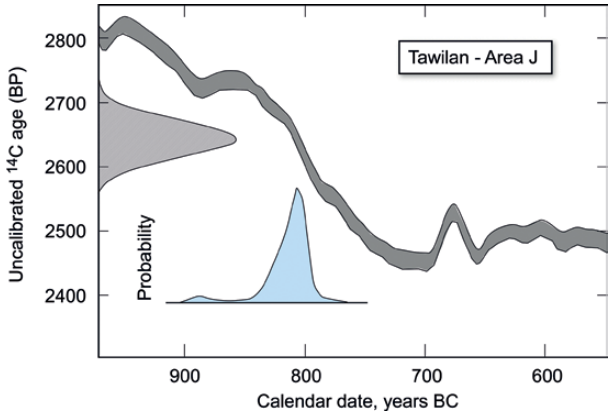

Figure 18. Simplified representation of 14C dates of the Iron Age II B–C stratum at Tawilan, according to Smith et al. *(2014).*

Superficially, the 14C dates and the archaeological ages seem to agree. Even so, we still are confronted with a major dilemma. For these same layers also contain pottery that is otherwise found in 7th century BC strata. It is especially the locally manufactured *Assyrianised* vessels (imitating Assyrian Palace Ware and simpler styles), including carinated bowls with everted rims and dimpled beakers, that give us food for thought. For these vessels are clearly imitations of pottery styles so typical of the Sargonid era, stretching from the late 8th to the end of the 7th century BC, while at other sites (including Tall Shekh Hamad) these vessels continued to be manufactured during the Neo-Babylonian period. Fine specimens of the Sargonid period (including dimpled beakers, as a true innovation of that period) have been found at multiple centres throughout the Assyrian heartland, including Nimrud and Ashur

(Hausleiter 2010). The local imitations deviate from their Neo-Assyrian prototypes in thickness, design and firing. As seems logical, the original prototypes must have existed *earlier* than their locally manufactured *imitations* and not vice versa (for a detailed discussion see van der Veen 2020 and in this volume, pp. 291–310). In fact, it seems reasonable to assume that some years must have passed before local potters even started imitating these styles in their southern Levantine workshops. It is therefore totally inacceptable to assume that they had produced these vessels one century earlier, even before the onset of the Neo-Assyrian hegemony over the southern Levant (Na'aman and Thareani-Sussely 2006).

As an *ad hoc* hypothesis one can always argue that those 'earlier styles' are dissimilar and do not therefore relate to the 'later' finer Neo-Assyrian prototypes, as Smith and Levy actually claim in terms of some of their carinated bowls (i.e. the Khirbet en-Nahas BL 22-types; Smith and Levy 2008). However, as Piotr Bienkowski has rightly argued: 'close examination of the Transjordanian parallels demonstrates that there is no appreciable difference' between the two groups (Bienkowski 2011, 77).

The observation is well taken, for it shows that the supposed synchronism between ^{14}C and traditional archaeological dates is not necessarily set in stone. In a similar vein, we have previously referred to another astounding discrepancy between ^{14}C and historical dates. That one relates to the destruction of Nineveh, whose historical date is generally accepted to be firm (612 BC; Zerbst and van der Veen 2015). In 2012, Taylor *et al.* published ^{14}C dates of bones belonging to three human skeletons, which had been excavated at the most southern bastion of the eastern city wall. The bones belonged to warriors who had died during the city's assault in 612 BC. Since the skeletons were found *in situ* (i.e. they had been found buried under the collapsed structure), there can be little doubt that the archaeological assignment is valid. A total of nine samples were examined, which, however, did not qualify as a series for Bayesian statistics, since all men had died at the same time. However, since the *preliminary* ^{14}C ages fall in a steep section of the calibration curve, the quality of the calibrated age is nonetheless comparably good.

The result came quite unexpected. Two samples showed multiple solutions. The ages of all others fell within a range between 845 to 760 BC (two standard deviations), see *Figure 19a*. Since there is no doubt about the historical date of 612 BC, the only possible conclusion is that the ^{14}C ages are too high, i.e. by almost two centuries.

Of course, the authors did consider various possible causes, why the results deviate by so many years. Indeed, various solutions were examined. Only one however stood to testing: the deviating results could only have been caused by a dietary reservoir effect, if the soldiers had mainly lived on fish that had lived in a different isotope equilibrium of the Tigris River or in some related lake. However, no fish bones were found in the archaeological repository. Insofar, the problem remains unsolved.

Further ^{14}C data of a chronologically undisputed event were recently published by Regev *et al.* (2024a, b). They relate to the destruction of Jerusalem by the Neo-Babylonians in the year '586' BC [eds *i.e.* 587 in this volume]. The authors list 15 short-lived samples from

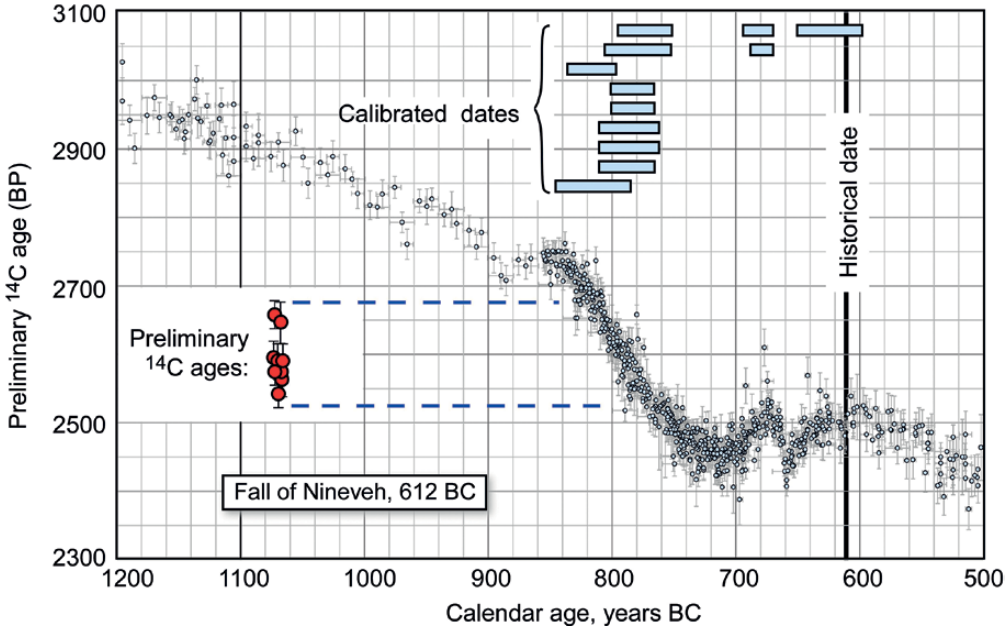

Figure 19a. ^{14}C ages relating to the fall of Nineveh in 612 BC. Data taken from Taylor et al. *(2012).*

Sample RTD	Material	Uncalibrated date years BP	Calibrated date range shift to '586' BC in years		Probability
			min.	max.	
9612*	Thin branches ('basket')	2581±17	+ 192	+ 208	68.3%
10525	Olive pit	2561±25	+ 10	+ 20	68.3%
10776	Bones	2540±23	+ 10	+ 20	68.3%
10532	Isum sp.	2538±18	+ 10	+ 20	68.3%
10754	Wood shrub (twig)	2537±16	+ 10	+ 20	68.3%
9963*	Twig	2537±59	+ 158 + 78 – 36	+ 208 + 105 + 60	21.1% 10.6% 36.5%
9715*	Thin branches ('basket')	2536±21	+ 166 + 82 + 38 + 6	+ 198 + 96 + 46 + 25	35.2% 13.1% 4.6% 15.4%
10649	Linen	2527±24	+ 10	+ 20	68.3%
8128	Olive pit	2526±25	+ 9	+ 20	68.3%
10650	Linen	2524±25	+ 9	+ 20	68.3%
10592	Barley	2521±21	+ 9	+ 20	68.3%
10753	Wheat	2514±17	+ 9	+ 20	68.3%
9613*	Grape pip	2513±17	+ 164 + 81 + 2 – 14	+ 185 + 99 + 50 – 8	16.4% 14.3% 33.9% 3.6%
10759	Olive pit	2506±30	+ 88 + 9	+ 91 + 20	10.2% 58.0%
10755	Olive pit	2490±17	+ 88 + 8	+ 91 +20	10.0% 58.3%

*Table 1. Calibrated dates for one standard deviation according to Regev et al. (2024b). The samples marked with * derive from 'Area U' and were taken from their Table S5. These combined data sets were not processed with Bayesian statistics. The remaining (individual) samples from the Givati Parking Lot (GPL) correspond to Fig. S42 in their SI appendix. Here Bayesian statistics were applied.*

the corresponding destruction horizon. Table 1 shows the deviations of the calibrated ages (scatter band for one standard deviation s) from the date of '586' BC. As these are all short-lived samples, the differences should be minor. For comparison: The outer ring of an oak possibly from a roof, yielded a date between 896 and 932 BC (sample RTD 11182) 42.2% probability; Regev *et al.* (2024b, Tab. S5), i.e. no less than three centuries prior to the destruction (not long after the first temple had been constructed under Solomon). Be this as it may, this still makes reasonable sense, given the old wood effect.

The comparison of the '586' BC-data is made more difficult by the fact that not all of them (*i.e.* from Area U of the City of David National Park) have been processed with Bayesian statistics. For example, RTD 9612 (with only one, very pronounced probability peak) shows a discrepancy of 192 to 208 years with the historical date, a situation reminiscent of the 'Nineveh 612 BC' example.

In Area U, the excavators uncovered part of a basket (RTD 9612), the remains of which were 'glued' to a large body sherd of a so-called 'rosette storage jar.' This diagnostic oval shaped storage jar (named after the rosette seal impressions stamped on its handles, i.e. Tufnell storage jar type-483), is the clear successor of the *lmlk*-jars (Tufnell storage jar type-484) and is a diagnostic type only found during the final phase of the Iron Age IIC horizon (*c.* 630–587 BC, van der Veen 2014, 143–144, Figs. 31a–b). There can therefore be no doubt that the basket belongs to the final years of Jerusalem, prior to its destruction by the Neo-Babylonian army in 587 BC. Its calibrated date 794 BC (68.3%) 778 BC is therefore off by some 200 years.

However, the individual samples RTD 10525, 10532 and 10754 show ages that are merely off by one to two decades, i.e. after Bayesian statistics had been applied. The by far highest of several probability density peaks P of the calibrated (albeit unmodelled Bayesian) data, would likewise have yielded larger discrepancies (RTD 10525: 174-210 years, P = 66.5%; RTD 10532 168-199 years, P = 42.4%; RTD 10754 168-199) years, P = 42.0%, [i.e. the original data according to Tab S5]), had the Bayesian method not been applied. Concerning other samples (RTD 10649, 8128 10650, 10592 and 10753) the calibrated data without Bayesian statistics, yielded multiple peaks and relate to a rather flat probability density function. For samples RTD 10759 and 10755, the discrepancy of the modelled data is *c.* 10 to 20 years (P ≈ 58%) and approx. 90 years (P ≈ 10%).

Regev *et al.* rule out laboratory bias, as well as a low resolution of the calibration curve in the relevant section, but suspect 'some real site or regionally specific effect' that could have caused the temporary ^{14}C depletion.

To summarise the above, the situation can be described as follows: As can be seen in *Figure 19b*, the raw ^{14}C data tend to lie above the calibration curve in both cases (as related to Nineveh 612 BC and Jerusalem '586' BC), albeit to a lesser extent for Jerusalem. To render the calibration more consistent, the calendrical ages need to be shifted to the left, i.e. to older values in both instances. With respect to Jerusalem '586' BC (although this does not affect all the samples), the trend remains the same. What makes things more difficult, however, is that the 'corrected "586" BC-data' fall within the Hallstatt plateau, whereas for 612 BC, they would

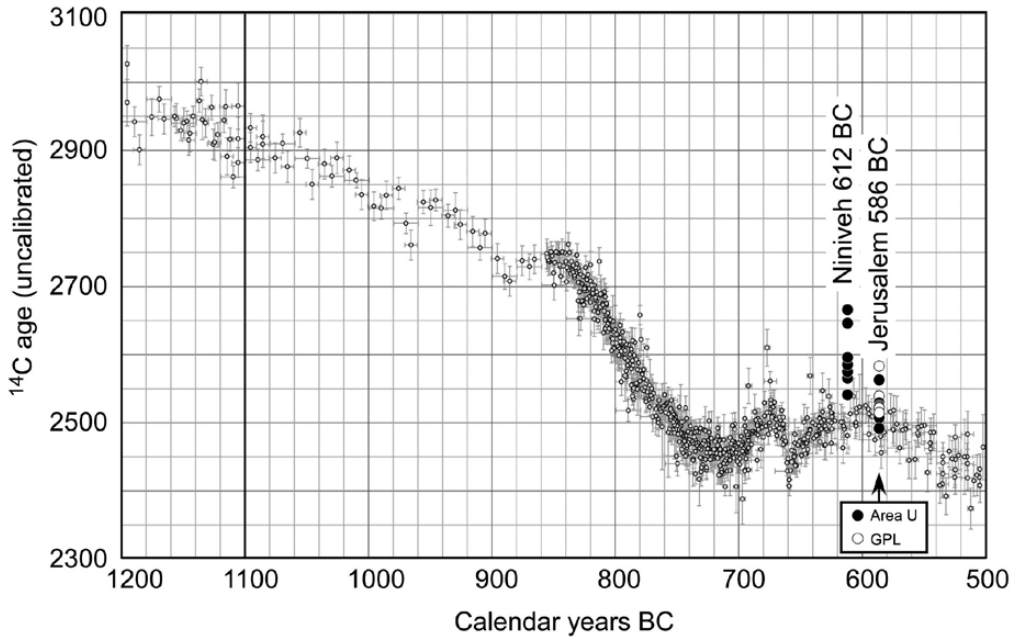

Figure 19b. The '586 BC' Jerusalem ^{14}C dates (Area U, white circles, and GPL, black circles) in comparison with the Nineveh 612 BC-event.

be shifted to a steep section of the curve, before this period.

3.6 Summary and potential explanations

Based on the current state of knowledge, we note that although the application of Bayesian statistics has reduced the hitherto larger offset between ^{14}C and archaeological ages, the discrepancy remains. A detailed discussion is especially problematic as a thorough comparison of data is only possible to a limited extent, as these were generated under different conditions (with respect to sampling strategies, which calibration curves were used, etc.). Nevertheless, a pattern is discernible. For the earlier time considered here (i.e. the Old Kingdom and Early Bronze Age), ^{14}C dates are between 150 and 300 years higher than the traditional dates. The difference decreases during the Middle Bronze Age in Egypt and the Levant and during the subsequent Aegean Late Bronze Age. Even so, ^{14}C dates are still too high, but the difference has been reduced to between 50 and 150 years, i.e. on average to *c.* 100 + x years, in line with the results derived from Tell el-Daba (*Figure 1*). This offset continues until the beginning of the Iron Age. For the subsequent period we observe in various studies a narrower gap, while different prior information and outlier rejection strategies have led to reduced offsets of only half a century or so. Toward the end of the Iron Age ^{14}C and conventional archaeological dates seem to largely agree. Yet, this observation may be more apparent than real. For the archaeological evidence cited above (i.e. locally produced Neo-Assyrian styles) suggests that the archaeological chronology is still substantially flawed. Similarly, ^{14}C probes relating to the Fall of Nineveh (an event that undisputedly occurred in 612 BC) and those in connection with the fall of Jerusalem, 587 BC, rendered dates that are virtually one and a half to two centuries too high.

While protagonists of the ^{14}C based high chronology claim that archaeological dates of the earlier periods are too low, the finds at Nineveh and Jerusalem show that ^{14}C yield dates that are clearly too high. What could be the potential causes that may have led to the noted chronological discrepancies between ^{14}C and historical dates?

(a) Sample contamination

It goes without saying that sample contamination mainly with ^{14}C depleted old carbon must always be considered. The question is, however, whether contamination, which occurs repeatedly in individual cases, can explain the overall trend, or if these discrepancies are rather related to outliers that are excluded from the analyses. In other words: Are more general contamination effects conceivable or does contamination rather relate to individual samples only? Bietak (2021b) recently asked if older material in stratified settlements could perhaps have been displaced by the construction of pits, graves and foundation trenches, explaining the higher dates. This, however, is not a very likely explanation for monumental structures, inside of which pits and foundation trenches, let alone graves, are only rarely found. Fantuzzi (2019) also suspects that residual seeds from stratigraphical modifications could have affected chronological offsets. So far, these suggestions can however only be *ad hoc* hypotheses. Even so, it is conceivable that a consistent application of *microarchaeological* principles in sampling will answer some of these questions during future excavations.

(b) Local isotope equilibria

A whole series of proposals have been made concerning local isotope equilibria. In 2002, Keenan formulated a hypothesis suggesting that ^{14}C depleted water from the last Ice Age could have upwelled to the surface in prehistoric times. The result would have been a prolonged period of outgassing of old $^{14}CO_2$ into the local atmosphere throughout the eastern Mediterranean. That this may have been the case, is substantiated by the influx of fresh water from the Black Sea, which flooded the region at the end of the Ice Age, penetrating into the eastern Mediterranean. The fresh water would have formed an insulating layer on top of the Mediterranean water over an extended period of time. Eventually the old water would have reached the sea surface around 4500 BC, causing the given effect. Except for one response by Manning *et al.* (2002), who rejected Keenan's proposal, we remain unaware of any further discussions on the subject. As a matter of fact, Keenan's theory will likely be difficult to prove.

The idea of old $^{14}CO_2$ from the Mediterranean has also been advocated by Hagens (2014). That his theory does hold some water, is substantiated by the fact that the isotope equilibrium in the southern hemisphere (with its much larger maritime surface and its higher wind speeds) has shifted in comparison to the northern hemisphere. Regarding Tell el-Daba, which lies more or less at the heart of the discussion, another effect may have been inflowing seawater into the Nile delta which had influenced the isotope equilibrium in plants. Hagens cites several studies confirming which effect wind-blown sea water can have even some 50km inland.

Hagens also discussed the possibility of a freshwater reservoir effect from the Nile water (Hagens 2014) and refers to crops that had been harvested in the Nile Valley between AD 1702 and 1881. These yielded ^{14}C dates that were about 20 years higher than anticipated. The study was conducted by Dee *et al.* (2010) who, however, did not agree with Hagens' interpretation. As a matter of fact, the effect is far too small to explain the discrepancies mentioned above, as the 19.5 years-offset could easily be explained as the mere result of seasonal differences in ^{14}C accumulation in plants in connection with the different vegitation phases in Egypt (late winter–

early spring), especially if we compare these with the vegetation phases to the north (late spring–early summer) in Europe and to the mountain landscapes in North America, where the existing calibration curve was determined.

A general argument against the Mediterranean influence on ^{14}C data is that the discrepancies are also found at greater distances from the coast, such as in desert areas. This argument also concerns local hard water effects (i.e. that the water comes in contact with carbonate rocks).

Regarding the volcanic island of Santorini, the possibility of outgassing old CO_2 with low ^{14}C content from the volcano has been considered. There can be little doubt that this effect indeed exists (for examples, see Wiener 2012). The arguments for and against a volcanic effect will not be studied here, but for more details, see Wiener (2012) and Zerbst and van der Veen (2015).

On the whole, we observe that the strongest counter-argument by far is the consistent offset in different regions.

(c) Problems with archaeological dating

Scholars including Felix Höflmayer and Sturt Manning working with higher dates for the Middle and earlier Late Bronze Ages based on ^{14}C, have recently sought to correct Manfred Bietak's archaeological chronology for Tell el-Daba (which is based on a long-term cooperation with other scholars). This scheme served most Levantine archaeologists as the chronological benchmark for sites in the Levant since the early 1990's. While this timetable is primarily based on ceramic imports and imitations (including Painted Levantine Ware, Minoan Kamares Ware, Base Ring I and II) and metallurgical synchronisms (mostly based on weapons, including socketed spearheads, fenestrated axes and duckbill-axes), as well as scarab evidence from Egypt and Syria and the Levant, proponents of the higher ^{14}C dates now seek to relativise these archaeological synchronisms by questioning their relevance. By placing the emphasis on ^{14}C dates derived from other Levantine Middle Bronze I–III sites (such as Tell el-Burak, Tel Ifshar, Tel Kabri and Ashkelon) and museum artefacts from Middle and New Kingdom times (Bronk Ramsey *et al.* 2010b), Tell el-Daba's central role for dating the stratigraphy of other sites in the region, now vanishes. Even Bietak's major pillar in the Tell el-Daba 'master stratigraphy' – the discovery of multiple seals and bullae of early 18th Dynasty pharaohs in Stratum C/3 – is now being questioned (Höflmayer 2015). For by reallocating the strata to historically earlier events, the 18th Dynasty sealings can no longer belong to that stratum but must be assumed to be intrusive. The overall claim that stratigraphical dates of MB I–III must be raised, is further believed to find support from Upper Egyptian Tell Edfu, where Nadine Moeller and Gregory Marouard (most recently 2018) uncovered bullae of the 13th and 15th Dynasty pharaohs Sobekhotep IV and Khyan in one stratum side-by-side with scarabs of the so-called 'late Palestinian' (Canaanite) series, that were hitherto assigned to the later half of MB II/III, at Tell el-Daba and in Syria and the Levant. Based on Tell Edfu, both pharaohs are now considered to be near contemporaries, assuming that the start of the Hyksos' 15th Dynasty must be raised to before 1700 BC, i.e. some 100+ years earlier than was hitherto believed. This interpretation would have important repercussions for Tell el-Daba and for the Second Intermediate Period as a whole, if of course the interpretation of the finds stands up to scrutiny.

And indeed, this is the issue at stake, for it remains uncertain if the Tell Edfu evidence has been correctly understood (for later pottery types suggest that the stratum outlived *at least* the 13th Dynasty, while the scarabs of Sobekhotep IV merely indicate a *terminus post quem*). More critically so, it remains unclear if the finds at Tell Edfu suggest a lengthening of the Second Intermediate Period (as ^{14}C chronologists claim) or a shortening, in which case the difference between 'the archaeological/historical dating and the ^{14}C dating' would be aggravated (Bietak 2021, 28). Either way, scarab specialists, including Daphna Ben-Tor, have repeatedly argued against a radical overlap of Dynasties 13–15, as this would cause innumerable problems with arthistorical questions relating to scarabs of the so-called 'late Palestinian series' (Ben-Tor 2018, 86; also Bietak 2021a). Moreover, a systematic study of stratified royal scarabs at Egyptian and Levantine sites, also seems to support the traditional sequence of Dynasties 12–18 (Ben-Tor 2018). As a matter of fact, the earliest Khyan seals appear at Tell el-Daba in the early Hyksos Stratum E/2–1 (Ben-Tor 2018; Forstner-Müller and Reali 2018), rendering a restratification as per the ^{14}C chronologists unlikely (Forstner-Müller and Reali 2018).

Recently, in a long reply to his critics, Bietak listed numerous examples from ceramic and scarab studies, why he feels that the 'master stratigraphy' of Tell el-Daba still holds water. According to him, also the connection between Stratum C/3 and the early 18th Dynasty based on scarab evidence cannot be easily broken:

> Another firm peg in the chronology of Tell el-Dabᶜa stratigraphic series is Phase C/2, dated by a series of scarabs from the time of Ahmose, Amenhotep I, Thutmose I, Thutmose III and Amenhotep II, *found in a workshop of the late Thutmosid Period*, since the latest seal dates the assemblage' (Bietak 2021a, 38; emphasis added). While the scarabs belong to the magazines of the royal palace and no evidence supports the intrusion of the scarabs, 'an attempt

> to separate Phases C/3 from C/2 and to date D/1 and perhaps even C/3 back into the Hyksos Period ... is based on wishful thinking' (Bietak 2021a, 39).

Bietak also feels that his lower dates for the end of the Middle Bronze Age in the early 15th century BC are now substantiated by lower ^{14}C dates from Jerusalem by Regev *et al.* (2021), where 'MB III (MB IIC) dates into the 16th and lasts until into the 15th century. This is a result where William Dever and myself agreed upon ... already four decades ago.'

(d) Calibration curve: Beyond any possible doubt?

One important link in the ^{14}C dating process has not yet been addressed: the calibration curve with the tree-ring chronologies in the background. For if the calibration curve contained erroneous sections, the corresponding age data would also be erroneous. But would such errors even be conceivable? Before we can hastily answer this question with a negative, we need to consider some basic aspects and look briefly at the history of how tree-ring dating and ^{14}C joined together.

Just as ^{14}C dating is closely intertwined with tree-ring dating, the reverse is equally true. This concerns two aspects: (a) the preliminary dating of wood remnants of unknown age by ^{14}C and (b) the indirect allocation of the time window by wiggle matching with existing calibration curves (Leavitt and Bannister 2009). The problem consists in the creation of the 'long' or 'master' chronologies for determining the calibration curve. The method for joining tree ring series of different time periods is called '*cross-matching*'. If trees do not grow under stress-free conditions (for instance if they do not grow on riverbanks), their ring widths from year to year are different and if they grow under comparable conditions, their ring patterns also will be similar. If trees stem from different periods with partial overlaps, the ring sequences of the overlapping sections should roughly coincide. This match then is determined by *cross-matching* on a visual plus mathematical-statistical basis.

In an early article (see Pilcher *et al.* 1977), Irish dendrochronologists described the methodology they followed in creating a 2990 year 'floating' oak tree ring sequence as follows: They first built so-called 'site chronologies'. These are chronological blocks of timbers from one particular location (for an example, see *Figure 20*). This way, differences due to individual site conditions are averaged out and the noise-to-signal ratio is reduced. Pilcher *et al.* describe how they determined the age of sub-fossil oaks that had been discovered during road works in the 1960s: 'At that time the potential age of the timbers was not known, and a large number of radiocarbon determinations were carried out ... to establish the relative ages and the age range of the groups of timbers under investigation.'

These site chronologies rather than individual timbers were used as the basic blocks for long term master chronologies, which finally merged with tree-ring chronologies from different regions in Europe such as from Ireland, England and northern and southern Germany. To eliminate long-term trends, i.e. low-frequency fluctuations in the ring width record, the data must be 'detrended' by a high-pass filter, i.e. only the high-frequency components are being compared. Noticeably the specification of the approximate age by ^{14}C at the beginning of the process only provided mere 'ball parks', so to say (Leavitt and Bannister 2009). The final positioning was then accomplished by dendrochronological cross-matching. Possible errors were then easily eradicated. But could this mean that also larger offsets occurred during the process?

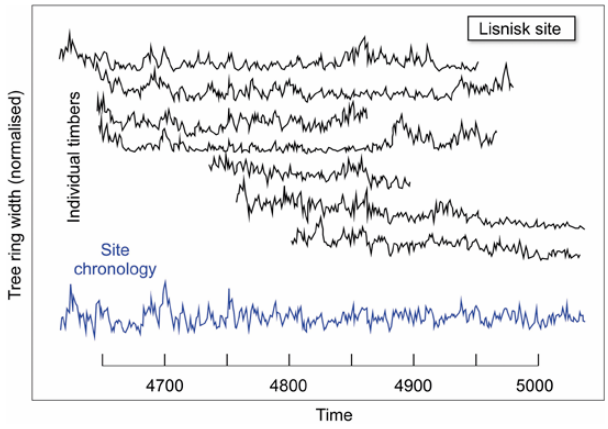

Figure 20. Construction of a local site tree-ring chronology; according to Pilcher et al. *(1977).*

A recurrent problem with time series in nature is that certain patterns are repetitive. In terms of tree-ring dating, this means that similar ring sequences repeat over time. In other words: Not just one, but several cross-matching solutions may be conceivable. The problem can be diminished by complementary measures such as by high-pass filtering. However, it is unlikely that inherent mistakes can be completely eliminated. In other words, if only one result remains, we cannot automatically expect that it is the right one, unless the correct time window is known based on other, prior information, sometimes through visual comparison.

In an older article Baillie (1991/1992) describes how the European master chronology was generated during the 1980s. Initially they were faced with the following discrepancy: during the process of cross-dating, the Irish and South-German chronologies did not match at the first go, which indicated that at least one of them was flawed. It turned out that the German chronology was only supported by three sequences in about 550 BC, which only weakly correlated. Furthermore, by evaluating the ^{14}C tree-ring year curves based on the

American Bristlecone pine calibration curve (which had existed since the late 1970s), an offset of some 70 years was discovered. The issue was finally solved by inserting an additional section of 71 years into the German chronology. Dendrochronological comparisons within the calibration curve are done by *wiggle matching*, i.e. by upward and downward swings of the curve new data are brought into position. Of course, such a procedure is only justified if the existing calibration curve is beyond any possible doubt. In section 2.1 (d), *supra Figure 2*, it was shown that the consensus calibration curves between 1993 and 2020 only differed slightly. However, this is not necessarily the case with the early American calibration curve, in which the scatter band of the preliminary ^{14}C ages was much larger.

The correction of the South-German curve was done back in 1984. In the same year the chronology was seemingly confirmed by the North-German curve. Baillie saw this as proof that potential errors in the calibration curves could in principle be detected and eliminated. In this he evidently was right, but even so the question remains whether a larger offset in the calibration curve could be discovered in the same way or whether a potentially wrong curve section, as a 'bequest' of the early American curves, would remain undetected.

Eventually, the first consensus calibration curve for ^{14}C dating was developed in 1986 on the basis of two American pine and the European oak tree ring chronologies. Since then, it has been revised on average every five years (Reimer 2021). Since 1998 the term IntCal was introduced to define this specific approach.

4. Could a More Profound Issue be at Stake?

At this point a general remark is due. It is well known from the theory of science that scientific communities identify themselves with so-called *paradigms*. The term stems from Thomas Kuhn (1970), while Imre Lakatos (1970) prefers the term 'research programmes'. Chalmers (2004, 108–109) characterises *paradigms* as follows:

> A paradigm is made up of the general theoretical assumptions and laws and techniques for their application that the members of a particular scientific community adopt ... The paradigm sets the standards for legitimate work within the science it governs. It coordinates and directs the 'puzzle-solving' activity of the groups of normal scientists who work within it.

While 'a mature science is governed by a single paradigm', over time, different paradigms may coexist. The relationship between them will likely be tense.

In principle, the scientific statements of a *paradigm* can be falsified (otherwise, according to Popper 1972, they cannot be called science). When a paradigm is attacked, its representatives will eagerly defend it. This can be accomplished by adopting *ad hoc* hypotheses. '*Ad hoc*' literally means 'to that purpose'. An *ad hoc* hypothesis is an 'extra postulate or a change in some existing postulate that has no testable consequences of the unmodified theory.' Its function is merely to 'protect a theory from threatening falsification' (Chalmers 2004, 75). Of course, this does not mean that all *ad hoc* hypotheses must necessarily be false or implausible. What it does do, however, is to prevent arguments of an alternative paradigm from being seriously considered, rather than debating it in an unbiased atmosphere.

If we apply these considerations to our current subject matter, we must stress that we are dealing with more than one *paradigm*. Some time ago, Manfred Bietak, the former director of the Tell el-Daba excavations, complained:

> Increasingly, scientific evidence is being used in an uncritical way to support many specific views and arguments. This may serve its purpose for a while, but it is highly doubtful whether such a procedure advances progress in research. In a similar way, it has in some circles become a crime to cast doubt on the reliable accuracy of radiocarbon dating. A critical attitude of itself becomes a *casus belli* instead of reflecting it and where there could be complications (Bietak 2013).

We would do well not to ignore Bietak's warning. If one scrutinises the publications of the ^{14}C community that were written over the last ten to twenty years, one regularly encounters the demand to abandon conventional archaeological chronologies in favour of the new data. If at all, they justify their challenge by pointing out that conventional archaeological dating based on ceramic stratigraphy or epigraphy is a highly complex field, to which frequently no satisfactory solutions may be found that are accepted by all other archaeologists. For sure, several historical-archaeological chronologies are on offer for certain periods of time, while regularly their proponents disagree among themselves.

For example, the archaeological fixation of the otherwise 'floating chronologies' of the Old and Middle Kingdoms in Egypt and the Early and Middle Bronze Ages in the Levant and elsewhere, widely depends on astronomical retrocalculations of the Venus and Sirius observations that allow for more than one solution. Another example concerns the conventional 'high' and 'low' Iron Age chronologies (although the latter was popularised by Israel Finkelstein, it had several forerunners including Dame Kathleen Kenyon), which differ from each other by some 50 to 80 years. In the shadow of the debate, several scholars argue for a more radical revision of Iron to Bronze Ages. While radically

lower chronologies were on offer in pre-Christian and early Christian times (Kokkinos 2015), and while the renowned Norwegian Egyptologist Jens Lieblein (1873) and the British classical scholar Cecil Torr proposed similar schemes during the 19th century, the discussion was more recently triggered by the book *Centuries of Darkness* authored by James *et al.* in 1991. Some of their arguments were subsequently adhered to by Finkelstein (James pers. comm.). Unlike Finkelstein's proposal, however, their approach challenges the traditional Egyptian chronology of the Third Intermediate and New Kingdom periods and the long-held synchronism between the biblical pharaoh Shishak and the founder of the Libyan 22nd Dynasty, namely Shoshenq I (for several articles on the topic, see James and van der Veen 2015). Although Shoshenq I was also dated to the time of the Israelite king Jehoahaz (more than a century later than is currently held) by the early church fathers Eusebius and Jerome (Chronicon, Bern Burgerbibliothek,_Cod._ 219,_f._35v), this topic should not further concern us here. (See further Wallenfels, 'Redating the Byblos Inscriptions', elsewhere in these Proceedings, pp. 249–263).

As stated above, archaeological dating is currently faced with several unsolved questions, even after 150 years of intensive research. Its protagonists continue to disagree in many respects. It is therefore even more understandable that proponents of the ^{14}C community have sought to push their results to the fore. ^{14}C is methodologically stringent and the progress that the approach has made in recent years, cannot be simply dismissed. Nonetheless, adherents of both methods often find little sympathy for each other, often due to ignorance concerning the others' specialties. One cannot therefore agree more with Wiener (2009) when he writes: 'Communication between scholars of ancient texts and archaeologists, on the one hand, and physical scientists, on the other, is often marked by lack of understanding of the nature and degree of uncertainty in data from other disciplines.'

While one may find sympathy with the current attitude of the scientists, this does not by itself mean that they must be right! For there remains an unneglectable inconsistency between the results obtained by ^{14}C and archaeological field work, even if the gap is narrowing. The following three developments may be anticipated in the years to come:

(a) Further research based on the most advanced state of the art will likely come to reveal virtual consistency between ^{14}C and conventional archaeological dating. This could be taken as evidence that both schemes are correct. However, this is not necessarily the case, as conceivably both sides could be mistaken. What will complicate the matter even more, is that currently we have several historical and archaeological chronologies at our disposal.

(b) The ^{14}C-community may well prevail, while previously derived archaeological dates will gradually be replaced by ^{14}C dates. This at least is what many scientists anticipate. Even so, while it may be justified to reject one or more archaeological scheme(s), one must also provide conclusive evidence that these chronologies are indeed flawed. The scientists will have to do more than just point at inconsistencies with ^{14}C. The replacement of one scheme by another will simply not do and is truly unscientific. For such proof must come from field archaeology itself.

(c) The problem is that we are comparing the results generated by two very different approaches. Any attempt at identifying the cause of the current discrepancies will fail if scholars continue to blame each other for retarding the process. To properly move forward, there can be only one solution. The uncertainties on either side must be removed by in-depth study and by showing that there is a consistent pattern to be followed. This has already been suggested by the present authors (Zerbst and van der Veen 2015). While it may not solve all remaining problems, it will certainly be an important step forward to check ^{14}C dates against undisputed historical and archaeological dates (i.e. from the late 7th century BC onwards). This can only be done properly, if always the latest state of the art and careful *microarchaeological* sampling is applied. One particular example comes to mind, i.e. the conflicting data associated with the Fall of Nineveh in 612 BC. Naturally such results must be carefully re-examined to avoid unnecessary mistakes and by asking questions if perhaps any offset may have been caused by other effects, such as by dietary effects in the case of Nineveh. Comparative evidence from other sites would also be needed to remove any prevailing doubts.

Two final examples will illustrate why we believe that such an approach can indeed succeed. Recently Regev *et al.* (2020) published a paper on Wilson's arch at the Herodian temple in Jerusalem. Based on *microarchaeological* sampling they provided ^{14}C dates ranging from the time of its construction until about AD 70. Their work included the entire Herodian era (20 BC to AD 70), during which the temple was rebuilt and its later history, when in AD 135 the Roman emperor Hadrian renamed the former Judean capital Aelia Capitolina. Their results impressively confirm that modern ^{14}C can indeed be successful. These are presented in *Figures 21* and *22* for the two periods mentioned. With respect to the building phase, Regev *et al.* obtained dates between 20 BC (with only one probability peak being somewhat earlier) and 60–70 AD (*Figure 21*) confirming known historical data. This is also true for the second example (*Figure 22*). In AD 135 Hadrian came to Jerusalem while visiting the eastern provinces of the Roman empire. On this occasion Aelia Capitolina was founded as a Roman colony. The ^{14}C investigation confirmed that the Roman

theatre at the site was constructed soon after, as one might have expected.

In both cases we find an almost perfect match between 14C and historical ages, and it is reasonable to assume that this will be true also for all of the AD period. As the Nineveh 612 BC and (less clearly so) Jerusalem 587 BC examples show, this is likely not the case before *c.* 600 BC, assuming that by then we enter troubled waters for 14C which will therefore need to be closely scrutinised.

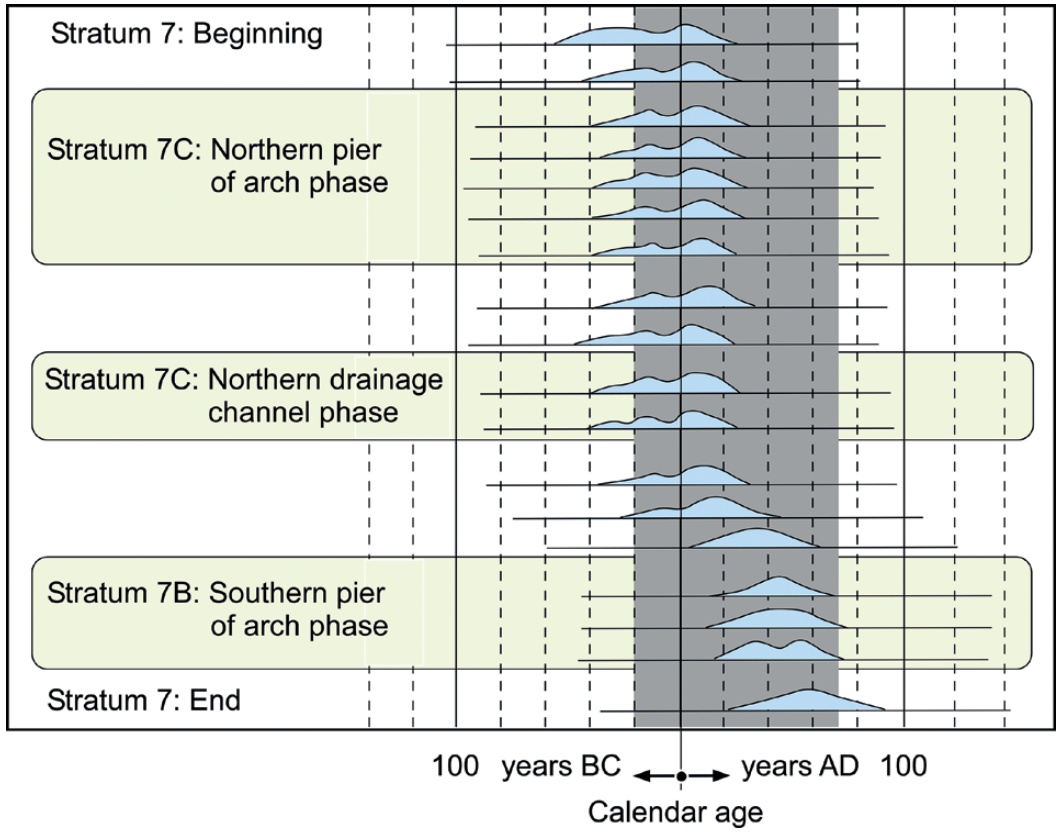
Stratum 7: Beginning
Stratum 7C: Northern pier of arch phase
Stratum 7C: Northern drainage channel phase
Stratum 7B: Southern pier of arch phase
Stratum 7: End
100 years BC
years AD 100
Calendar age

Figure 21. A simplified representation of 14C dates of the construction phase of Wilson's arch at the Herodian temple in Jerusalem, according to Regev et al. *(2020).*

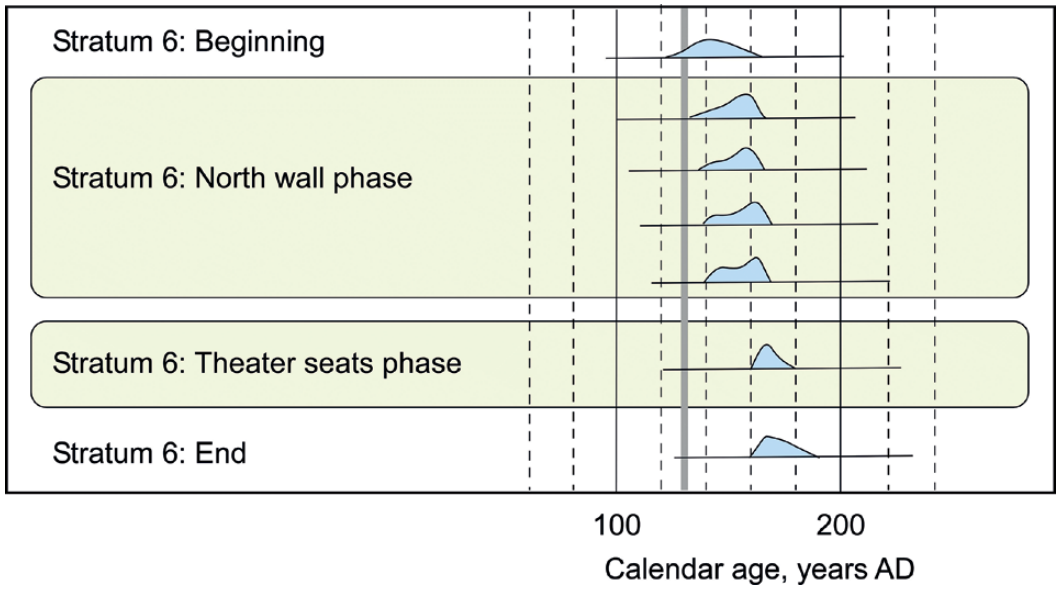

Figure 22. A simplified representation of 14C dates of the construction phase of the theatre near Wilson's arch at Aelia Capitolina after Hadrian visited the site in AD 135, according to Regev et al. *(2020).*

5. Conclusions and Work Ahead

In an earlier paper (Zerbst and van der Veen 2015), the authors addressed the question of the extent to which modern ^{14}C dating confirmed the conventional archaeological chronologies of the ancient Near East, as it was suggested by some papers published after 2010, or whether discrepancies still existed. The latter was the case. The main conclusion, however, was that caution should be exercised with regard to any far-reaching conclusion until the method is validated against the background of historically/archaeologically undisputed data from the late 7th century BC onward. For this period, the data situation was rather limited, but contradictions suggested themselves.

In the meantime, development has progressed further. The combination of Bayesian statistics in calibration and controlled sampling on site (microarchaeology) allows the very accurate dating of sample series with a small scatter band. In particular, it makes it possible to bridge horizontal sections of the calibration curve, and to sort out potential outliers in advance. This enables an optimization between accuracy and certainty that was not possible before.

The discussion of recent ^{14}C results in this paper distinguishes between different periods: Old Kingdom and Early Bronze Age, Middle Kingdom and Middle Bronze Age, Late Bronze Age and Iron Age. The results confirm the discrepancy found earlier, which seems to increase towards higher ages. One consequence is that demands become louder calling for the conventional archaeological chronology to be revised on the basis of the recent ^{14}C data, i.e. to shift it toward higher ages. However, since doubts regarding the 'secure' period after 600 BC have not been resolved either, the authors' concern takes on, if anything, even greater urgency. Before any far-reaching conclusions are drawn, the methodology – and indeed the whole chain including data collection of the ^{14}C data, the calibration curve, and the calibration itself – needs to be subjected to validation. This can only be done, as noted earlier, on the basis of a data set that is generally accepted historically, i.e. for the period from the late 7th century BC onward.

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The Ammiṣaduqa Tablet and Babylonian Chronology in Retrospect, Successes and Failures – 30-Day Month Analysis Revised

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The main focus in this article is the study of the astronomical considerations that provide chronological flexibility for alternative lower chronological scenarios (of which examples can be found in this volume) by widening the available search window. In other words, do we find lower candidates for the Ammiṣaduqa Venus solutions that will provide sufficient space for such scenarios, while the traditional retrocalculations do not? The present author believes that such evidence exits and that the methodology to arrive at these results complies with the accepted standards in the field

Introduction

There is an arguable hypothesis that at least one cuneiform tablet, Tablet 63 of the Babylonian planetary omen series *Enūma Anu Enlil* (= *EAE*) contains copies of observations made during the lifetime of the late Old Babylonian king Ammiṣaduqa, who reigned *c.* 1646–*c.* 1626 BC in the conventional, so-called Middle Chronology (see Reiner and Pingree 1975). It is not necessary that the remaining Venus texts originated from the same source because multiple primary sources are possible. Nevertheless, regardless of *EAE* 63's origins, inconsistencies among the preserved Venus observations make the tablet an unreliable source for dating the Babylonian First Dynasty. Yet by including further data such as solar and lunar eclipses, a more positive result can be achieved. Consequently, the 70 attested 30-day months — (38 for Ammiṣaduqa and 32 for his immediate predecessor Ammiditana (Huber 2017) — and the putative solar eclipse from Mari Eponym Chronicle (= MEC) II (A.1288 I) 25′–26′ (Birot 1985; Glassner 2004, 160–163) dated to one year after the birth of the Old Assyrian king Šamši-Adad I (Michel 2002), hold greater chronological potential. In general, previous analyses have used fewer month-lengths and offered poor support to most of the putative Venus solutions.

It seems probable that all the intercalations attributed to Ammiṣaduqa have been found unless there was one more early in year 1 of his reign (Roaf 2012; Huber 2017). A near complete set of intercalations for Ammiditana are extant, unless some are missing between his years 15 and 22, and in year 36, while doubt exists about the year 25 intercalation. Comparing putative Venus solutions with the Šamši-Adad I eclipse candidates (dated between the 20th and 17th centuries BC) have not yielded conclusive results, because the analysis offered also supports multiple solutions between the late 18th and late 15th centuries BC.

Taking into account the determination of Babylonian months, of which 12 possible sequence variations were needed to determine uncertain intercalations, 11 potential Venus solutions and several lunar sequences without Venus solutions were investigated by the author to determine 30-day months. Despite its limitations due to a wide range of clock-time (ΔT) determinations, the highest number of matches was discovered with the Venus solutions for the years 1483 BC and 1702 BC, followed by the 1582 BC solution. In addition, the 1499 BC year 1 lunar sequence was best supported by a putative Mari eponym no. 126 solar eclipse, i.e. the total eclipse of 16 April 1699 BC.

The Venus Tablets

The so-called Venus or Ninsianna tablet of Ammiṣaduqa is identified as Tablet 63 in the 70-tablet-long canonical planetary omen series *EAE* (Reiner and Pingree 1975, 1998, 2005). These astrological omens appear to associate astronomical observations with past historical events as predictors of future occurrences, should the astronomical phenomenon reoccur (Reiner and Pingree 1975; Huber *et al.* 1982). The first millennium BC Neo-Assyrian and Late Babylonian cuneiform sources known for Tablet 63 consist of some 27 fragments that appear to represent pieces of perhaps 11 different original tablets. In the authoritative edition of Reiner and Pingree (1975, 11), the 26 then-known fragments, with 6 recognisable joins, were sorted into

20 identifiable source tablets, labelled A–H, J–R, T–V. The authors there surmised that several further joins among these sources seemed likely: A(+)M, F(+)H, L(+)P(+)Q, and T(+)U. Walker (1984) has further suggested that J and O may join with F(+)H, and G and R may join with L(+)P(+)Q. Ossendrijver (2013) has identified yet another Late Babylonian fragment (BM 32107), source 'W' following established convention, which appears to join F(+)H(+)J(+)O. Source C (K.160), found amongst many other tablets and fragments comprising the 'library' of the seventh-century BC Neo-Assyrian king Assurbanipal excavated at Kuyunjik (Nineveh), remains the most complete copy (Rawlinson and Smith 1870; Sayce 1874). Further omens involving Venus are found in *EAE* Tablets 59–60 (Reiner and Pingree 1998).

Franz Kugler (1912) first recognised that Tablet 63 (K.160) could be a copy of astronomical texts from the reign of Ammiṣaduqa, based on the reference in omen 10 to the 'year of the golden throne', which recalls, in part, the name given to year 8 in contemporary inscriptions of Ammiṣaduqa.[1] A consequence of this is that the *EAE* tablets have been considered to have been compilations from the succeeding Kassite period and that they refer to events of earlier dominant city-states and dynasties within Mesopotamia.

The information contained on the Venus tablets has been considered as projections into a distant past (Kugler 1912; Langdon and Fotheringham 1928) and more recently so (Huber *et al.* 1982; Weir 1982) it has been suggested that they operate on the hypothesis that the orbit of Venus or the Earth, etc., has not changed appreciably. While making allowance for suspected copy errors and observational mistakes, the tablets would then imply that the orbit of Venus has not undergone a major change over time. In the main the planet appears to have behaved in a similar manner as at present. However, much depends on when the observations were made and whether they are all from the same Venus cycle. The dates are summarised in *Figure 1* based on the work of Reiner and Pingree (1975), with further additions (Weir 1982).

Hypothesis One

It is assumed that the Venus observations were originally recorded during the reign of Ammiṣaduqa, the penultimate king of the First Dynasty of Babylon. Tablet 63 is believed to be a copy of a First Dynasty tablet and that all other copies were derived from that same original. Thus, the Venus tablets originate from primary sources that originated during Ammiṣaduqa's reign. Some support for this view was explored recently (Nahm 2013; Roaf forthcoming), suggesting that the attested intercalations between years 1 and 16 agreed with the attested and inferred intercalary months on the Venus tablets. These are as follows: year 4, month XII_2 ($4.XII_2$), $5.VI_2$, $10.VI_2$, $11.VI_2$, $13.XII_2$, while the later intercalations $17.XII_2$, $19.VI_2$ or $19.XII_2$, $20.VI_2$ or $20.XII_2$ are inferred by the Venus tablets and not by other sources.

Hypothesis Two

It is assumed that only Tablet 63 is an authentic copy from Ammiṣaduqa's reign, while the others are a compilation of Venus observations from more than one single reign. Some of the omens associated with the Venus observations could be from earlier or later periods, because among the copies of the Venus texts, we detect inconsistencies in Venus appearances and disappearances (*Figure 1*).

In addition, Tablet 63 might also contain an idealised sequence of dates. This particularly relates to the second cycle on the tablet. Where the data possess a peculiar regularity, the disappearance at superior conjunction is always three months, i.e. the initial appearances in month I on day 2 (I.2), month II on day 3 (II.3), month III on day 4 (III.4), etc., continuing to month XII on day 13 (XII.13), while the morning and evening visibility is exactly eight months and four or five days. The data on the reverse of K.2321+K.3032 (source A) show disappearance and appearance dates arranged in monthly rather than in chronological order (Craig 1899; Weir 1982). All the disappearances in the month of *Nisannu* (I) are grouped together, as are all disappearances in the month of *Ajaru* (II), etc., until we encounter the disappearances in the month of *Addaru* (XII).

The many variations and inconsistencies in the recorded dates concerning the duration of Venus appearance and disappearance could be due to copyist mistakes, presumably because the texts were re-copied once earlier versions had deteriorated. There is also direct evidence for the manufacture of copies. Tablet W 1924.802 (source B), found at Kish in 1924, is dated to the reign of Sargon II (721–705 BC) of Assyria and is the oldest Venus tablet yet recovered. It appears to be a Neo-Babylonian copy of K.2321. The tablets that cover the same part of the Venus cycle are damaged and do not agree. On K.2321 the western setting is given as year 1, month XI, day 25 (1.XI.25) with the eastern rising on 1.XI.28, but on W 1924.802 this was recorded as 1.XI.15 with an eastern rising on 1.XI.28. Other variants mention the disappearance on 1.XI.18 and the appearance three days later, or else a disappearance on 1.XI.20 with a three-day period of invisibility. Also other dates disagree, but in many cases the differences concern only a few days. The prolonged disappearance in year 9 is found to be about nine months on K.160,

[1] 'Year in which (Ammiṣaduqa) brought into (the temple) E-namtila a throne made of reddish gold (GIŠ.DÚR.GAR KÙ.GI.GA.A ḪUŠ.A.TA), fitting for a distant place and a statue (representing him like a messenger) in a running posture' (https://cdli-gh.github.io/year-names/HTML/T12K10.htm).

with a western setting on 9.III.11 and the eastern rising on 9.XII.15 or 9.XII.16. An inferior conjunction disappearance should only last two to ten days. We also find a prolonged disappearance in year 12 lasting more than five months with an early eastern setting on 12.I.8 accompanied by a late western rising on 12.VI.25.

Errors in years 13 and 14 concern a shortened superior conjunction and a lengthened inferior conjunction. These are found to be contradictory. While in year 20 the western rising occurs on VI.24, the eastern setting falls on 20.III.25, but this is contradicted by the length of the invisibility of two months and six days (*Figure 1*).

Hypothesis Three

It is assumed that Tablet 63 is not a copy from the time of Ammiṣaduqa as it was mistakenly attributed to his year 8. In addition to year 8 Ammiṣaduqa, a number of Mesopotamian year names mention a golden throne: year 26 Hammurapi; year 5 Samsuiluna, years 6 and 19 Ammiditana at Babylon (Sigrist and Damerow 2001); year 'c' Lipiṭ-Eštar at Isin; year 'k' Zimri-lim at Mari; and year 'n' Ipiq-Adad II at Eshnunna. This evidence could challenge the view that a 'year of the golden throne' is unique to Ammiṣaduqa.[2] But a flaw in this argument is that year 8 is also a necessary corroboration.

This is an extreme view, given the evidence in view of the other omen texts, although it cannot be categorically ruled out. It would however mean that the *EAE* texts would be of little use chronologically and would be without a historical context. Furthermore, any references in the *EAE* omens to past events must then be considered dubious.

Only three Neo-Assyrian kings had regnal lengths compatible with 21 years of Venus observations: Shalmaneser III (858–824 BC), Adad-nirari III (810–783 BC) and Sennacherib (704–681 BC), whereas Sargon II (721–705 BC) had a reign long enough to cover the first two Venus cycles. The Bur-Saggilê-eponomy solar eclipse of 15 June 763 BC (or the less likely alternatives 13 June 809 BC, 8 June 803 BC, 24 June 791 BC or 16 July 754 BC) from the reign of Aššur-dan III (772–755 BC) disallows Adad-nirari III to match a Venus solution. While on the other hand Shalmaneser III could match a Venus solution in 858 BC, the dates would be 14–15 days out of step. This could have provided some of the information on the Venus tablets such as the observations of years 19–21. Sennacherib would fit a 706 BC Venus solution, but this is one year earlier than his traditional accession year and is also too late for the earlier Venus tablets. Having Sargon II as a contemporary of the oldest known copy of these tablets will almost fit the observations, i.e. covering the first and second Venus cycles, but the calculated sequence of disappearances and appearances is several days (five or more) out of step with the tablet dates. In principle some variants of the texts could possibly have derived from observations made during Sargon's reign, which would lend support to the notion that the Venus texts had multiple primary sources.

Comment

Several primary sources such as from the First Dynasty of Babylon appear feasible. If so, scribes would not originally have intended to record the precise nature of the Venus cycle and a plausible conjecture would be that omens associated with the observations were considered to be more important. Scribes would then have sought to make sense of the Venus cycle and compiled dates in a more systematic fashion. This could have happened by the 8th century BC or possibly earlier. The information on the tablets would allow for several potential candidates, supporting various chronologies, but a completely satisfactory result would still need to be found. Some potential exceptions would demand further study, while other corroborative evidence would require discrimination between the various Venus solution candidates.

Historical Overview

There have been many attempts to use the Venus tablets for dating the First Dynasty of Babylon. The first attempt was made by Franz Kugler (1912), who dated year 1 Ammiṣaduqa to the late third millennium to early second millennium BC (2041 BC, 1977 BC or 1857 BC). Langdon and Fotheringham (1928) generally agreed with Kugler, accepting that the dates belong to Ammiṣaduqa, but they disagreed with the astronomical calculations as the earth's deceleration had not been taken fully into account. Consequently, additional candidates for year 1 Ammiṣaduqa were added: 1921 BC, 1857 BC, 1809 BC and 1801 BC. Yet excavations at Mari showed that the First Dynasty of Babylon could not have come to an end earlier than the 17th century BC (Smith 1940), which was confirmed by Ungnad (1940) who suggested that the Venus tablet dated to 1646 BC. Van der Waerden (1945/1948) favoured a later date for the Venus observations of year 1 and proposed the date 1582 BC. His suggestion supported the Short Chronology proposed by Albright (1942) and Cornelius (1942). Next came Weir (1972), who supported the Middle Chronology (Rowton 1958), dating the Venus

[2] Note the cuneiform writing for the word 'throne' (Akk. *kussû*) in most of these year-names is typically rendered logographically GIŠ.GU.ZA, but, seemingly uniquely and perhaps not accidentally, the year 8 Ammiṣaduqa year-name is written with the synonym GIŠ.DÙR.GAR, just as it is on all four exemplars of *EAE* 63 (A, C, D, J) where omen10 is preserved (courtesy R. Wallenfels, personal communication).

Omen	Conjunction		Disappearance Date	Duration		Appearance Date	Duration	Cycle
1 & 57	Inferior	W	1.XI.15 or 25	3 d	E	1.XI.18 or 28	8 m 23 d	**1**
2 & 52	Superior	E	2.VIII.11	2 m 7 d	W	2.X.19	8 m 4 d	
3 & 48	Inferior	W	3.VI.23	20 d	E	3.VII.13	8 m 19 d	
4 & 43	Superior	E	4.IV.2	2 m 1 d	W	4.VI.3	8 m 29 d	
5 & 40	Inferior	W	5.II.2	18 d	E	5.II.8 or 18	8 m 7 d	
6 & 57	Superior	E	5.IX.12, 24 or 25	2 m 4 d	W	5.XI.16, 28, or 29	8 m 29 d	
7 & 51	Inferior	W	6.VIII.18 or 28	3 d	E	6.IX.1	8 m 20 d	
8 & 46	Superior	E	7.V.21	2 m 11 d	W	7.VIII.2	8 m 23 d	
9 & 44	Inferior	W	8.IV.25	7 d	E	8.V.2	7 m 23 d	
10	Superior	E	8.XII.25	2m 7, 9 or16 d	*W*	9.III.2, 4 or 11 d	9 m? 4d?	
11	Inferior	W	9. III.11 (XII.11)	9 m 4 d or 4 d	E	9.XII.15 or 16	8 m 25 d	**2**
12 & 49	Superior	E	10.VIII.10	2 m 6 d	W	10.X.16	8 m 10 d	
13 & 53	Inferior	W	11.VI.26	11 d	E	11.VIb.7 or 8	7 m 2 d	
14 & 38	Superior	E	12.I.8 or (III.8)	5 m 16 d	W	12.VI.25	7 m 10 d	
15 & 41	Inferior	W	13.II.5	7 d	E	13.II.12	8 m 9 d	
16 & 55	Superior	E	13.X.20 .21 or 24	15 d	W	13. XI.21 or XII.21	8 m 29 d	
17,50 & 60	Inferior	W	14.VII.,3 10,11 or 21	1 m 16 d	E	14.VIII.26 or 28	8 m 26 or 21 d	
18 & 47	Superior	E	15.V.20 or 21	2 m 15 d	W	15 VIII.5 or IX.5	9 m 0 d	
19 & 45	Inferior	W	16.IV.4	15 d	E	16.IV.20	7 m 25 d	
20 & 58	Superior	E	16.XII.15or 25	2 m 9 d	W	17.III.2 or 4	8 m 15 d	
21 & 59	Inferior	W	17.XII.10 or 11	4 d	E	17.XII.14	9 m	**3**
	Superior	E	(18.IX.8)	1m 28 d	W	(18.XI.6)	8 m	
34	Inferior	W	19.VIb.2	15d	E	19.VIb.17	9 m 8 or 11 d	
35 & 42	Superior	E	20.III.25	1m 9d, 2m 6 or16d	W	20.VI.1 or24	8 m 2 d	
36 & 39	Inferior	W	21.I.27 or 28	4d	E	21.II.3	8 m 25 d	
37 & 56	Superior	E	21.X.28	2m	W	21.XII.28		

E =East, W=West; the first Arabic number = year, Roman numeral = the Babylonian month, final number = the day; m = lunar month, d = days. Adapted from Reiner and Pingree (1975).

Figure 1. Babylonian dates of Venus disappearances and appearances.

solution of year 1 to 1646 BC, 1638 BC or 1630 BC. Subsequently Huber *et al.* (1982) dismissed the Middle Chronologies arguing that the attested 30-day month analysis rendered these dates statistically unlikely and in turn favoured Sidersky's Long (High) Chronology (Sidersky 1940), dating the first year of Ammiṣaduqa to 1702 BC by arguing that 1582 BC was impossible. Weir subsequently supported their analysis (Weir 1982).

Using the 30-day month dates listed by Huber *et al.* (1982) and Weir (1982), Mitchell (1989/1990) proposed a much later Venus solution in 1427 BC or 1419 BC suggesting that his new dates supported more drastic chronological revisions of two centuries or more, while Gasche *et al.* (1998) and Gurzadyan (2000) proposed a somewhat higher date for year 1 Ammiṣaduqa, namely 1550 BC. While this date is too high for the revision adhered to by James *et al.* and van der Veen and Zerbst (2022), it does support Gasche's Ultra-Short Chronology, a scheme accepted by Gertoux.[3] In 2010, Mebert preferred the 1574 BC Venus solution, supporting the Short Chronology. In the same year De Jong and Foertmeyer (2010) argued in favour of a middle-Middle Chronology date, namely1638 BC, based on the then accepted radiocarbon and ice core dates for the Theran eruption (Santorini). This argument was followed by Roaf (2012, forthcoming), Nahm (2013), and De Jong

[3] G. Gertoux, 'Mesopotamian chronology over the period 2340–539 BCE through astronomically dated synchronisms and comparison with carbon-14 dating'. ASOR Annual Meeting, Nov 2019, San Diego, CA. https://hal.science/hal-03090272v5/file/Mesopotamian-chronology%20revised.pdf (last revised 13 Jan 2024).

(2013). However, problems with ice core, radiocarbon dating and dendrochronology used in support of the Middle Chronology have presented themselves (Blackford *et al.* 2014; Zerbst and van der Veen 2015 and elsewhere in these Proceedings, pp. 373–399; Pearson *et al.* 2018).

Attested sets of 30-day lunar months, believed to be contemporary with the Venus observations, must be supported by a Venus solution. For almost all mentioned Venus solutions are accompanied by poor matches with the contemporary record of attested 30-day months (Lappin 2013; Huber 2017).

Following discussions in the early 2000s with Mitchell, Newgrosh, van der Veen, and Brack-Bernsen, the present author presented his initial research at the First BICANE Colloquium held at Woltersdorf (Berlin) in 2006, followed by the publication of his research on the Venus solution in 2013 (Lappin 2013). The analysis compared 12 putative Venus solutions and confirmed that Huber's Long Chronology date 1702 BC fared better than the Middle or Short Chronology candidates, while the later Venus solution of 1483 BC attained an even higher number of 30-day months by concentrating on ramifications of this finding.

Various solar and lunar eclipses were utilised to test the validity of a putative late 16th- or early 15th-century BC (1483 BC) solution. Investigations included the eclipse omens in *EAE* Tablets 18–21 (18.XI, 20.I, 20.III, 20.IV, 21.I, 21.IV, 21.VI, 21.VIII, and 21.XII) and in Shulgi Hymn D (Rochberg-Halton 1980), which likely relate to the dynasties of Akkad, Guti, Uruk V, Ur III, and Babylon I (Huber *et al.* 1982). However, suffice it to say, doubt cast on the Venus Texts in the *EAE* would likely also cast doubt on the reliability of these *EAE* sources themselves. Fortunately, we possess near contemporary records concerning a potential solar eclipse and a lunar eclipse contemporary with the First Dynasty king Hammurapi, the great-great-grandfather of Ammiṣaduqa. These are:

(1) From the time of Puzur-Ištar son of Nur-ilišu as found in the Mari Eponym Chronicle II. 25′–26′ with what is more than a mere allusion to a solar eclipse (i.e. 'the darkening of the sun') that occurred one year after the birth of Šamši-Adad I. Unfortunately, no month has been recorded which restricts the value of the eclipse identification,

(2) Mari Lunar Eclipse Tablet HC-A.25-115 was discovered by Parrot in the city of Mari. It was translated by Dossin (1951) and appears to be a letter to king Zimri-lim of Mari. The lunar eclipse from Mari occurred during the eponym of Ašqudum and accordingly would belong to years 11–12 of Hammurapi, even if years 4–5 cannot be excluded (Warburton 2000). Unfortunately, no month is given and as in some years several lunar eclipses occurred, a lunar eclipse in the correct year will merely confirm his first regnal year from a putative Venus solution. Lappin (2013) however argued that the 1483 BC solution may be supported by a credible date given for Šamši-Adad I's birth, by equating the MEC II. 25′–26′ solar eclipse with the large partial eclipse at sunset on 11 November 1683 BC or the total eclipse of 16 April 1699 BC.

Since the initial publication (Lappin 2013), there have been some modifications to our study: while two of the months used in the previous analysis are no longer

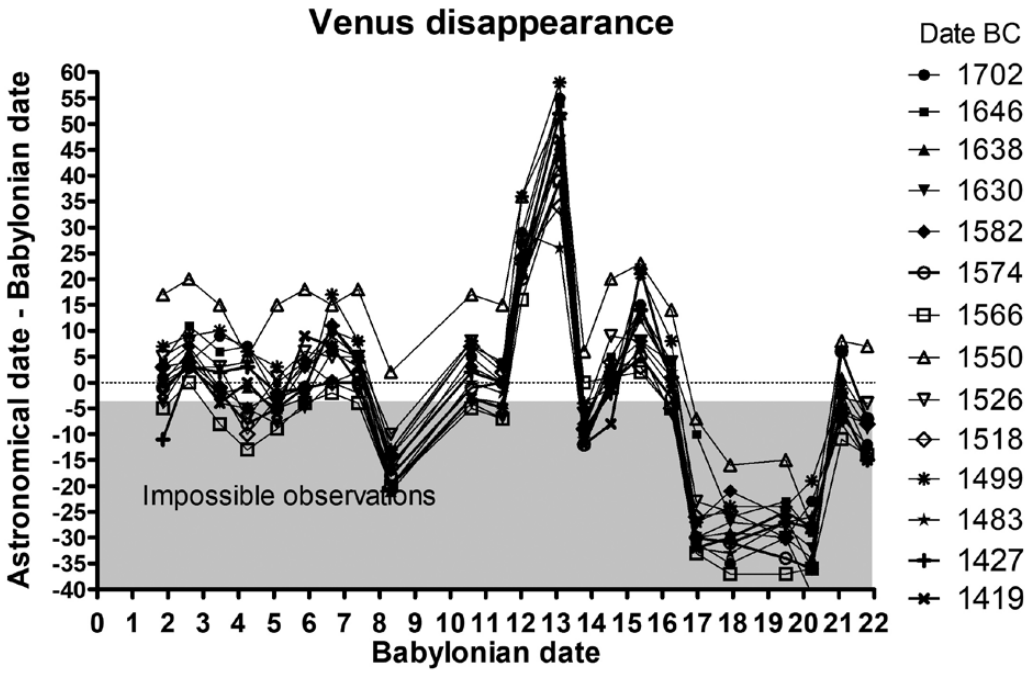


Figure 2. Venus disappearances of 14 Venus solutions.

considered valid, a greater number of 30-day months, i.e. 70 30-day months attested under Ammiṣaduqa and his immediate predecessor Ammiditana, have become available (Huber 2017). Some may need further backing. However, with a few exceptions, almost all Venus solutions analysed to date have failed, as they resulted in poor matches with the contemporary record of attested 30-day months (Weir 1982; Lappin 2013; Huber 2017). The required set of established cross-references in the Near East appears to be reliable and can now be employed to support the placement of Venus solutions in a firmer context.

The proposed strategy is based on a three-pronged approach. The first two approaches accept *Hypothesis One* or *Hypothesis Two* as cited above: i.e. (1) that the various Venus solutions are to be investigated regardless of other information and (2) that the MEC II. 25′–26′ (Šamši-Adad I) solar eclipse is a genuine phenomenon which allows for a comparison of Venus solutions with potentially visible solar eclipses over Mari or Ashur during the 20th–17th centuries BC, while accepting that the internal chronology of the First Dynasty of Babylon is sufficiently flexible to accommodate such a comparison. Consequently, it was necessary to ascertain that the 30-day month tallies with each Venus solution. The final approach (3) assumes that *Hypothesis Three* is correct and that the Venus tablets are not related to Ammiṣaduqa, while a match of 30-day months should be performed without a Venus solution determining the best matches in the lunar sequence of 29- and 30-day months between the 18th–15th centuries BC. The study should likewise match the putative MEC II. 25′–26′ solar eclipse candidates two centuries earlier.

Comparison of Venus Solutions between the 18th and 15th Centuries BC

The dates of Venus' disappearance and appearance were determined by a computer analysis for 14 Venus sequences between the late 18th and late 15th century BC, while Venus disappearance dates are shown in *Figure 2* and Venus appearance dates are found in *Figure 3* for three Venus cycles with solutions starting in year 1 Ammiṣaduqa in 1702 BC, 1646 BC, 1638 BC, 1630 BC, 1582 BC, 1574 BC, 1566 BC, 1550 BC, 1526 BC, 1518 BC, 1499 BC, 1483 BC, 1427 BC, and 1419 BC. The area of impossible sightings is shaded on each chart, indicating where astronomical disappearance dates are earlier than the tablet dates and where appearance dates on tablets are earlier than the astronomical dates. There are 11 satisfactory Venus solutions for year 1 in 1702 BC, 1646 BC, 1638 BC, 1630 BC, 1582 BC, 1574 BC, 1526 BC, 1518 BC, 1483 BC, 1427 BC or 1419 BC, whose matches, some better than others, depend on which versions of the Venus dates are selected. There is nothing much to discriminate between them.

Most potential solutions shown in *Figures 2–3* reveal a similar pattern with underlying variations particular to individual solutions. However, in the case of the 1566 BC, 1550 BC and 1499 BC solutions, disappearance dates are several days earlier than the astronomical dates (*Figure 2*) while the associated Venus appearance dates are impossibly early, some being more than 10 days earlier than the astronomical dates (*Figure 3*). These dates then are clearly untenable candidates.

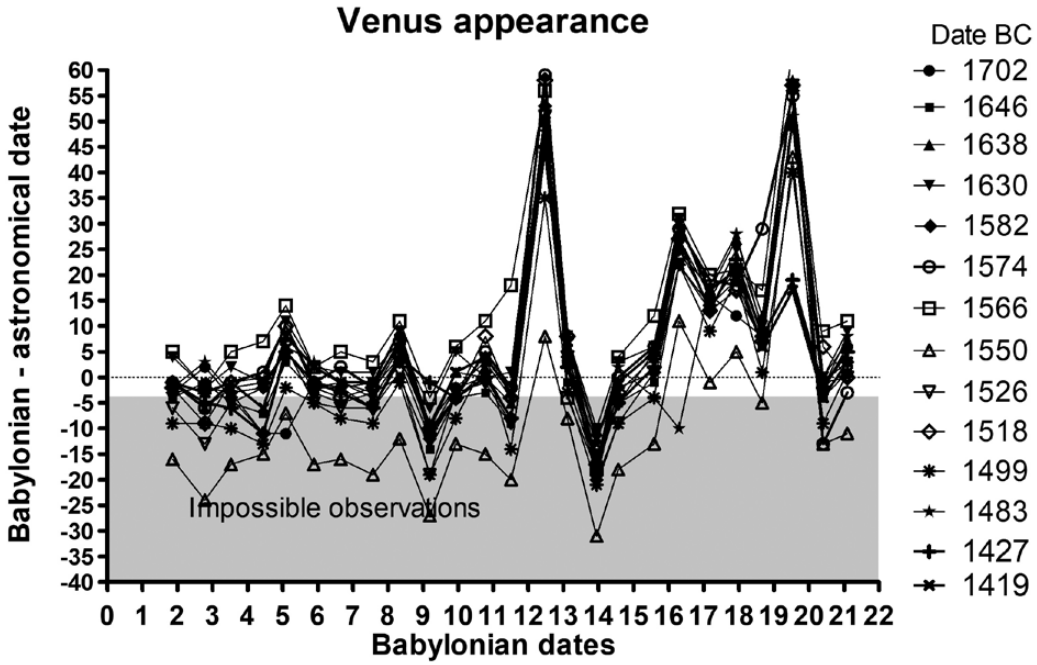


Figure 3. Venus appearances of 14 Venus solutions.

The long periods of disappearance in years 8–9 and 12–13 may be due to severe atmospheric disturbance. De Jong and Foertmeyer (2010) and De Jong (2013) have speculated that these disappearances coincide with the Theran eruption on the Isle of Santorini which blew large amounts of dust into the atmosphere. This could have affected the observation of the planet. It was believed that ^{14}C, dendrochronology and ice core supported an eruption date soon after 1630 BC (Baillie and McAneney 2014, 2015). This in turn was in close agreement with the Middle Chronology solution of 1638 BC (Roaf forthcoming) supported by radiocarbon dates for artefacts associated with Šamši-Adad I or his successors (Roaf 2012, forthcoming). But recalibration of the radiocarbon dates of olive branches yielded lower dates for Santorini around 1578 BC, (i.e. 2σ range 1614–1538 calibrated BC; Pearson *et al.* 2018). Robinson argues that the IntCAL20 sequence of radiocarbon may be 130–180 years too high and that a date nearer the first half of the 15th century BC may be in order (pers. comm. with S. Robinson; concerning problems with dating the Theran olive branch, see Ehrlich *et al.* 2021; also Zerbst and van der Veen 2015, 199–124, and elsewhere in these Proceedings, p. 381). Ice core data formerly attributed to the Theran volcano are now attributed to the Aniakchak II eruption in western Alaska (Blackford *et al.* 2014).

While describing the sequence of dates on the Venus tablets, Weir (1972, 1982) argued that the Middle Chronology solution yields an unfavourable set of dates. Whether the variations could be attributed to poor weather observations, adverse atmospheric conditions, differences in planetary motion or scribal errors, is doubtful.

Alternate primary sources

Hypothesis Two suggests that the Venus tablets derive from several primary sources and that they are a compilation of dates from several Venus cycles. These then would have generated the sequence which we have today (*Figures 2–3*). Copy errors of damaged original tablets would have contributed to the confusion. Moreover, the first Venus cycles of 1662 BC, 1566 BC, and 1542 BC agree with alternative dates of Venus disappearance and appearance and some periods of invisibility on variants of Venus tablets (*Figure 1*). First Dynasty kings that would match Venus dates traditionally ascribed to Ammiṣaduqa are Hammurapi and Abi-ešuh, who according to the traditional internal chronology of the First Dynasty ascended the throne only one or two years later than the potential Venus solutions. The Venus cycles in question would however need similar sequences of dates for the disappearance and appearance of Venus with slight variations which account for different implementation of intercalations in their year sequences. This would require only a minor adjustment to the traditional internal chronology. Even then it would not completely resolve all issues, particularly those related to the year 8/9 and 12/13 observations. Furthermore, observations after year 18 remain largely problematic. They could likely belong to another Venus cycle entirely. Although with some Venus solutions, years 19–21 dates could fit the Venus cycle attributable to Abi-ešuh about 67 years earlier than the Ammiṣaduqa Venus solution. However, there may also be alternative contenders from the Kassite through the Neo-Assyrian periods that could have contributed to the final collection of Venus dates, accounting for some of the major discrepancies in the Venus solution and of the third Venus cycle. Also other data would be required to determine the best possible Venus solution. If one considers *Hypothesis Three* (i.e. that the tablets are late constructions and that their value for determining First Dynasty Chronology is minimal), other sources would need to be found to locate a lunar cycle of 30-day months like those attested for Ammiṣaduqa, etc. Intercalations only attested by the Venus Tablets also would need reconsideration, but this would only impact on a few dates from the end of Ammiṣaduqa's reign which would not greatly affect the result.

Solar and Lunar Eclipses

The following data must be considered in addition to the *EAE* omens, which themselves could be of dubious validity. The following eclipses are of special significance: These concern (1) the Mari Lunar Eclipse Tablet - HC-A.25-115 (year 11/12 Hammurapi) and (2) the putative MEC II. 25′–26′ solar eclipse of one year after Šamši-Adad I's birth. According to the Assyrian King List Šamši-Adad I reigned for 33 years. He is believed to have died in the year of MEC II. 99 (Ṭab-ṣilli-Aššur). This would imply some 73 lunar years or 70 or 71 solar years, if a lunar calendar was in operation at Mari or 73 solar years if a Babylonian-style calendar was in use there. The phenomenon would possibly have occurred in year 10 or more likely in year 17 of Hammurapi (Warburton 2000). While the validity of the eclipse has been disputed by Gertoux,[4] who questions that the text refers to an eclipse, his doubts may have been influenced by his own assertion that the year 1 accession of Ammiṣaduqa dates to 1550 BC and Hammurapi's to 1697 BC.

The 'foundation of Babylon lunar eclipse' described on a cuneiform tablet found in Tell Muhammad in Iraq contained information regarding lunar eclipses that occurred in year 38 after the resettlement of Babylon by the Kassite kings (Gurzadyan 2000). The Tel Mohammed economic texts bore the inscriptions 'year that the Moon was eclipsed' and 'year 38 that Babylon was resettled' (Gurzadyan 2000). This eclipse is believed

[4] See note 3, above.

to have occurred in *Nisannu* (month I), likely near the start of the year, while the description seems to imply that the eclipse started near sunset. There appears to be a reference to a second eclipse in the month of *Abu* (V), but the date of the event (day 10) is suspect, except that a 360- or 365-day civil year allows eclipses to occur on different days of the month.

Although solar and lunar eclipses can be computed with a high degree of accuracy, clock errors determine whether sighting is feasible. Solar eclipses of less than 0.93 magnitude during the day are unlikely to be seen by unaided observers, except under exceptional conditions. However, low magnitude, 0.1–0.5, near-dawn or sunset solar eclipses are visible and are attested in the Neo-Babylonian record. Low magnitude lunar eclipses are almost always obvious as the full moon is darkened during the penumbral phase of the eclipse. Then the timing of the eclipse, when that information is available in the record, is a critical determinant.

Clock-Time error

The exact position of the moon in the sky on a particular date can be cast into doubt. This affects the possible sighting of the moon, changing the start of the Babylonian month. Also the alteration of the date when Venus would appear or disappear, the observation of lunar eclipses and more crucially seeing solar eclipses at specific locations, must be taken into account. This is due to the deceleration of the Earth's spin and gradual lengthening of the moon's orbital period (lunar month). These factors can be estimated from precisely dated events in the past, especially solar eclipses. To this effect spline curves have been constructed that extrapolate into the past, even beyond dateable eclipse events, as recorded in Arab, Hellenistic, Greek, Neo-Babylonian and Assyrian sources. These have been used in computer programs to assist the determination of ancient astronomical events. More precise measurements of astronomical phenomena in recent times have greatly facilitated the precision of these calculations.

It has been estimated that the error would amount to about half a day over the last 4000 years (Morrison and Stephenson 2004; Huber 2006, 2017).

The estimations of clock-time error utilised in the following analysis are:

ΔT (seconds) = c (32.5) × t^2
t = (astronomical date −1800)/100
(Huber 2006, 2017)

ΔT (seconds) = −20 + c (32) × t^2
t = (astronomical date −1820)/100
(Morrison and Stephenson 2004)

The accuracy of these determinations can be tested by the arrangement of solar eclipses which are usually sensitive to small clock errors. As stated above, *EAE* also contains details of lunar eclipses. Crucially, some solar eclipses associated with omens have been cautiously assigned to earlier kings from Akkad and to other Bronze Age Mesopotamian kingdoms. Long (respectively High) Chronology dates have been explored by Huber *et al.* (1982), Huber (1987, 2000, 2006); Middle Chronology dates by Huber *et al.* (1982), Huber (1987, 2000, 2006), Khalisi (2020); and Short Chronology dates by Banjević (2006), Huber *et al.* (1982), Huber (2000, 2006). Ultra-short Chronology dates ('New Low Chronology') were studied by Gasche *et al.* (1998), Gurzadyan (2000), Gurzadyan and Warburton (2006), Huber (2006) and Extremely Short Chronology dates by Lappin (2013). Dates related to the revised 'New Chronology' envisaged by Rohl (1995) were suggested by Mitchell (1989/1990). *EAE* eclipses have not been pursued further in the current investigation.

Mari eponym Chronicle II. 25-26 solar eclipse

As inferred above, MEC II. 25′–26′ may not necessarily refer to a solar eclipse. But if it does, it still does not mean that a total solar eclipse was implied. Rather it may refer to a near sunrise or sunset eclipse, the expression in the inscription being merely 'darkening of the sun'. However, assuming that a solar eclipse is meant, potential eclipse candidates for each putative Venus solution can be found in *Figure 4*. Based on this analysis, we assume that the calendar in use at Mari during the period of Old Assyrian ascendancy under Šamši-Adad I would have been a lunar calendar without intercalation. Even so, a Babylonian style intercalated lunar calendar has also been considered.

Within the limits of the predicted clock-time error, the list shows potential eclipses in addition to probable ones. While large magnitude solar eclipses are more likely to be seen, the sighting of lower magnitude eclipses under certain circumstances, such as at sunrise or sunset or in the middle of the day (if the right amount of cloud cover allows direct sighting of the solar disk under eclipse) cannot be precluded. Putative Venus solutions could be disputed if no corroboration was possible. The default acceleration of c = 32.5 was used in the calculations. The nearest candidates to the 1702 BC solution are the dawn eclipse (magnitude 0.85) on 18 February 1919 BC at Mari (34.55° North, 43.89° East) or at Ashur (34.5° North, 43.9° East) or the low magnitude (0.10) dawn eclipse on 12 May 1905 BC. The nearest eclipse candidates compatible with the 1646 BC, 1638 BC and 1630 BC solutions, are the 27 October 1858 BC eclipse (magnitude 0.89), the annular eclipse at dawn 24 March 1838 BC (magnitude 0.92) and the total eclipse of 24 June 1833 BC (magnitude 1.06) at Mari or at Ashur. The nearest candidate to the 1582 BC, 1574 BC, 1566 BC

VS Year 1 Ammiṣaduqa		Year 1 Hammurapi		Šamši -Adad Mari Eponym 126 Solar Eclipse			* Mag	Notes	Year 1 Ammiṣaduqa		Year 1 Hammurapi		* Diff
1702	**BC**	**1848**	**BC**	**12-May**	**1905**	**BC**	**0.10**	**Dawn**	**1705**	**BC**	**1851**	**BC**	**3#**
1646	BC	1792	BC	27-Oct	1858	BC	0.89	or 1838	1658	BC	1804	BC	12
1638	**BC**	**1784**	**BC**	**24-Mar**	**1838**	**BC**	**0.9**	**Dawn**	**1638**	**BC**	**1784**	**BC**	**0**
1630	BC	1776	BC	24-Mar	1838	BC	0.9	Dawn	1638	BC	1784	BC	8
1630	**BC**	**1776**	**BC**	**24-Jun**	**1833**	**BC**	**1.06**	**Total**	**1635**	**BC**	**1779**	**BC**	**3#**
1582	BC	1728	BC	27-Jul	1779	BC	0.46	or 1768	1579	BC	1725	BC	-3
1574	BC	1728	BC	27-Jul	1779	BC	0.46	or 1768	1579	BC	1725	BC	5
1574	BC	1720	BC	20-Dec	1768	BC	0.96	Dawn	1568	BC	1714	BC	-6
1566	***BC***	**1712**	**BC**	**20 Dec**	**1768**	**BC**	**0.96**	**Dawn**	**1568**	**BC**	**1714**	**BC**	**2#**
1550	BC	1696	BC	20-Dec	1768	BC	0.96	Dawn	1568	BC	1714	BC	18
1526	BC	1672	BC	09-Oct	1737	BC	0.91	Sunset	1537	BC	1683	BC	11
1518	BC	1664	BC	09-Oct	1737	BC	0.91	Sunset	1537	BC	1683	BC	19
1499	**BC**	**1645**	**BC**	**16-Apr**	**1699**	**BC**	**1.02**	**Total**	**1499**	**BC**	**1645**	**BC**	**0**
1483	**BC**	**1629**	**BC**	**11-Nov**	**1683**	**BC**	**0.86**	**Sunset**	**1483**	**BC**	**1629**	**BC**	**0**
1427	**BC**	**1573**	**BC**	**29-May**	**1627**	**BC**	**0.84**	**Dusk**	**1427**	**BC**	**1573**	**BC**	**0**
1419	BC	1565	BC	29-May	1627	BC	0.84	Dusk	1427	BC	1573	BC	8

$\Delta T = -20+32\ t^2$ (c=32, t =years until 1820/100)).

* Mag = magnitude of the eclipse, Diff = the difference between predicted Hammurapi year 1 or the required reduction of the eclipse date.

Poor Venus solutions.

Venus solution date estimated from the Eponyms 199-126 (73 lunar years about 70-71 years) for the age of Šamši-Adad and death in year 17 of Hammurapi.

Or potentially exact if a solar calendar was in use at Mari.

If Šamši-Adad died in year 10 of Hammurapi the compatible year 1 Hammurapi (B) and Ammiṣaduqa dates (B) would be moved down by 7 years.

Figure 4. MEC II. 25′–26′ (Šamši-Adad I) Solar Eclipse and Venus solutions.

and 1550 BC solutions is 27 July 1779 BC (magnitude 0.46) and the dawn solar eclipse on 20 December 1768 BC (magnitude 0.96). The nearest to 1526 BC and 1518 BC Venus solutions are the 26 May 1738 BC (magnitude 0.84) eclipse or more likely the near sunset annular solar eclipse of 9 October 1737 BC (magnitude 0.91).

The 1499 BC and 1483 BC solutions are compatible with solar eclipses on 16 April 1699 BC (magnitude 1.02), sunset eclipse 11 November 1683 BC (magnitude 0.86) or the sunrise eclipse 20 August 1678 BC (magnitude 0.45). The 1427 BC and 1419 BC Venus solutions were potentially matched by the annular solar eclipse midday 28 April 1635 BC (magnitude 0.95) or the near sunset eclipse 29 May 1627 BC (magnitude 0.84). The death of Šamši-Adad I is fixed to MEC II. 99 while year 1 Hammurapi can be fixed between 54 to 64 years (70 – 16 to 73 – 9) after the solar eclipse.

Figure 4 shows the best MEC II. 25′–26′ eclipse candidates closest to each of the Venus solutions, year 1 (A) of Ammiṣaduqa and Hammurapi according to the Venus solution or (B) as required by a Mari eponym eclipse. The MEC II. 25′–26′ eclipse candidates match the three Venus solutions in 1638 BC, 1483 BC and 1427 BC. If intercalations were used in the Mari calendar, solar eclipses would support the High 1702 BC, Lower Middle 1630 BC and Lower Short 1566 BC chronologies. However, if Šamši-Adad I died in year 10 Hammurapi, other solutions appear favourable: 1630 BC, 1427 BC and 1419 BC, while in addition 1646 BC and 1526 BC solutions are possible if a Babylonian-style calendar had been in use at Mari.

The most impressive solar eclipses over the period are the total eclipse of 24 June 1833 BC, the Middle Chronology solutions 1638 BC or 1630 BC, and the total eclipse of 16 April 1699 BC eclipse at Mari or Ashur. This requires

Ammiditana					Ammiṣaduqa				
2.XI	14.IX	27.VII	32.XII		1.VII	5.XII	12.VIII	14.VI	16.XII
2.XII	14.XII$_2$	27.XII$_2$	33.IX		1.VIII	6.VI	13.I	14.VIII	17.XI
4.VIII	24.I or V	29.II	34.VIII		1.XII	7.XII	13.II	15.II	17.XII
4.XII$_2$	24.IV	30.IV	34.IX		3.IV	11.II	13.VI	15.X	18.III
5.XII	24.VII	30.VII	36.I		3.VI	11.VII	13.X	15.XII	18.V
6.IV	24.VIII	31.II	36.II		4.XI	11.IX	13.XII	16.I	18.X
7.XII	26.VI	31.XII	36.XII		4.XII$_2$	12.IV	13.XII$_2$	*16.V*	19.III
14.III	26.IX	32.VIII	37.IV		5.VIII	12.VII	14.IV	16.XI	*19.X*

First Arabic number = year, Roman numerals = the Babylonian month, Year and Month underlined = last month of the year, Date in Italics = authenticity doubted and not used.

Figure 5. Attested 30-day months of Ammiditana and Ammiṣaduqa.

a longer period between the accession of Hammurapi and Ammiṣaduqa to match the nearest acceptable Venus solution in 1483 BC. Although, the internal chronology of the First Dynasty is well understood, there are major discrepancies with Babylonian King List 7 (BKL 'B'; Grayson 1980–1983, 100) that could imply flexibility in the dating of year 1 Hammurapi. BKL 7: 6 gives Hammurapi a reign of 55 years, while 43 years are supported by the list of eponyms.[5] The shorter reign is however contradicted by the number of intercalations, i.e. 18, 19 or 20 between years 3–40 (Langdon and Fotheringham 1928; Huber *et al.* 1982). One would expect some 14 or 15 intercalations in this period. There is a seven-year gap (years 3–10), and none are listed after year 40. A reign of 55 years would require at least 20 intercalations to keep the seasons in order while a 43-year reign would need 16 intercalations.

Compatible lunar eclipses in year 11/12 Hammurapi were observed with all the above Venus solutions (data not shown). But without a month date no more can be determined.

Tell Muhammad Lunar Eclipse(s)

While it is possible that two lunar eclipses are described, a putative month V (*Abu*) day 10 lunar eclipse is only possible if the so called *Nisannu* (month I) eclipse occurred in month XI (*Šabaṭu*). Hence, we can only be certain of the *Nisannu* eclipse, 38 years after the re-establishment of Babylon by the Kassites that is believed to have occurred during the late 15th century BC (van Koppen 2010). Consequently, this eclipse would have occurred in the early 14th century BC, i.e. three centuries later than the Long Chronology and about 85 years later than the early 15th-century BC Venus solutions. Sunset and near sunset lunar eclipses that are tenable for all Venus solutions between 1702 BC and 1483 BC in the months of *Šabaṭu* (XI), *Addaru* (XII) or *Nisannu* (I) are those of 12 January 1396 BC (XI), 18 May 1394 BC (I), 13 February 1380 BC (XI or XII), 18 May 1375 BC (I) or 16 March 1372 (XI–I) BC, all with a variable 'Dark Age' at Babylon as long as 250 years to as short as 30 years. Month V (*Abu)* lunar eclipses are possible on 18 July 1397 BC, 20 August 1381 BC, 9 August 1380 BC, 21 September 1373 BC or 10 September 1372 BC, respectively.

For the more drastic chronologies amounting to an offset of more than two centuries, the associated Venus solutions 1427 BC and 1419 BC would be suitable contenders. Likewise, the lunar eclipse on 18 March 1326 BC (XI.I4) would suit a month XI lunar eclipse with a month V (*Abu*) eclipse on 22 September 1327 BC or 11 September 1326 BC.

30-day Month Analysis

Several attempts have been made in support of different Venus solutions with the contemporary evidence of 30-day months attributable to Ammiṣaduqa (as mentioned above), but none support the Middle or Short Chronology. The 30-day month analysis favours either a Long Chronology date 1702 BC, as advocated by Sidersky (1940), Huber *et al.* (1982) and Huber (1987, 2017), or the unconventionally Late Chronology 1483 BC Venus solution, advocated by this author (Lappin 2013). Like with many of the earlier efforts, Lappin (2013) used a narrower range of dates than is currently available.

[5] See conveniently, https://cdli-gh.github.io/year-names/HTML/T12K6.htm.

Since the early 2000's the list of attested 30-day months has increased (*Figure 5*). Certain changes in the list have also occurred: i.e. at least 3 dates used by Weir (1982) and Lappin (2013) are currently in doubt. The year 1 month I date, as proposed by Weir (1982), is read as month VI. Rejected also are year 2 month XII (2.XII) and year 16.V, although I cannot find fault with the latter. The remaining 23 30-day months are supplemented by 15 or 16 further ones (38 to 40), plus 32 additional 30-day months ascribed to Ammiditana, as compiled by van Koppen and Roaf (Huber 2017). Even so and as far as one can see, only the 18 dates from year 26 to year 37 have been utilised for the analysis. This is due to the uncertainty about missing intercalations prior to year 22 and a questionable year 25.XII intercalation. Dates 16.V and 19.X are additions to the list in Huber (2017). Weir (1982) supported 16 V with cuneiform Text ii 48. Huber (2017) lists 19.X as a possible date but excludes it because either number 2 is overwritten by 30 or *vice versa*.

Attested intercalary months of Ammiṣaduqa are: 4.XII_2, 5.VI_2, 10.VI_2, 11.VI_2, 13.XII_2 with intercalations on 17.XII_2. 19 VI_2 or XII_2 20 VI_2 or XII_2, as is inferred by the Venus texts. Attested intercalary months of Ammiditana are as follows: 4.XII_2, 10.XII_2, 11.XII_2, 13.XII_2, 14.XII_2, 22.XII_2, 25.XII_2(?), 26.XII_2, 27.XII_2, 28.XII_2, 32.XII_2, 33.XII_2 and 37.XII_2 (Huber 2017). One or more intercalary months between years 15 and 22 are possible, as are an intercalary month (i.e. a second year 36.VI), or XII_2, or in year 1 of Ammiṣaduqa (i.e. a second Month VI).

A detailed analysis was undertaken to determine the best candidate years for the start of the Venus cycle attributed to Ammiṣaduqa. Intercalations were those described above for both Ammiṣaduqa and Ammiditana and are listed in *Figure 5*. The analysis of the 30-day months is also represented in *Figure 5*.

Errors in lunar sighting

Poor atmospheric conditions, the vantage point and the skill of observers, all have an impact on what was or was not seen. Whether the Babylonians were awaiting the sighting of the first crescent or had anticipated a crescent moon to start the new month, has a marked effect on the interpretation of the data. Years of observation would be required to determine a pattern in the lunar phases, because the synodic month averages 29.53 days and oscillates between 29.24 and 29.86 days. While 53.1% of the lunar months are 30 days, 46.9% are 29 days with < 0.1% 31 days long, the prediction of 30-day months is difficult and can easily fail.

A recorded 30-day month might result from what in the astronomical record is either an authentic 30-day month or an error imposed on a 29-day month with the result of a mismatch. Theoretically, a crescent moon could have been observed or imagined a day earlier than should be mathematically possible (hence starting the month early). It is also possible that a visible first crescent moon had not been observed until the following evening (hence ending the month late). Furthermore, an assumed first crescent moon, absent or present during a spell of poor weather, could convert a 29-day month into a 30-day month. The imposition of a statutory 30-day month would influence the sequence, which then would need to be disregarded during the analysis, see Huber *et al.* (1982) and Huber (2017).

Huber laid out some ground rules for considering the validity of recorded 30-day month sequences and mismatches within the sequence. After investigating Neo-Babylonian 30-day months, Huber estimated the expected and acceptable error for judging the various solutions for the 30-day month sequences (Huber *et al.* 1982; Huber 2017).

Lunar crescents have time to gain or lose altitude between 0.3°–0.5° in one hour, such that the perceptible duration between observations may be affected. Only slight changes in the measurable 30-day month sequences are seen with the current estimates of DT that accompany a particular Venus solution. The method for determining first crescent visibility varies and can give quite different results. The two methods used in this study were: (1) the required altitude depends on the azimuth difference (0°–23°) between the sun and moon – ranging between greater than or equal to 10.4°±1.1 to 4.9°±1.1, respectively, at sunset (Neugebauer 1929; Huber *et al.* 1982; Huber 2017); and (2) the moon's altitude is greater than or equal to 4.0° at the end of civil twilight (Anderlič and Firneis 2007). On the balance of current evidence, the preferred method was that of Neugebauer (1929) and Huber *et al.* (1982). The reasons are as follows: Doggett and Schaefer (1994) have shown that only 43% of the observers were able to see a thin crescent with less than 10° elevation because of large extinction and refraction effects for near horizon observations. This has a greater effect on waning crescent than on waxing crescent observation, but waxing crescents are harder to locate. The bias of observers also has an effect. Given all these considerations, 2 or 3 out of 12 crescent moons are wrongly determined and almost 15% of the observers have been shown to report false positive observations of the new moon (Wells 2002). The new crescent moon must usually be at least 17 hours old before it can be observed with the naked eye in the evening sky, but younger moons have been reported (Pepin 1996). In some cases, the new moon must be approximately 36 hours old to be observable.

It is assumed that ancient Babylonian observers would have looked for a crescent at dusk on day 29. If it was seen that evening then the old month was a 29-day month, while the new month started that evening. If it was not observed, the old month was a 30-day month and the month started the next evening. There would

have been occasions when a thin crescent would have been seen at a lower altitude than expected and the month started earlier than was calculated on other occasions, when a thin crescent was missed so that the month ended late. It is possible to emend the tally by identifying 29-day months (mismatches). These could be extended. Some mismatches are obviously wrong and could be attributed to poor weather conditions. This analysis has avoided modifying the totals by looking for near misses to enter them as matches.

New analysis of the 30-day months of Ammiṣaduqa and Ammiditana

An analysis was undertaken to determine the best candidate years for the start of the Venus cycles attributed to Ammiṣaduqa. Intercalations were those described above for the Venus solution analysis. In addition, the attested intercalations attributed to Ammiditana were also utilised to employ as many of the recorded 30-day months attributed to his reign as possible. The month-dates of Ammiditana prior to year 26 cannot be analysed directly with certainty because it is likely there was at least one unattested intercalation between year 15 and year 22, while the year 25 intercalation is suspect (Huber 2017). Huber (2017) likewise suggests that there is a missing intercalation either in year 36 of Ammiditana or early in year 1 of Ammiṣaduqa. The analysis takes these aspects into account (see below).

Theoretical considerations with the 30-day months

The mismatch rate corresponding to Neo-Babylonian control material (Huber *et al.* 1982) for a correct chronology is $p = 0.33$ (Huber 2017) while the theoretical mismatch rate for random wrong chronologies is $q = 0.47$.

With 38 (Ammiṣaduqa) and 32 (Ammiditana) recorded 30-day months, respective mismatches of 13 and 11 would be expected. With some 70 observations mismatches of some 23 would be expected.

However, this is a small data set and errors effects are magnified. 33 or less mismatches might suggest a viable chronology, while the mismatch probability $p < q = 0.47$ greater than 33 mismatches suggest that the chronology is wrong, where $p > q = 0.47$. The major limitations of this and any former study are the sample size and the lack of statistical power. Using a binomial distribution, Huber (2017) estimated that a sample size exceeding 60-month lengths would be required for Ammiṣaduqa alone when comparing four chronologies and a greater number when the month lengths of Ammiditana were included because of the uncertainties in the placement of intercalations. In the current study a sample size calculation assumed that type one statistical error = 5% and type 2 statistical error = 20%. Thus 80% statistical power indicated that 38-month lengths of Ammiṣaduqa would only just be sufficient with a binomial probability $p > 0.95$ (null hypothesis of no difference $p < 0.05$) for a single comparison between two chronologies. However, the current investigation has at least eleven potential Venus solutions under investigation, so even if the chronological issues with the lunar months of Ammiditana could be resolved and if they are to be combined with those of Ammiṣaduqa (i.e. arriving at the total number of 70 month-lengths), the study would still lack sufficient statistical power for the multiple comparisons required.

In addition, Huber (2017) reported that when he compared the lists of attested 30-day months from the First Dynasty with those from later Babylonian periods, twice the expected proportion of 30-day months occurred in the final month of the year during the First Dynasty period. Suspecting that Old Babylonian practice assigned 30 days to the last month, he proposed that the final months of the year should be removed from his analyses. The impact of this suggestion has been considered and the data were analysed before and after the exclusion.

Analysis of 70 30-day months

Concerning a preliminary analysis with 56 30-day months, ranging from year 26 Ammiditana to year 19 Ammiṣaduqa. In brief, depending on the presence or absence of an intercallation between year 36 Ammiditana and early year 1 Ammiṣaduqa, Venus solution 1483 BC has a greater number or equal number of matching 30-day months than the 1702 BC Venus solution. Regardless of an intercalation late in Ammiditana's or early in Ammiṣaduqa's reign, both of these solutions are superior to the putative solutions of the 17th–15th centuries BC (data not shown). However, many of the details of this analysis on several Venus solutions have been published previously by Roaf (forthcoming) and Huber (2017). The analysis used the 38 attested 30-day months from years 1–19 Ammiṣaduqa and 32 attested 30-day months from the reign of Ammiditana. In performing the analysis, the following issues were considered: (1) it is not certain whether one or two intercalations might have been placed between year 15 and prior to year 22; (2) the second month XII in the year 25 ($25.XII_2$) might not have existed; and (3) the possibility that an additional intercalation, either a second year 36 month XII (XII_2) or a second month VI (VI_2) early in year 1 of Ammiṣaduqa should be included. To investigate the impact of these considerations, 12 sequences of lunar months were determined.

Lunar month sequences

1) All attested intercalations or those inferred on the Venus tablets (known), including $25.XII_2$,

2) All known intercalations without $25.XII_2$,

Month/Year 1	Additional Intercalation (s) or presence of year 25 month XII_2											
1.VI_2 or 36.XII_2	0	0	0	0	0	0	1	1	1	1	1	1
25.XII_2	1	0	0	0	1	1	1	0	0	0	1	1
15-21	0	0	1 x	2 x	1 x	2 x	0	0	1 x	2 x	1 x	2 x
1702 BC	40	42	39	40	41	44	44	43	44	47	47	44
1646 BC	34	34	33	35	36	36	35	33	35	35	35	32
1638 BC	33	31	32	28	29	35	29	34	30	36	35	31
1630 BC	37	36	35	35	37	36	33	35	35	34	32	33
1582 BC	37	39	38	37	36	38	41	40	39	41	43	42
1574 BC	34	36	35	33	32	33	31	33	31	32	32	31
1566 BC	34	34	32	30	32	35	34	36	34	37	37	38
1550 BC	39	37	37	35	37	39	40	42	40	42	42	41
1526 BC	24	23	24	26	26	29	27	25	27	30	30	28
1518 BC	36	36	37	34	33	34	31	36	33	34	32	33
1483 BC	38	38	37	38	39	36	46	46	47	44	43	43
1428 BC	39	39	38	40	41	39	37	35	37	35	35	39
1419 BC	36	36	35	35	36	41	36	37	37	42	41	34

Analysis of months I to XI and XII_2, 30 day month tallies in each Venus solution.

1702 BC	35	35	34	34	35	37	36	36	36	38	38	36
1646 BC	27	26	26	27	28	29	25	24	25	26	26	23
1638 BC	26	25	25	22	23	27	24	28	25	29	28	27
1630 BC	30	30	28	28	30	29	27	29	29	28	26	28
1582 BC	31	31	32	31	30	31	35	34	33	34	36	36
1574 BC	24	28	25	25	24	27	26	26	26	29	29	25
1566 BC	27	26	25	25	27	27	28	28	28	28	28	31
1550 BC	30	30	28	29	31	31	34	33	34	34	34	34
1526 BC	21	20	21	21	21	23	22	22	22	24	24	23
1518 BC	29	28	30	27	26	27	24	29	26	27	25	27
1483 BC	34	34	33	33	34	33	39	40	40	39	38	37
1428 BC	33	33	32	34	35	32	31	29	31	28	28	32
1419 BC	29	29	28	28	29	32	30	31	31	34	33	29

Bold font = Acceptable tallies of 30 day months, regular font = unlikely matches with year start too early.

Figure 6. Confirmed 30-day months attested to Ammiṣaduqa and Ammiditana, $\Delta T = -20+32\ t^2$

The number of 30-day months in putative Venus solution for 12 lunar sequence variants. Analysis of months I–XI and XII_2, 30-day month tallies in each Venus solution.

3) All known intercalations without 25.XII_2 plus one intercalation in years 15–21,

4) All known intercalations without 25.XII_2 plus two intercalations in years 15–21,

5) All known intercalations, 25.XII_2 plus one intercalation in years 15–21,

6) All known intercalations, 25.XII_2 plus two intercalations in years 15–21,

7) All known intercalations plus intercalations 25.XII$_2$ and 36.XII$_2$,

8) All known intercalations plus 36.XII$_2$ but without 25.XII$_2$,

9) All known intercalations plus 36.XII$_2$, without 25.XII$_2$, plus one in years 15–21,

10) All known intercalations plus 36XII$_2$, without year 25.XII$_2$, plus two in years 15–21,

11) All known intercalations, plus 36.XII$_2$ and year 25.XII$_2$ plus one in years 15–21,

12) All known intercalations plus 36.XII$_2$ and year 25.XII$_2$ plus two in years 15–21.

Figure 6 shows an analysis using the lunar sighting criteria of Neugebauer (1929), Huber *et al.* (1982) and Huber (2017) with the formula for clock time (DT) correction by both Morrison and Stephenson (2004) and Huber (2017), with c set to 32.5 in the latter case. The top half of the table shows the sum of 30-day months for each lunar sequence. At present there is insuffient evidence to say which of the above models is likely to be correct.

The lower half of the table shows the effect of removing the final months from each year from the analysis: 15 final months, either XII or XII$_2$, which were excluded from sequences 1–6 listed above, and 14 from sequences 7–12, when the intercalation took place on 36.XII$_2$. Accordingly, 36.XII would no longer be the last month in year 36. Some chronologies are more susceptible to a larger deduction of matches than others and changes in the overall result can be observed. The highest number of matches and the lowest number of mismatches with acceptable mismatch probability scores less than q = 0.47 (in order of the highest number astronomically determined 30-day months to the lowest) was as follows: 1483 BC > 1702 BC > 1582 BC > 1550 BC > 1419 BC = 1427 BC. The Middle Chronologies' dates 1646 BC, 1638 BC, 1630 BC and the Short Chronology date 1574 BC resulted in high mismatch scores, exceeding p = 0.47. While this makes them less appealing candidates, it does not facilitate their exclusion.

Using the lunar sighting criteria of Anderlič and Firneis (2007), first crescent altitude exceeding 4° at the end of civil twilight produced different results where the highest number of matches were different for the Venus solutions (the full data set is not shown but was again presented at the Fourth BICANE Colloquium; see *Figure 7* for the highest scores attained using these criteria). In this analysis, the order of the highest number astronomically determined 30-day months to

Confirmed 30 day months at each c value								30 day months I to XI & XII$_1$ at each c value						
c	31.8	32	32.3	32.5	32.8	-20+32	F&A	31.8	32	32.3	32.5	32.8	-20+32	F&A
1702 BC	**45**	**46**	**46**	**46**	**48**	**47**	**48**	**39**	**38**	**38**	**38**	**40**	**38**	**38**
1646 BC	**37**	**37**	**36**	**36**	**36**	**36**	**36**	**30**	**30**	**29**	**29**	**30**	**29**	**29**
1638 BC	**36**	**36**	**35**	**36**	**36**	**37**	**37**	**28**	**29**	**28**	**29**	**28**	**30**	**30**
1630 BC	**37**	**37**	**37**	**37**	**35**	**37**	**41**	**30**	**30**	**31**	**30**	**29**	**30**	**33**
1582 BC	**43**	**43**	**43**	**43**	**43**	**43**	**40**	**37**	**37**	**37**	**36**	**36**	**36**	**31**
1574 BC	**37**	**36**	**36**	**36**	**37**	**36**	**38**	**29**	**28**	**26**	**26**	**29**	**28**	**29**
1566 BC	**37**	**37**	**37**	**36**	**36**	**37**	**41**	**28**	**28**	**28**	**27**	**27**	**28**	**32**
***1550* BC**	**42**	**43**	**43**	**43**	**42**	**42**	**45**	**33**	**35**	**35**	**35**	**34**	**34**	**36**
1526 BC	**27**	**28**	**28**	**28**	**27**	**27**	**44**	**23**	**24**	**24**	**22**	**22**	**21**	**35**
1518 BC	**37**	**37**	**37**	**36**	**36**	**37**	**43**	**30**	**30**	**30**	**29**	**29**	**30**	**35**
***1499* BC**	**46**	**46**	**47**	**47**	**47**	**47**	**41**	**36**	**36**	**38**	**38**	**37**	**36**	**32**
1483 BC	**48**	**47**	**47**	**46**	**48**	**47**	**44**	**40**	**40**	**40**	**39**	**41**	**40**	**38**
1428 BC	**39**	**41**	**41**	**42**	**42**	**41**	**41**	**32**	**35**	**35**	**36**	**36**	**35**	**34**
1419 BC	**40**	**41**	**41**	**42**	**42**	**42**	**42**	**32**	**33**	**33**	**35**	**35**	**35**	**35**

Bold font = Acceptable tallies of 30 day months, -20+32 = -20 + 32t^2, F&A = Firneis and Anderlič (2003) & -20 + 32t^2.

Figure 7. 30-day month tallies at different values of c and DT (ct^2) for 14 solutions

Venus	Confirmed 30 day months						30 day months I - XI & XII_1					
Solution	30 d	29 d	STD	p=	Rank	Prob	30 d	29 d	STD	p=	Rank	Prob
1702 BC	**48**	**22**	**3.9**	**0.31**	**1**	**0.331**	**40**	**16**	**3.4**	**0.29**	**2**	**0.280**
1646 BC	**37**	**33**	**4.2**	**0.47**	**13**	**0.001**	**30**	**26**	**3.7**	**0.46**	**12**	**0.001**
1638 BC	**37**	**33**	**4.2**	**0.47**	**13**	**0.001**	**30**	**26**	**3.7**	**0.46**	**12**	**0.001**
1630 BC	**41**	**29**	**4.1**	**0.41**	**10**	**0.005**	**33**	**23**	**3.7**	**0.41**	**10**	**0.005**
1582 BC	**43**	**27**	**4.1**	**0.39**	**6**	**0.018**	**37**	**19**	**3.5**	**0.34**	**4**	**0.048**
1574 BC	**38**	**32**	**4.2**	**0.46**	**12**	**0.001**	**29**	**27**	**3.7**	**0.48**	**14**	**0.001**
***1566* BC**	**40**	**30**	**4.1**	**0.42**	**11**	**0.003**	**31**	**25**	**3.7**	**0.45**	**11**	**0.001**
***1550* BC**	**45**	**25**	**4.0**	**0.36**	**4**	**0.057**	**36**	**20**	**3.6**	**0.36**	**5**	**0.027**
1526 BC	**44**	**26**	**4.0**	**0.37**	**5**	**0.032**	**35**	**21**	**3.6**	**0.38**	**7**	**0.015**
1518 BC	**43**	**27**	**4.1**	**0.39**	**6**	**0.018**	**34**	**22**	**3.6**	**0.39**	**9**	**0.008**
***1499* BC**	**47**	**23**	**3.9**	**0.32**	**3**	**0.184**	**38**	**18**	**3.7**	**0.33**	**3**	**0.086**
1483 BC	**48**	**22**	**3.9**	**0.31**	**1**	**0.331**	**41**	**15**	**3.3**	**0.27**	**1**	**0.503**
1428 BC	**42**	**28**	**4.1**	**0.40**	**8**	**0.010**	**36**	**20**	**3.6**	**0.36**	**5**	**0.027**
1419 BC	**42**	**28**	**4.1**	**0.40**	**8**	**0.010**	**35**	**21**	**3.6**	**0.38**	**7**	**0.015**
Correct	**Chr**	**23.1**	**3.9**	**0.33**	<22.29d	0.0059		**18.5**	**3.5**	**0.33**	<15.29d	0.0015
Wrong	**Chr**	**32.9**	**4.2**	**0.47**				**26.3**	**3.7**	**0.47**		

Italics Poor Venus solutions, **Bold** font = Acceptable tallies of 30 day months, d = days, STD = Standard deviation, p = probability, Prob = relative (posterior) probability, Chr = chronology.

Figure 8. Summary of the 30-day analysis showing highest score for each solution with ranking and probability analysis.

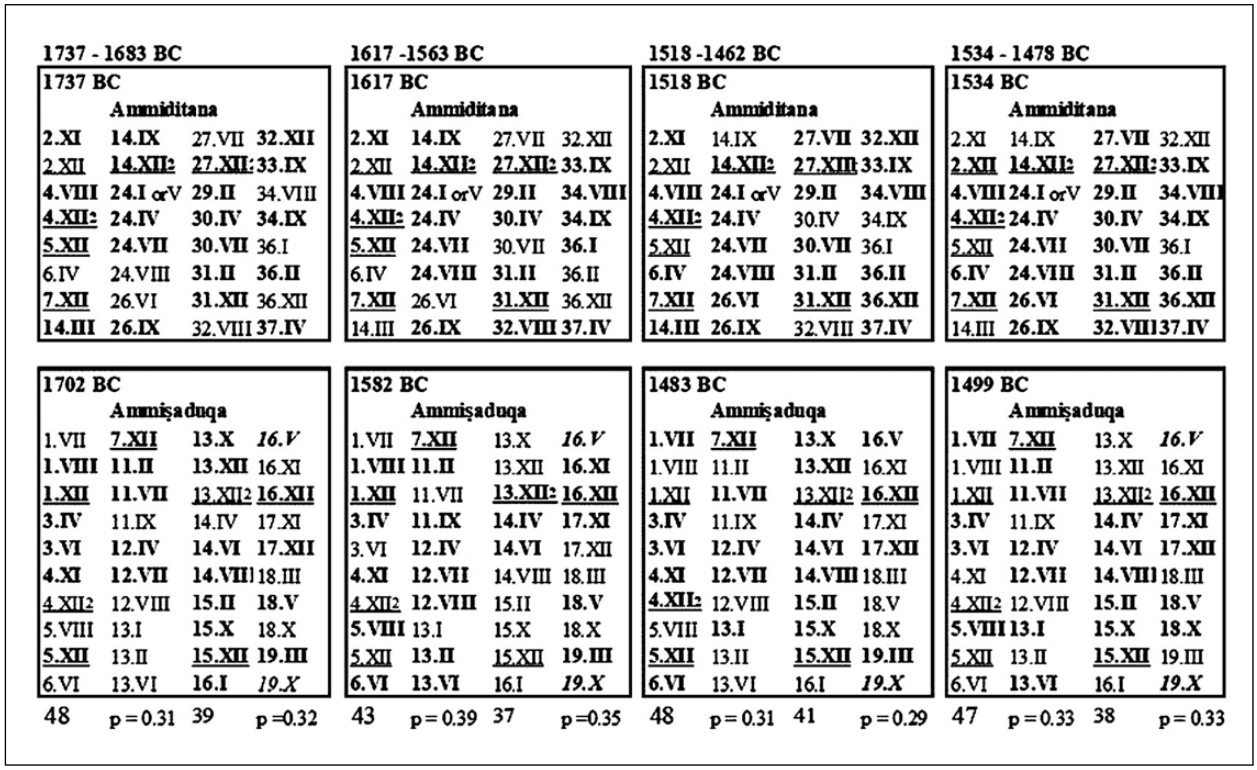
the lowest, the year 1 candidates were 1702 BC > 1550 BC > 1526 BC = 1483 BC > 1518 BC > 1419 BC> 1630 BC = 1566 BC = 1566 BC = 1499 BC = 1427 BC > 1582 BC. The Middle chronologies' dates 1646 BC, 1638 BC and the Low Short Chronology date 1574 BC have all high mismatch scores exceeding p = 0.47. When the final months of each year were excluded from the determination of 30-day months using the Anderlič and Firneis (2007) criteria, the highest tallies were found in the following order for solutions starting in 1702 BC = 1483 BC > 1550 BC > 1582 BC = 1427 BC >1630 BC. The 1550 BC 30-day month solution is not accompanied with a viable Venus solution or a MEC II. 25′–26′ solar eclipse, but it could provide a potential chronology if the Venus solution and the eclipse identification were to be ignored (see below).

The effect of altering the clock time error

A detailed analysis was carried out with different calculated clock-time errors, using Huber's formula (Huber *et al.* 1982, Huber 2017), DT = ct^2 which uses a range of acceleration factors (c) values from c = 31.8 to c = 32.8 with t = (years until 1800/100).

The alternate sequences listed above were used in the analysis and the best permissible sequence, those that did not commence too early in the year, were selected. The results obtained – using Neugebauer's first crescent visibility criteria – were also compared with the calculations of Morrison and Stephenson's clock-time error and with the first crescent calculations of Anderlič and Firneis (2007). In total, solutions were compared. The results shown in *Figure 7* are a summary of approximately 4500 first crescent determinations. The left side of table in *Figure 7* shows the highest tally of 30-day months concerning each solution shown for the range of acceleration factors used for the clock-time error calculations.

The highest number of confirmed 30-day months and the lowest mismatch scores are found when an intercalation is placed in 36.XII_2 for most of the viable chronologies, i.e. 1483 BC and 1702 BC, etc. Replacing the 25.XII_2 intercalation with one during the period of year 15 to year 21 appears to improve both the 1483 BC and 1702 BC chronologies (*Figure 7*). However, the difference is manifest also when the final months of each year are removed from the analysis as shown on the right side of *Figure 7*.



1737 - 1683 BC				1617 -1563 BC				1518 -1462 BC				1534 - 1478 BC			
1737 BC				1617 BC				1518 BC				1534 BC			
Ammiditana				Ammiditana				Ammiditana				Ammiditana			
2.XI	14.IX	27.VII	32.XII	2.XI	14.IX	27.VII	32.XII	2.XI	14.IX	27.VII	32.XII	2.XI	14.IX	27.VII	32.XII
2.XII	14.XII²	27.XII²	33.IX	2.XII	14.XII²	27.XII²	33.IX	2.XII	14.XII²	27.XII²	33.IX	2.XII	14.XII²	27.XII²	33.IX
4.VIII	24.I or V	29.II	34.VIII	4.VIII	24.I or V	29.II	34.VIII	4.VIII	24.I or V	29.II	34.VIII	4.VIII	24.I or V	29.II	34.VIII
4.XII²	24.IV	30.IV	34.IX	4.XII²	24.IV	30.IV	34.IX	4.XII²	24.IV	30.IV	34.IX	4.XII²	24.IV	30.IV	34.IX
5.XII	24.VII	30.VII	36.I	5.XII	24.VII	30.VII	36.I	5.XII	24.VII	30.VII	36.I	5.XII	24.VII	30.VII	36.I
6.IV	24.VIII	31.II	36.II	6.IV	24.VIII	31.II	36.II	6.IV	24.VIII	31.II	36.II	6.IV	24.VIII	31.II	36.II
7.XII	26.VI	31.XII	36.XII	7.XII	26.VI	31.XII	36.XII	7.XII	26.VI	31.XII	36.XII	7.XII	26.VI	31.XII	36.XII
14.III	26.IX	32.VIII	37.IV	14.III	26.IX	32.VIII	37.IV	14.III	26.IX	32.VIII	37.IV	14.III	26.IX	32.VIII	37.IV

1702 BC				1582 BC				1483 BC				1499 BC			
Ammiṣaduqa				Ammiṣaduqa				Ammiṣaduqa				Ammiṣaduqa			
1.VII	7.XII	13.X	*16.V*	1.VII	7.XII	13.X	*16.V*	1.VII	7.XII	13.X	16.V	1.VII	7.XII	13.X	*16.V*
1.VIII	11.II	13.XII	16.XI	1.VIII	11.II	13.XII	16.XI	1.VIII	11.II	13.XII	16.XI	1.VIII	11.II	13.XII	16.XI
1.XII	11.VII	13.XII²	16.XII	1.XII	11.VII	13.XII²	16.XII	1.XII	11.VII	13.XII²	16.XII	1.XII	11.VII	13.XII²	16.XII
3.IV	11.IX	14.IV	17.XI	3.IV	11.IX	14.IV	17.XI	3.IV	11.IX	14.IV	17.XI	3.IV	11.IX	14.IV	17.XI
3.VI	12.IV	14.VI	17.XII	3.VI	12.IV	14.VI	17.XII	3.VI	12.IV	14.VI	17.XII	3.VI	12.IV	14.VI	17.XII
4.XI	12.VII	14.VIII	18.III	4.XI	12.VII	14.VIII	18.III	4.XI	12.VII	14.VIII	18.III	4.XI	12.VII	14.VIII	18.III
4.XII²	12.VIII	15.II	18.V	4.XII²	12.VIII	15.II	18.V	4.XII²	12.VIII	15.II	18.V	4.XII²	12.VIII	15.II	18.V
5.VIII	13.I	15.X	18.X	5.VIII	13.I	15.X	18.X	5.VIII	13.I	15.X	18.X	5.VIII	13.I	15.X	18.X
5.XII	13.II	15.XII	19.III	5.XII	13.II	15.XII	19.III	5.XII	13.II	15.XII	19.III	5.XII	13.II	15.XII	19.III
6.VI	13.VI	16.I	*19.X*	6.VI	13.VI	16.I	*19.X*	6.VI	13.VI	16.I	*19.X*	6.VI	13.VI	16.I	*19.X*
48	p = 0.31	39	p =0.32	43	p = 0.39	37	p =0.35	48	p = 0.31	41	p = 0.29	47	p = 0.33	38	p = 0.33

Figure 9. Identity of 30-day and 29-day months in the three highest ranking Venus solutions and 30-day month solution with the MEC II. 25′–26′ total solar eclipse (16 April 1699 BC).

In most cases it appears to make little difference whether the final months are excluded from the highest scoring chronologies regarding mismatch rates, suggesting that their exclusion from the calculation is debatable.

Figure 8 shows a summary of the best achievable results with each Venus solution within the sample size limitations of the study. For the analysis in *Figure 8* it has been assumed that the month lengths of both kings can be combined. On the left side of *Figure 8* the tally of 30-day months lists the number of mismatches (as 29-day months) with the standard error. The mismatch probability is also shown for each Venus solution. Both the rank and the relative probability that the chronology is correct, is included below. The binomial analysis shows the number of mismatches expected by the correct and wrong chronologies and the relative probability of achieving a lower number of mismatches than the best represented chronology.

In the analysis of 30-day months, the highest number of 30-day months was found with the 1702 BC and 1483 BC Venus solutions, while the highest tally of 48 confirmed 30-day months with 22 mismatches and the probability of achieving less than 22 mismatches is p = 0.0059. After the removal of the final months of each year from the analysis, as shown on the right side of *Figure 8*, the 1483 BC Venus solution reached the highest tally of 41 confirmed 30-day months and 15 mismatches and was ranked first. The probability of achieving less than 15 mismatches was p = 0.0015 (*Figure 8*).

The relative probability that any of the solutions is the correct chronology of the 12 contenders was determined, prior to and after the removal of the final month of the year. The probability that either 1702 BC or 1483 BC are the correct solutions were p = 0.331 and about 6 times the probability that the third ranked solution 1550 BC (p = 0.057) was the correct chronology. After the last month of the years were removed the analysis showed that 1483 BC (p = 0.503) was almost twice as likely as 1702 BC (p = 0.280) or 10 times more likely than the third ranked Venus solution 1582 BC (p = 0.048), number four in the ranking. A typical sequence of 30-day months for the three highest scoring Venus solutions and the 1499 BC = year-1 solution is shown in *Figure 9*. Matches are shown in bold text, mismatches in regular text and the final months in given years are underlined. An additional two 30-day months might be added to the 1582 BC, 1499 BC and 1483 BC solutions, if both 16.V and 19.X are included, while in the tally only one addition is possible in the case of the 1702 BC Venus solution. It goes without saying, that the 1483 BC solution is highly compatible with the *CoD*-chronology presented by James *et al.* (1991 and in this volume) and by van der Veen and Zerbst (2013, 2022), supporting a lowering of the standard chronology by one and a half centuries or more during the second millennium BC.

Analysis of 30-day months without the Venus solution

Working with the basic sequences of 30-day months as determined above for putative year 1 Ammiṣaduqa dates between 1705 BC and 1406 BC of the 300 potential candidates for year 1, no less than 118 candidates had less than 33 mismatches each $p < 0.47$, while only 13 had less than 24 mismatches $p < 0.33$ and are as follows: 1702 BC (Venus solution), 1678 BC, 1676 BC, 1623 BC, 1580 BC, 1570 BC, 1560 BC, 1547 BC, 1499 BC, 1483 BC (Venus solution), 1444 BC, 1438 BC and 1421 BC. Regardless of the two Venus solutions discussed above, if year 1 Ammiṣaduqa was in 1580 BC (with year 1 Hammurapi in 1726 BC), it could still be compatible with a partial (Šamši-Adad) solar eclipse on 27 July 1779 BC. If Ammiṣaduqa year 1 was in 1570 BC (with year 1 Hammurapi in 1716 BC), it would be compatible with a partial (Šamši-Adad) eclipse on 20 Dec 1768 BC. If 1499 BC was Ammiṣaduqa year 1 (with year 1 Hammurapi in 1645 BC), it would be an exact match with a total (Šamši-Adad) solar eclipse on 16 April 1699 BC. If year 1 Ammiṣaduqa was in 1438 BC (with year 1 Hammurapi in 1584 BC), it would be virtually compatible with an annular (Šamši-Adad) eclipse on 28 April 1635 BC.

Enūma Anu Enlil omen texts

EAE also contains details of lunar eclipses and crucially solar eclipses associated with omens, presumably attributed to earlier kings from Akkad and other Bronze Age Mesopotamian kingdoms. The tablets report several lunar eclipses that are always seen on day 14 of the month and solar eclipses that always occur on day 28 (Huber *et al.* 1982; Huber 2017).

Once we have accepted a particular Venus solution, solar or lunar eclipses can be located that allow precise dating of the associated omens.

Long Chronology dates have been explored by Huber *et al.* (1982), Huber (1987, 2000, 2006), Middle Chronology dates by Huber (1987, 2000, 2006), Khalisi (2020), Short Chronology dates by Banjević (2006), Huber (1987, 2000, 2006), Ultra Short Chronology dates by Gasche *et al.* (1998), Gurzadyan (2000), Gurzadyan and Warburton (2006), Huber (2006) and Lowest Chronology dates by Lappin (2013). Still lower dates were suggested by Mitchell in line with the revised chronology of Rohl (Mitchell 1989/1990), but Mitchell no longer accepts his former conclusions (pers. comm. with the editors).

A particularly pertinent solar and lunar eclipse combination is explicitly associated with Babylon: i.e. the *EAE* Tablet 20 Šabaṭu (month XI) eclipse pair. It is usually argued that the lunar eclipse preceded the solar eclipse in the same month, but it cannot be excluded that the observations were in reversed order, as suggested by Lappin (2013) and Khalisi (2020). It is generally believed to have occurred late during the reign of Samsuditana. However, there is no definite proof of this. Furthermore, none of the eclipse pairs over the relevant period are particularly convincing (data not shown). Alternatively, it may be suggested that the 20.XI omen, associated with the conquest of Babylon and the death of a king, is related to Tukulti-Ninurta I and the death of Kaštiliašu IV, while it is also possible that it refers to the conquest of Babylon by Shutruk-Nahhunte I of Elam when the last Kassite king Zababa-šuma-iddina was deposed.

Conclusion

The highest tally of 30-day months of Ammiṣaduqa and Ammiditana was found with the 1483 BC and 1702 BC Venus solutions and 11 other 30-day month solutions with less than 24 mismatches. Errors with these solutions are consistent with Neo-Babylonian error rates $p = 0.33$. While in the 1483 BC chronology a late year start in years 5 and 7 might make this chronology less appealing, one could argue that this conjecture is based on Neo-Babylonian practice to limit the period between the spring equinox and the new year's start. It is more likely that intercalations were dictated in a haphazard way and were frequently implemented in clusters of 3 or 4 with large lacunae in between. For they do not appear to be incorporated in an obvious systematic fashion, as became the practice in later times (Parker and Dubberstein 1956). The large surplus of intercalations ascribed to Hammurapi remains a conundrum.

For the 1483 BC solution a good candidate for a MEC II. 25′–26′ (Šamši-Adad I) solar eclipse on 11 November 1683 BC exists. The match with the 1702 BC Venus solution is less certain (*Figure 2*). These occur in close agreement with current estimates for the Earth's deceleration.

While at present these results are not supported by current radiocarbon and dendrochronology dates, problems with these systems have been highlighted by Zerbst and van der Veen (2015, and elsewhere in these Proceedings, pp. 373–399). If the Venus tablets were not a contemporaneous record of the Venus cycle, the most likely – albeit slightly higher – candidate would be 1499 BC = year 1 Ammiṣaduqa (47 30-day months). Yet, this candidate is an ill-fitting Venus solution, as it would place year 1 Hammurapi in 1645 BC or 1646 BC aligning with a Venus solution compatible with a Šamši-Adad I total solar eclipse on 16 April 1699 BC.

In closing, if the 1483 BC Venus solution suggested in this article stands up to testing, it will have a significant bearing on the revised dates for the Middle and Late Bronze Ages suggested by some of the authors in this volume. Consequently, astronomical dating would no longer be an insurmountable obstacle for these scenarios.

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Ancient DNA: The Solution to Everything or the Devil's Work?

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This paper considers the potential significance and current situation regarding the use of ancient DNA (aDNA) for chronological purposes in the Central and Eastern Mediterranean and Near East from c. 2000 – 1 BC. The identification of biological relatives, their role in assessing the accuracy of radiocarbon dating, as well as the development of aDNA technology and results generally and in the area of study, are considered. The current situation in terms of the number of sites and chronological distribution is summarised. Some cautionary tales from prehistoric Europe are provided. Finally, specific observations and debates from the study area are outlined and the value of the available data assessed.

Introduction

The title of this paper derives from a comment by Philipp Stockhammer, the ancient Aegeanist, in a *Nature* article (Callaway 2018, 574) on the transition from the Neolithic to the Bronze Age in Europe and the impact of ancient DNA, in which Stockhammer states, perhaps tongue in cheek, that '[h]alf the archaeologists think ancient DNA can solve everything. The other half think ancient DNA is the devil's work'. This reveals something of the dichotomy facing archaeologists concerning this new source of evidence, particularly from the perspective of the tendency to see new scientific techniques as providing definitive evidence which replaces the need for existing methods (see e.g. Kristiansen 2022 and his identification of a third scientific revolution, versus Heyd 2017, or Maran 2022 and Kampourakis 2023, 176–192 on Mycenaeans and Minoans and genetic identity). The primary attraction of ancient DNA (aDNA) is that it offers the prospect of approaching the questions of migration and chronology from a fresh angle, but how best to do this is an open question. This paper is an archaeologist's perspective on aDNA, joining those of Anthony (2023) and Booth (2019), rather than an in-depth exploration of genetics.

Mitochondrial and Nuclear DNA

In terms of genetics it is crucial to note the differences between mitochondrial and nuclear DNA. Mitochondrial DNA (mtDNA) is located in the mitochondria, tiny organelles within cells that are the sites of energy production and contain 100–1000 copies per cell, while nuclear DNA is located within a cell's nucleus and usually has just two copies; therefore much of early DNA research concentrated on mtDNA, as it was easier to recover (as noted by Diroma *et al.* 2021 in their study of Iron Age Polizello on Sicily). On the other hand, mitochondrial DNA contains just 37 genes, while nuclear DNA contains 20,000 protein coding genes and an unknown number of ribonucleic acid (RNA) genes, so it allows for far more precise groupings. Also, the mitochondrial mode of inheritance is strictly maternal, whereas nuclear genomes (X- and Y-chromosomes) are inherited equally from both parents. Archaeologically this is significant, as female and male patterns of movement may be quite different, e.g. in the Viking Age occupation of Iceland (Ebenesersdóttir 2018).

Forensic Research

Initial work in the employment of DNA for population history studies developed out of its early use in specific forensic cases (Jeffreys *et al.* 1985), with methodological developments in terms of reliability and decreasing cost leading to the scaling up of analyses allowing for population scale, from global, such as the Human Genome Project (Green *et al.* 2015) or the 1000 Genomes Project (Poznik *et al.* 2016), to national/regional, such as the People of the British Isles project (Bodmer 2015; Leslie *et al.* 2015). Although modern DNA samples are clearly more straightforward than ancient DNA, in terms of being able to extract a viable sample which could be placed within a specific descent line (female or male), termed a haplogroup, the obvious problem with most modern DNA studies was that it was not

possible to distinguish between population movements occurring at different times. Thus, although the People of the British Isles project did identify clear differences in haplogroup frequencies across Britain, attempting to tie these to major population movements at specific times in the past involved a great deal of speculation (Leslie *et al.* 2015).

Following the demonstration of the feasibility of recovering ancient DNA (although see below) through Pääbo's experiments on Egyptian mummies (1985), improvements in sampling (Kontopoulos *et al.* 2020), in the avoidance of contamination and in analytical methods (Rizzi *et al.* 2012), have led to a spectacular increase in available data from the first reliable results in 2010 (Rasmussen *et al.* 2010). In 2023 the number of results passed the 10,000 figure (Callaway 2023).

One valuable use of aDNA follows on from a common use in forensics – the identification of relatives. This has particularly been a focus in Egypt, but its use is certainly not limited to there. One particular study undertaken was of the royal family of the late 18th Dynasty (Hawass *et al.* 2010; Habicht *et al.* 2016; Gad *et al.* 2020; Brier 2022, 156–164), for obvious reasons. The possibilities for checking relationships between known individuals (where they can convincingly be identified) are clear. It is worth noting that relatives have been identified in other genetic studies, without this being the primary purpose of the study, e.g. pairings have been noted at Pylos in Greece (Lazaridis *et al.* 2022a, 2022b), at Beirut in Lebanon (Haber *et al.* 2020; discussed below), at Deir Rifeh in Egypt (Drosou *et al.* 2018) and at Pian Sultano in Italy (Moots *et al.* 2023), while several cases have been found at Armenoi on Crete (Foody 2021, 64–65), at Megiddo in Israel (Agranat-Tamir *et al.* 2020) and at Alalakh in Turkey (Ingman *et al.* 2021). As the database grows, the possibility of chance connections increases, as happened in the largest-scale Viking DNA study undertaken so far (Margaryan *et al.* 2020), where two men, one buried in a mass grave in Oxford and the other from the Galgedil cemetery on the island of Funen in the middle of Denmark, were shown to be second degree relatives, i.e. half-brothers, uncle and nephew or grandfather and grandson. Building up such patterns could certainly provide valuable chronological information, the opportunities for which are enhanced by the development of new methods for identifying relatives, including those with more distant relationships to the third and fourth degree (Monroy Kuhn *et al.* 2018; Vai *et al.* 2020; Ringbauer *et al.* 2021; Popli *et al.* 2023; Lefeuvre *et al.* 2024; Ringbauer *et al.* 2024).

Testing Radiocarbon Dating

A further avenue for chronological use of aDNA is as a verification check on the accuracy of radiocarbon dating – if relatives can be identified, this acts as a constraint on the radiocarbon dating of the skeletons (Sedig *et al.* 2021; Massy *et al.* 2022). For an example, we turn to the Copper Age (Chalcolithic) in Britain: two Beaker culture burials, Amesbury and Porton Downs, separated by approximately 5kms had been radiocarbon dated, and the dates did not seem implausible, at 2480–2280 cal BC (95% probability) for the Amesbury Down man and 2140–1940 cal BC (95% probability) for the Porton Down woman (Sedig *et al.* 2021), although this would make the Amesbury Down burial one of the earliest Beaker burials. However, aDNA analysis revealed that the Amesbury Down man was the father of the Porton Down woman (due to them sharing 50 % of their autosomal genomes and with differing mtDNA haplotypes); the minimum date of death separation between the father and daughter was 140 years, which comfortably exceeds the maximum biological lifespan estimates. The father was therefore redated (to 2200–2031 cal BC at 95% probability) and the new date fits within the expected date of death separation range (Sedig *et al.* 2021). In the same publication the authors noted that of the 203 pairs of related individuals in their database, five had gaps which were too large to be feasible, thus reminding us of the possibility of radiocarbon dating error. A similar approach was taken to develop a chronological model for the Pocklington Iron Age cemetery in Yorkshire, UK, where the radiocarbon dating of a brother and sister was used to refine the model (Hamilton and Adams 2023).

Population Movements in the Near East

The use of DNA evidence to throw new light on the question of population movements in the transition from the Bronze Age to Iron Age in the Eastern Mediterranean and Near East is the most obvious use of aDNA relevant to the topic of these proceedings. From the perspective of those interested in events at the end of the Late Bronze Age and into the Early Iron Age, population movements are a crucial issue, as they have long been proposed (especially in the form of the 'Sea Peoples') as explanations for supposed gaps in the archaeological and historical record, or for apparent changes in material culture and practices (on this topic, see also in this volume: Millek on pp. 205–217 and James on pp. 219–248). The issue of the existence of the Sea Peoples as a factor in the changes at the end of the Bronze Age is much debated, with groups both in favour (e.g. Leriou 2011; Fischer and Bürge 2017; Mumford 2018; Cline 2021, 2022; Yasur-Landau 2022) and entirely against (e.g. Middleton 2015; Knapp 2021; Matić 2023; Parmenter 2024), while others argue for a greater complexity in the nature of migration and population movements (Maeir 2022a, 2022b). Unsurprisingly, the suggestion has been made that aDNA could provide useful information here (Chyleński *et al.* 2018).

The Concern of Contamination

Concerns, encouraged by Pääbo's results (1985) later being recognised as contaminated, that climatic conditions in the Near East (e.g. in Egypt) would not allow the survival of ancient DNA, (e.g. Marota *et al.* 2002; Gilbert *et al.* 2005; Lorenzen and Willerslev 2010), are now generally considered to be overly pessimistic (Zink and Nerlich 2003, 2005; Loreille *et al.* 2018; Gad *et al.* 2020) with the identification of the petrous bone at the base of the skull as showing the best survival of DNA (but not the only part of the body which can be used – Parker *et al.* 2020) being crucial here, although survival of aDNA outside tombs may be more problematic. Ancient DNA has also successfully been recovered from burnt remains at Pompeii (Scorrano *et al.* 2022) and in forensic cases (Emery *et al.* 2024), raising the possibility that ancient cremations undertaken at lower temperatures than modern-day practices may retain some DNA. It is also worth noting that this growing wave of genetic data is a development that has occurred without input from the vast majority of archaeologists and ancient historians working in the Eastern Mediterranean and Near East (partly because many of the results are published in scientific journals focusing on genetics). Exceptions include some of the articles and books cited above relating to the Sea Peoples debate.

The Current aDNA Picture

Turning to the specific issue of aDNA and population movements from the Bronze Age to Iron Age in the Eastern Mediterranean and Near East, although there has been a significant increase in the available data, this increase is by no means evenly distributed geographically. One difficulty is that the evidence is rarely gathered together, so there may well be gaps in any specific publication, which will often focus on new data. There are, however, a number of databases, such as the Ancient mtDNA database (https://amtdb.org/; Ehler *et al.* 2019) for the female line, the aYChr-DB for the male line (GitHub - eelhaik/aYDB: aYChr-DB: a database of ancient human Y haplogroups; Freeman *et al.* 2020) and the broader Allen Ancient DNA resource (https://reich.hms.harvard.edu/allen-ancient-dna-resource-aadr-downloadable-genotypes-present-day-and-ancient-dna-data; Mallick *et al.* 2024). It is also the case that a significant proportion of the aDNA sequences are still quite low coverage, i.e. they lack a degree of detail (Marciniak and Perry 2017) thus limiting their value, although this can to some extent be ameliorated by later work on the same individuals (e.g. Ingman *et al.* 2021 re-examining one of the Alalakh individuals first reported by Skourtanioti *et al.* 2020). A further crucial observation is that the work of aDNA as a whole has so far been dominated by Central and Northern Europe and by studies targeting the Neolithic, especially examining the spread of the Neolithic and the movement from the steppes westwards at the end of the Neolithic and in the Copper Age (Freeman *et al.* 2020).

The data summarised here comes from the later part of the Bronze Age over most of the area considered, i.e. after 2000 BC, and before the Hellenistic era (during which major changes in population occurred, e.g. Tepgeç and Görgülü 2022) in the East and the rise of Rome in the West, so a period of *c.* 1700–1800 years duration. Considering the Bronze Age as a single unit has understandably been the case in initial aDNA analyses for particular areas with a small sample size (e.g. Agranat-Tamir *et al.* 2020), but given that the Bronze Age of Israel, for example, is currently believed to have commenced *c.* 3500 BC (Nigro *et al.* 2019), it is the Middle to Late Bronze Age which is really relevant.

Country/ Region	Number of samples	Number of sites
Iran	37	4
Turkey	95	11
Syria	8	1
Jordan	21	1
Lebanon	22	3
Israel	51	5
Egypt	61	5+
Sudan	1	1
North Africa	13	2
Crete	104	7
Other Mediterranean Islands	9	2
Greece	61	13
Italy	245	30
Sardinia	81	20

Figure 1. Number of successful samples (both mitochondrial and nuclear DNA) and the number of sites from which these came. The number of sites in Egypt is uncertain, as two of the samples came from mummies of unknown provenience currently held in the Pushkin State Museum of Fine Arts, Moscow (Yatsishina et al. *2020) – they may thus have come from a site sampled by others, or one or two additional sites.*

Figure 1 presents, to the best of my knowledge, the current situation in terms of the data available

by country/region and period, including both mitochondrial and nuclear DNA, and all levels of coverage in terms of genetic detail. A significant issue, which, however, should resolve itself to some degree over time, is that the sampling is clearly uneven, with some countries such as Sudan and Syria having very few results so far, and the eight from Syria being derived entirely from Ebla (Skourtanioti *et al.* 2020). Within the Mediterranean islands (excluding Sardinia, for which the coverage is excellent, and Crete, also noted separately) coverage is poor at present, with results only from Paros and Aegina (Skourtanioti *et al.* 2023). Unfortunately, the only aDNA study so far undertaken on Malta (Ariano *et al.* 2022) was on Neolithic burials of the mid-3rd millennium BC from the Xaghra Circle on Gozo. Even for countries with somewhat larger numbers, these may be unrepresentative of the broader national pattern: in Jordan, for example, all 21 samples come from Jebel al-Qeşīr Cave B3, in the Baḳah Valley, some 20km northwest of Amman (Agranat-Tamir *et al.* 2020). Similar, if not so unrepresentative, are the situations in Iran, Lebanon and Israel.

Country/ Region	Bronze Age (after 2000 BC): number of samples	Bronze Age: number of sites
Iran	21	2
Turkey	54	4
Syria	8	1
Jordan	21	1
Lebanon	7	3
Israel	42	4
Egypt	19	4
Sudan	0	0
North Africa	0	0
Crete	104	7
Other Mediterranean Islands	9	2
Greece	56	13
Italy	39	12
Sardinia	43	16

Figure 2. Number of successful samples (both mitochondrial and nuclear DNA) of Bronze Age date and the number of sites from which these came.

Considering the Bronze Age (after 2000 BC) separately (see *Figure 2*), there are also striking differences within the second millennium BC data. Within both North Africa (other than in Egypt) and Sudan there is a complete lack of Bronze Age results. This makes assessing the data from Egypt itself more difficult in terms of using the aDNA evidence to consider population movements into Egypt (Vicente and Schlebusch 2022) – with a comparative sample only available from regions to the North the origin of any such movements is difficult to locate. In North Africa, the foci of research interest have been in whether Neolithic developments relate to population movements, perhaps across the Mediterranean (Fregel 2022; Simões *et al.* 2023), and in the nature of Phoenician population movement in the Iron Age (Moots *et al.* 2023).

Moreover, there are biases towards individual sites within countries: in Iran, although there are 21 individual aDNA sequences, these come from just two sites some 25kms apart - Hasanlu and Dinkha Tepe (Lazaridis *et al.* 2022a), although they are in the northwest part of Iran, so closest to the area considered here. Such biases are also acute for Israel, with 34 of the 42 results derived from the single site of Megiddo (Agranat-Tamir *et al.* 2020). For Turkey, the range of sites has increased in the last few years, but it is still the case that 39 of the 54 Bronze Age results (Skourtanioti *et al.* 2020; Ingman *et al.* 2021) come from Alalakh (Tell Atchana). The situation is somewhat similar in Crete, with the Ayios Charalambos cave providing 37 samples (Hughey *et al.* 2013), the Armenoi cemetery another 31 samples (Foody 2021; Richards *et al.* 2022) and tombs at Chania contributing 24 samples (Skourtanioti *et al.* 2023), thus three sites represent 90% of the total. The results are more widely distributed in Greece, with the site contributing the most to the 56 results being Pylos with 11 (Lazaridis *et al.* 2022a; Lazaridis *et al.* 2022b).

Considering the general picture for the Iron Age (see *Figure 3*), there are clearly countries and areas which are particularly poorly represented in comparison to the Bronze Age. The most striking case is that of Crete, where the 104 individual sequences from the Bronze Age contrast dramatically with a complete lack of data for the Iron Age, a situation resulting from the research questions being considered. Jordan also provides an absence of evidence from the Iron Age, against 21 samples (albeit from a single location) from the Bronze Age. Here it is unfortunate that Jebel al-Qeşīr Cave A4, which apparently dates to the Early Iron Age, was not sampled, even though the skeletal remains should be available (Agranat-Tamir *et al.* 2020), as this would have enabled a direct site comparison with the Bronze Age results. The situation in Greece is not quite so dramatic, but five samples for the Iron Age, three of which come from a reused tomb and settlement activity at Kastrouli (Lazarides *et al.* 2022a), is clearly inadequate to set against the 56 sequences from the Bronze Age.

Country/ Region	Iron Age: number of samples	Iron Age: number of sites
Iran	16	4
Turkey	41	7
Syria	0	0
Jordan	0	0
Lebanon	15	1
Israel	9	3
Egypt	42	3/4
Sudan	1	1
North Africa	13	2
Crete	0	0
Other Mediterranean Islands	0	0
Greece	5	3
Italy	206	23
Sardinia	38	8

Figure 3. Number of successful samples (both mitochondrial and nuclear DNA) of Iron Age date and the number of sites from which these came. The number of sites in Egypt is uncertain, as two of the samples came from mummies of unknown provenience currently held in the Pushkin State Museum of Fine Arts, Moscow (Yatsishina et al. 2020) – they may thus have come from one or two different sites.

Similarly, in Israel, there are 42 results from the Bronze Age (dominated by those from Megiddo, as noted above), but only nine from the Iron Age, with seven of these derived from Ashkelon (Feldman *et al.* 2019).

As was the case for the Bronze Age, there are cases where the data is dominated by a single site, with 12 of 13 results from North Africa provided by four tombs from the Punic settlement at Kerkouene on the coast of Tunisia (Moots *et al.* 2023), and all 15 from Lebanon come from sites within modern Beirut (Matisoo-Smith *et al.* 2018; Haber *et al.* 2020). Although the size of the aDNA sample for Iron Age Egypt is double that of the Bronze Age, 39 of the 42 results derive from mummies found at the single site of Abusir el-Meleq (Schuenemann *et al.* 2017). In other cases, such as Turkey, there have been significant improvements in the 2020s, with the number of results increasing from 12 (mtDNA only) in 2018 (all from Çemialo Sırtı in South-East Turkey – Yaka *et al.* 2018), to 41 from seven different sites across Turkey today (Lazarides *et al.* 2022a). The best data by far comes from Italy, including Sicily, with over 200 sequences (compared with 39 from the Bronze Age), which therefore provides the possibility that aDNA can be linked to population movements from the western to the Eastern Mediterranean (perhaps in the specific form of the Sea Peoples, e.g. Weiner 2017) once sufficient data in the eastern area is available.

aDNA and Population Movements – Cautionary Lessons from Prehistoric Europe

Any archaeologist or ancient historian concerned with the contribution aDNA may offer for the study of ancient population movements needs to take notice of some of the cautionary tales from prehistoric Europe of the dangers of filling in the gaps in the aDNA data too confidently, only to be shown to have been promoting over-simplified interpretations as further results were reported. Initial results from mtDNA suggested that there was a dramatic change across most of Central and northern Europe at the beginning of the Neolithic (Haack *et al.* 2005; Bramanti *et al.* 2009), with the disappearance of the hunter-gatherer population and their near complete replacement by incoming farmers from the Near East via the Balkans. However, further work has shown that this was too simplistic a view: for example, by focusing on sampling burials in formal cemeteries at Neolithic villages a random cross-section of the population was not acquired, and when cave burials in Germany were also examined (Bollongino *et al.* 2013) these produced quite different results. Individuals with the hunter-gatherer haplotype mtDNA buried in caves also seemed from isotope analyses to have had a rather different diet (based on fish) than farmers, showing that not only did this supposedly vanished population not disappear, but also that it maintained its traditional diet for some 2000 years after the introduction of farming into the area (Bollongino *et al.* 2013). Further investigations of 42 individuals from a tomb in central Germany dating to the same period confirmed this observation, as hunter-gatherer ancestry contributed roughly half of their aDNA (Immel *et al.* 2021). The simplest explanation is therefore that a significant part of the Early Neolithic population in Germany has thus far escaped sampling because of the focus on that part of the population buried in formal cemeteries.

Examining the end of the Neolithic in Britain, sometimes termed the Beaker period or Copper Age, *c.* 2450–2000 BC, we have a similar situation of a proposed population replacement, in this case with the arrival of people ultimately from the steppes. Based on a comparison of 51 individuals from earlier in the Neolithic with 37 from the Beaker period, the conclusion drawn by the team was stark: 'The arrival of people associated

with the Beaker complex precipitated a demographic transformation in Britain, exemplified by the presence of individuals with large amounts of steppe-related ancestry after 2450 BC' (Olalde *et al.* 2018, 194). Reflecting on this, Armit and Reich (2021) argued that the choice for prehistorians was clear: either there was a dramatic turnover of population in a short space of time *c.* 2450 BC, or there was more gradual movement of population into Britain from *c.* 2700 BC onwards. In either case, a transformation was assumed. Booth and colleagues then reassessed the detailed information contained in the data supplement to the Olalde paper (probably perused by only a tiny fraction of those who read the main paper), concluding that there was indeed a population shift, but that it had occurred over a far longer period, perhaps *c.* 2450–1600 BC, i.e. well into the Bronze Age (Booth *et al.* 2021). Looking at a much smaller scale, a comparison of 22 Bronze Age individual sequences on the Orkney Islands with the local Neolithic results demonstrated that most of the male lineages in the Bronze Age were derived from local Neolithic aDNA rather than those introduced in the Beaker period, while the reverse was the case for women (Dulias *et al.* 2022). We must therefore be extremely wary of over-confident pronouncements made on the basis of small or geographically uneven samples.

aDNA and Population Movements in Eastern Mediterranean and the Near East – Case Studies

Returning to the Eastern Mediterranean and the Near East, what can we say so far at both the local and the wider level? At Alalakh (modern Tell Atchana) in southernmost Turkey, near the Syrian border, a large number of DNA analyses were undertaken in two programmes (Skourtanioti *et al.* 2020, supplemented by Ingman *et al.* 2021), along with isotope analyses (the latter to examine the movement of individuals during their lifetime). This showed that the sampled Bronze Age population (39 individuals) had very little variability in genetic terms (both mitochondrial and nuclear), with the fairly substantial sample size making this a plausible conclusion. This may make it easier to identify population movements when further Iron Age remains are sampled, especially from this area of Turkey. However, despite the homogenous nature of the local gene pool, one extreme genetic outlier was identified among the sampled individuals: 'The aDNA analysis from [Late Bronze Age] Tell Atchana revealed that the sampled individuals are genetically very homogeneous – with the exception of ALA019 [the Well Lady]' (Ingman *et al.* 2021, 27).

This was an adult woman discovered thrown face down into a well, probably having met a violent death; this method of body disposal is a unique example from the site. Her ancestry can be traced back to groups in Central Asia (Turkmenistan, Uzbekistan or Afghanistan) or eastern Iran. While no other individuals have thus far been identified at Alalakh who share her genetic background, the complementary isotopic analysis indicated that she was probably born and grew up locally (Ingman *et al.* 2021, 33), implying that she was not the only person in the city with this distant ancestry. This case also acts as a reminder that isotopic analyses can only indicate whether an individual moved within their own lifetime, and not whether their forebears moved, as seems to be the case here.

A similar case comes from Egypt, where recent re-examination of a 25th Dynasty mummy, Takabuti of Thebes (Drosou *et al.* 2020; White *et al.* 2023), showed that she had probably been killed by an axe blow and that she had a mtDNA type for which the nearest match was from Bronze Age Bulgaria, with others in Bronze Age Germany and the Czech Republic. The Ancient mtDNA database (https://amtdb.org/) still shows no closer cases. Again this argues that there were people moving over very long distances in the Bronze and Iron Ages. Of course, it also shows that any attempt to simply pool the Egyptian ancient DNA results available so far from the Third Intermediate and Late Periods to produce a genetic overview would be utterly misleading, as it would currently suggest a significant population movement from Bulgaria at this time – something which would clearly be utterly implausible.

We can see an example of movement in the opposite direction from 6th–4th century BC Beirut, where the Iron Age III period genomes from 12 individuals were very similar, except for the burials of a woman and a man from the Saifi 1075 cemetery area (Haber *et al.* 2020). Their results clustered not with the other Beirut results, but with those from pre-Ptolemaic Egypt, leading to this conclusion: 'Thus, these results suggest that SFI-43 was an Egyptian woman and SFI-44 was her son from a man who himself had both Egyptian and Lebanese ancestries' (Haber *et al.* 2020, 151). Such long-distance genetic links clearly demonstrate the possibilities of precise synchronisms being produced by chance through the accumulation of data, as with the Viking Age case mentioned above.

aDNA and Population Movements in Eastern Mediterranean and the Near East – the Wider Picture

These anecdotal cases are certainly of interest but are so mainly because they are clear outliers rather than part of a general trend of population change. What they do demonstrate, however, is that there were individual cases of long-distance movement in the Bronze and Iron Ages, which need to be filtered out from broader comparisons between the dominant genomic

landscapes of the two periods in a given area. They also raise the question of what level of change might need to be detected to conclude that a significant population movement has indeed occurred. A recent large-scale study of the genomic history of the Mediterranean and Europe, generating over 300 new results for the period 1000 BC to 1000 AD (unfortunately only providing 13 new Iron Age results from two sites in the area considered here) concluded that 'at least 7–11% of historical individuals are ancestry outliers' (Antonio *et al.* 2024, 7). Thus 11% of their results were for individuals who did not match the general pattern for the area within which they were found. The 7% figure represents those for whom a likely source region could be identified. It is perhaps coincidental, but nonetheless intriguing, that the general survey of Mediterranean strontium isotope analyses (Leppard *et al.* 2020, 224) produced figures for non-local individuals of 9.38% (33/352) for the Middle Bronze Age to Early Iron Age and 8.58% (23/268) for the Late Iron Age. Antonio and colleagues also note (2024, 9) that their figure of 7–11% could be an underestimate, as this does not include clusters of individuals with a different ancestry from the majority. If these were separated out, then the outliers (individuals and groups) might rise to around 20% of the whole, at least for some of their regions – major population movements should perhaps only be identified with any confidence when this figure is exceeded.

Taking a broader view now, Haber and colleagues (2020) argued that at Beirut there was genetic continuity during the Iron Age and from there through into the Roman period, based on a total of 19 results, but that there was discontinuity from the Bronze Age. However, this interpretation was the conclusion of a comparison with Bronze Age results from Sidon rather than from Beirut itself. However, the results from Sidon are not a good match for other Bronze Age sites, except for Ashkelon, as noted by Agranat-Tamir and colleagues (2020, 1154):

> [The] Sidon ... population is genetically heterogeneous and has different individuals showing resemblance to different Southern Levantine groups. During the 2nd millennium BCE, Sidon was a major port city and was connected in trading relations with the eastern Mediterranean basin, which could have led to a significant genetic inflow, making its population more heterogeneous than that of inland cities. This might also be the reason that the site that most resembles Sidon is Ashkelon, which is another coastal site.

The argument that the coastal Levant may be rather different in terms of genomic variability to inland areas is entirely reasonable but clearly complicates matters in terms of population change. If attempting to identify a new population requires a baseline percentage of new genetic material to be identified, then a heterogeneous population renders this significantly harder to propose with any confidence, while defining the populations to be compared in geographical terms also becomes more difficult.

On this theme of variation within modern countries, the evidence from Greece and the Eastern Mediterranean islands comes thus far overwhelmingly from the Bronze Age (currently 169 individuals from 22 sites, 13 from mainland Greece, seven from Crete, one from Paros and one from Aegina), but may have implications for the Iron Age when sample numbers increase for this period (currently five from three different sites, all from mainland Greece). The Early Bronze Age genomes are homogeneous, according to Clemente and colleagues (2021, 5), confirmed by additional data (Lazaridis *et al.* 2022b) and derive most of their ancestry from Neolithic Aegeans, running against earlier theories that the Neolithic-Bronze Age cultural transition was due to massive population turnover. In contrast, the Middle Bronze Age individuals of northern Greece whom they analysed differed from Early Bronze Age populations in showing ~50% Pontic-Caspian Steppe-related ancestry. That this is not a chance finding is suggested by the presence of this ancestry in present-day Greek genomes at ~40%. This is intriguing, but more results were needed, in particular more from the later part of the Bronze Age. Two results from later Minoan Crete (Clemente *et al.* 2021), including one from Armenoi, were different from roughly contemporaneous individuals from northern Greece, and more like those from the Early Bronze Age, but there was a lack of information about the area in between. The significance of the results so far is that we need to be aware that the Aegean Bronze Age genomic pattern is not simple, and the interpretation of a representative Iron Age sample of results, when this appears, will need to take this into account. One specific case of this variation is a father and son pair found in a tomb at Pylos (Lazaridis *et al.* 2022a, 2022b), with a Y-chromosome in a rare haplogroup (only a handful of other ancient examples are known) with a match in the northern Caucasus (Wang *et al.* 2019).

Another potential complicating factor is the claimed Mycenaean takeover of Minoan Crete (e.g. Popham 1994; Skourtanioti *et al.* 2023). A major study of Late Minoan burials at Armenoi provided only the most limited scientific evidence in support of a Mycenaean subjugation of Crete (Richards *et al.* 2022): the analyses undertaken were sulphur and strontium isotope measurements on 62 burials, and mtDNA analysis of eight of these. The isotope levels of just four of the 62 individuals showed that they were not Cretan natives and they might therefore have migrated from the Greek mainland, although it was not possible to determine their origin. Also, the mtDNA results are described (Richards *et al.* 2022, 11) as being 'typical for this region and time period' and as lacking any outliers.

Against this, however, Foody (2021) undertook whole genome analyses of 23 individuals from Armenoi, with all producing mtDNA data and seven Y-chromosome results. The overall indications of this project were that 'the Armenoi population in this study appear most similar to the published Mycenaeans' (Foody 2021, 68). With regard to Foody's sample it is worth noting that these individuals come from a fairly small number of tombs, and several of them were closely related, with close kin being detected in three of the tombs. Further work was undertaken by Skourtanioti and colleagues (2023) on 34 Late Minoan skeletons (from Aposelemis, Chania and Krousonas), with the outcome being conclusions that a population change occurred during the Late Minoan period, that Late Minoan genomes could be divided into three groups, and that Chania demonstrated a far greater genomic variability throughout the period (although it was also much the largest sample). The overall conclusion is strikingly similar to that argued for Lebanon: that 'the genetic analyses demonstrate that Cretan populations at larger port cities biologically mixed with populations coming to the island during the course of a few centuries' (Skourtanioti *et al.* 2023, 297). This provides another reminder of the need to exercise significant caution with the interpretation of small sample sizes and of the value of larger targeted samples to cover a range of sites and take into account geographical factors in any interpretation.

At a more general level, aDNA results have shown connections between the Levant, North Africa and Sardinia from Phoenician and Punic sites (Matisoo-Smith *et al.* 2018; Moots *et al.* 2023), as we should expect, although one of the earliest results from Carthage (dating to the 6th century BC) was of a European mtDNA type, known otherwise from Mesolithic northwest Spain and modern-day Portugal and areas further north, so again an unexpected result (Matisoo-Smith *et al.* 2016). The Ancient mtDNA database (https://amtdb.org/) still shows no geographically closer cases.

Finally, we return to the Levant, and the site of Ashkelon, one of the better-known Iron Age sites in genomic terms, and to the topic of the Philistines and the Sea Peoples (Feldman *et al.* 2019). Sample from three Middle-Late Bronze Age, four Iron Age I and three Iron Age II burials were successful in yielding aDNA. Feldman and colleagues (2019) concluded from their analyses that a European genetic population arrived at the end of the Bronze Age or the beginning of the Iron Age, which they equate with the arrival of the Philistines. They also argue that this new genome did not last and was replaced by a more Levantine type. In their words (Feldman *et al.* 2019, 1): 'We find that the early Iron Age population was genetically distinct due to a European-related admixture. This genetic signal is no longer detectible in the later Iron Age population. Our results support that a migration event occurred during the Bronze to Iron Age transition in Ashkelon but did not leave a long-lasting genetic signature.'

However, a critical review of the stratigraphic data provided for the burials suggests that this change (assuming that the sample size is large enough to make such statements in the first place) may take place either earlier or later in the settlement sequence than the authors propose. The characterisation of this as a specific event rather than a more gradual process is thus over-confident. It may also be the case that their dating of this supposed event was influenced by the identification of an apparent destruction horizon during the early excavations at the site, something which is now seen as dubious (e.g. Millek 2018). It should be noted that within the main body of the text Feldman and colleagues do qualify their conclusions somewhat, by noting the need for more results from Ashkelon itself, a stronger evidence base from the Levantine Bronze Age more broadly, and more comparative samples, but the conclusion nonetheless stands. Unsurprisingly, the conclusions without the caveats were the elements of the work which received all the publicity.

Conclusions

The publications considered above have shown that aDNA can play a valuable role in assessing the accuracy of radiocarbon dating, as in the case of the Beaker culture burials of a man at Amesbury and of his daughter just 5kms away at Porton Down (Sedig *et al.* 2021), where this relationship revealed a significant discrepancy (of up to 140 yrs) regarding the gap in time between the two burials according to radiocarbon dating and that which is plausible for a parent-child pairing. The same paper (Sedig *et al.* 2021) also noted that such discrepancies arose in several other cases. Without aDNA these problems with specific radiocarbon dates would not have been noted. aDNA also offers the possibility of synchronising cultural phases in distant areas in a novel way through identifying relatives, avoiding the standard debates about the workings of cultural influences, e.g. on pottery styles. It is unquestionably the case that aDNA results will continue to be produced, and that these will be used in discussions concerning migrations in the Eastern Mediterranean and the Near East in the Bronze to Iron Age. It is therefore incumbent on archaeologists and ancient historians of the region, especially those working on chronology, to be aware of these publications, as information on relatives, for example, is often hidden in the supplementary information. Archaeologists and ancient historians also need to be aware of the sampling biases in the presently available data and the history of grand interpretations of population movements and replacements elsewhere in using aDNA publications.

Ancient DNA analysis is not the answer to everything but cannot be shunned as the devil's work. It will continue with or without the involvement of those taking a critical stance on chronological matters – even though much of the interpretation is at present overblown, there will be meaningful results in the future, so do not leave the interpretation of these to geneticists and a handful of archaeologists.

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